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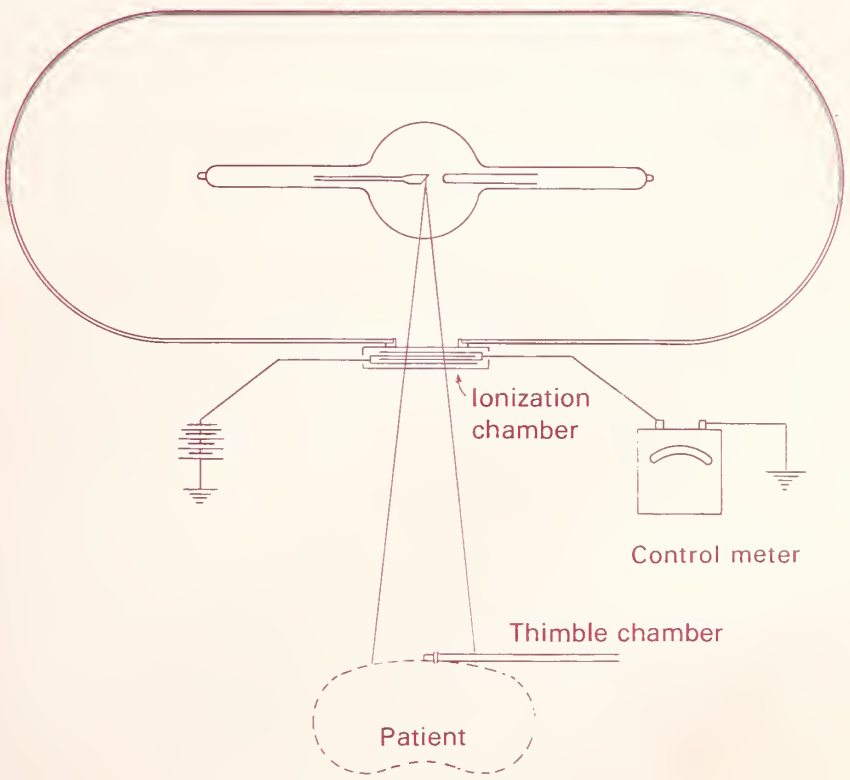
X-Ray Measurements and Protection 1913-1964



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X-Ray Measurements and Protection 1913-1964

The role of the National Bureau of Standards and the National Radiological Organizations

By:

Lauriston S. Taylor

Associate Director, Retired
National Bureau of Standards
Washington, DC 20234

Assisted by:
W. Reeves Tilley, Guest Worker
National Bureau of Standards

U.S. DEPARTMENT OF COMMERCE, Malcolm Baldrige, Secretary
NATIONAL BUREAU OF STANDARDS, Ernest Ambler, Director

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FOREWORD

In 1895, while experimenting with cathode rays, Wilhelm Roentgen discovered electromagnetic radiation of extremely short wavelength. This radiation he named the x ray, because so little was known about it. Although the name persists, a formidable body of knowledge now exists about the production, measurement, use and control of x rays. A great deal of this knowledge was generated by dedicated people at the National Bureau of Standards.

Bureau x-ray research began in earnest in 1927, when Lauriston Taylor joined the staff as chief of the x-ray group. For 35 years, he guided the Bureau's efforts in this area, a period during which a firm measurement base was constructed under his leadership. No one could better describe the activities of NBS in the area of x-ray measurements than Dr. Taylor, and I am delighted that he agreed to do so. His text, during this period, documents not only a record of scientific achievement, but of cooperative efforts between a Government laboratory and private organizations for the public benefit. We at NBS are proud of this record, and we are pleased to make this volume available.

E. Ambler
Director
National Bureau of Standards

AUTHOR'S PREFACE

"Radiation," in the context of this report, means ionizing radiation, that is, x, beta, gamma, and alpha rays from naturally occurring and artificially produced radioactive materials, as well as radiation of cosmic origin and neutrons. It excludes ultraviolet, visible and infrared, radio and microwave radiation.

In view of the current concern with ionizing radiation by nearly every branch of the Federal Government, it may be surprising to many to learn that the initial Government interest in ionizing radiation was focused for many years in the National Bureau of Standards. Moreover, this involvement reaches back 67 years whereas "atomic radiation," as it is known to most of the public today, was born only 37 years ago--just 30 years after the first NBS radiation programs were instigated.

The radiological profession is directly responsible for having urged NBS to establish its radiation programs in 1913 and to expand them dramatically in 1927. It was not until 25 years later that any other Federal agency concerned itself seriously with the radiation field, aside from clinical applications. Meanwhile, the close working relationship between NBS and the radiological community continued and intensified over a period of several decades.

The prime endeavor now is to lay out a record of the thinking and actions of the times, as they occurred, and ended, sometimes after false starts. Some of our early thinking may seem to some as very naive, and indeed it was. However, its recounting may be of value in showing how our measurement and protection philosophy was used in the applications of ionizing radiation in biology and medicine.

Since the research programs of NBS proceeded simultaneously with the radiological needs and activities of the outside organizations, it is sometimes difficult to untangle them for a more orderly presentation. To avoid this problem as much as possible, the different interests of each are treated in separate chapters interlaced so as to provide at least a small element of chronology and continuity. It is hoped that the reader will understand and bear with some sudden shifts in subject matter.

There will be numerous documented descriptions of our grasping for the most meaningful systems of quantities and units for depicting the radiation qualities significant to medical applications. Of necessity, this has involved a blending of the rigid philosophy of physics and the less scientific, more pragmatic needs and usages of medicine and biology. In a sense it would be difficult to find two more incongruent bedfellows but it has been done. Moreover, it is only through this laborious blending that today we know how to better utilize, and at the same time, better protect ourselves from ionizing radiation than from almost any of the multitudinous toxic agents by which mankind is assaulted.

Today radiation is faulted by some as a "deadly agent which cannot be seen or tasted or smelled or touched." Yet ionizing radiations can be detected and quantitatively measured--in a matter of seconds--at levels at least tens of thousands of times lower than the levels from which any biomedical effects have ever been found. The National Bureau of Standards and the radiological profession have played a critically important role in our ability to use radiation effectively and safely. This is the account of that role.

With few exceptions that will be easily recognized, the entire contents are based on published papers; summary reports describing programs; published and unpublished committee records and reports; and copies of correspondence--not always complete--from Government archives, from the files of the radiological societies, and from national and international committee files.

This is not exactly a history nor is it a technical review of the development of measurement standards and protection against x rays. It may be best described as an accounting of an era--an important one consisting of 5 decades. The story of x rays can be divided into three distinct periods--up to 1925, a period of discovery, application, and a recognized new danger; up to 1955, a period of exploitation, measurement, control, and protection; and from then, a time of consolidation, public awareness, and political activism. Each period builds upon the preceding one. My own background is not that of a trained historian, but of one who has taken part in the development of radiation measurement philosophy and techniques over the last two periods and especially the middle one. In the

middle period we saw the first organized efforts by medicine to understand, measure, and control what it came to recognize as a two-edged sword. On the one edge was the vision of an invaluable tool to aid in the quantitative analysis, diagnosis, and treatment of disease--especially cancer. On the other edge was the early recognition that the same radiation, if used to some undefined excess, could cause the same, or another form, of cancer that it could detect and, in some cases, cure.

It was at the beginning of that second period that the radiological professions throughout the world turned their concerns and energies to control and management of that important medical tool in their possession. This was almost an act of desperation, because they realized that if they could not control it, they might have to abandon its use altogether. Its potential danger to themselves and supporting radiation workers would otherwise be too great. In the more advanced countries, the radiologists turned to the national laboratories for help--in particular those of Germany, England, and the United States. More by chance than design, it became the lot of the author to organize the U.S. Government's first effort to develop an effective leadership, philosophy, and technology for the measurement of x rays as required by medical applications. The second challenge was to apply our developing knowledge to protecting ourselves from the ravages of excessive radiation exposure.

This book is an account of how Government and non-government organizations have worked together. It is a fairly good example of the patterns that apply to problem areas other than ionizing radiation, except that today there seems to be less mutual trust between the two; and, by the public, less trust in either. Attention will be directed to some of the changes and differences between Government laboratory operations under which the current generation chafes and those under which my generation thought they were chafing. Indeed the present generation of laboratory workers will note an almost unbelievable contrast between the research operations during the simple life of a half century ago and the more complicated ways of today.

The underlying motivation for this book as well as my recent one on Organization for Radiation Protection lies in the realization that I am very nearly the last of the x-ray research workers covering the period from the 1920s until about 1942, when atomic energy research reached full swing. In addition, I possess many of the records of the several key medically oriented organizations of that era in the field of ionizing radiation--the International Commission on Radiological Protection, International Commission on Radiation Units and Measurements, National Council on Radiation Protection and Measurements, and indirectly through them, of the National Bureau of Standards. How did this come about? The first three organizations are all non-government, yet they have had important working relationships with various governments. However, their files and records have generally paralleled and been outside of the Government records system. In spite of a diligent archival search by Mr. Walter Weinstein, Historical Information Specialist of NBS, only a few records of the Bureau's x-ray programs have been salvaged. Early correspondence records between the Bureau and the radiological organizations were found primarily in the records of the latter. Aside from formal publications of NBS, many of the records were maintained by the outside organizations, and these are now in possession of the author. Now, by way of making them more accessible to anyone who may follow me, I have felt the necessity of reducing them to more useful and manageable dimensions by condensing them into book size.

So that the reader may understand and be patient with my shortcomings as a historian, I must emphasize that I am really only a radiological physicist feeling a great debt to many people and organizations for the incomparable opportunities that have been open to me over more than 5 decades.

Lauriston S. Taylor
November 1980

ACKNOWLEDGMENTS

Important to the accuracy and authenticity of this book is its careful review by others having some first-hand knowledge of the work under discussion. However, by its very nature, this was not possible for the early period of the x-ray research at the National Bureau of Standards. Of the three known individuals still living who were associated with the early programs, each had short and relatively minor roles and no opportunity to develop an understanding of the overall programs. Dr. Harold Wyckoff joined the Bureau in 1941 and played a major role in the radiologically oriented research until his retirement in 1966. Through the literature he was also familiar with the Bureau's x-ray programs in the last half of the 30's. He has reviewed the entire manuscript in its final stages and has contributed many valuable comments for which the author is most appreciative. A review of the program after 1950 was provided by several staff members still at the Bureau or recently retired from Federal service. For this I am indebted to R. S. Caswell, M. Ehrlich, W. B. Mann, L.V. Spencer, L. Costrell, and R. Butenhoff, who covered the work of their sections as well as some details of other work during the period.

The Standardization Committee of the Radiological Society of North America played a critical role in the entire U.S. national radiation measurements program from 1926 to 1946. Because the Committee's published notes and proceedings were irregular and scattered, it was desirable to collect and republish them. To accomplish this I have received the kind permission of Dr. William R. Eyler, Editor of Radiology, to reprint related material from the journal.

Appreciated also is the permission by Dr. Edwin C. Ernst, Jr., to use a portion of his father's unpublished autobiography describing some of his early experiences as first Chairman of The Standardization Committee of The Radiological Society of North America.

I am especially indebted to Mr. W. R. Tilley, recently retired Chief of the Bureau's Technical Information and Publications Division, who initially edited and shepherded the manuscript through its drafting and publication process. Because of his contact with the radiation physics programs after 1946 and the close association with the author over more than 3 decades, he was in a position to make many useful technical as well as editorial suggestions to improve the clarity of the writing. And special thanks go to Mrs. Margaret Musick for her dedicated, careful typing of the manscript and preparation of the final camera-ready copy, under the expert guidance of Mrs. Miriam Oland.

L.S.T.
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X-RAY MEASUREMENTS AND PROTECTION, 1913-1964

The Role of the National Bureau of Standards and the National Radiological Organizations

CHAPTER 1. EARLY BACKGROUND (1912-1925)

In the development of the science and technology of ionizing radiations and their applications in biology, medicine, and industry, there were several turning points when an important series of discoveries or developments took place which greatly influenced trends for the next decade. One such turning point was the approximate period 1912-1913, marked by the sharp increase in the availability of radium and development of sophisticated and dependable gas x-ray tubes and the hot cathode x-ray tube.

Because of the increased uses of both x rays and radium, there was concern over the hazards to the user, particularly to the physician. Thus, it was necessary to develop much better measurement and descriptive procedures for all ionizing radiations to better deal with the hazards. This question had come to a head at an international congress in Brussels in 1910 and resulted in the preparation by Madame Marie Curie of a pure radium chloride standard, which was ultimately kept at the International Bureau of Weights and Measures at Sévres just outside of Paris. A concise description of this action and its relationship to national standardization laboratories is given in the report by E. E. Smith entitled "Radiation Science at the National Physical Laboratory, 1912 to 1955" (Smith, 1975). His description of that particular activity correlates well with what was going on in the United States and Germany at the same time. It is not entirely a coincidence that in December 1913, both the National Physical Laboratory (NPL) in England and the Bureau of Standards* in Washington started separate programs for the standardization of sealed radium chloride preparations. At that point, the International Standard in Sévres consisted of 21.9 mg of pure radium chloride sealed in a thin glass tube. The NPL standard was 16.08 mg, and the NBS standard was 20.28 mg (Smith, 1975).

Because of the injuries caused by excessive exposures to ionizing radiation, the Deutsche Roentgen-Gesellschaft, in 1913, put out its first set of recommendations for protection against the harmful effects of x rays (Taylor, 1979, 1-001).** A year or so later the British Roentgen Society formed a committee with the same general objectives in mind,

*When the Bureau was transferred to the Department of Commerce and Labor in 1903, the institution's name was changed from the National Bureau of Standards, as it was initially named in 1901, to the Bureau of Standards. By 1934 there were numerous other "Bureaus of Standards" in cities, States, and department stores. As a consequence, the name was changed back to the National Bureau of Standards to avoid confusion.

**The author recently published an extensive treatment of the development of radiation protection standards, entitled Organization for Radiation Protection, Sept. 1979. Frequent reference will be made to that book by date and page number, e.g., (Taylor, 1979, 1-001). Other literature references will be made to pertinent non-NBS publications (p. 334) by author and date, e.g., (Failla, 1929), and to NBS publications (p.338)) by reference number, e.g., (Ref. 155).

and its first recommendations were published in 1915 (Taylor, 1979, 2-001). For another discussion of these actions see "Radiation Protection Standards" (Taylor, 1971).

In other parts of the world, important research having no apparent relationship to ionizing radiation was taking place. This involved the production of electrons by a hot body, a phenomenon that Edison had observed in the early incandescent lamps and which was later known as the "Edison Effect." Subsequently, O. W. Richardson investigated the relationship between electron emission and temperature. There was considerable uncertainty at the time whether such emission would continue if the gas was completely removed from a hot body, such as a lamp filament (Miller, 1963, 87). It was at that juncture that W.D.

Coolidge, of the General Electric Company, became conscious of the fact that most of the limitations of the original type of x-ray tubes and their erratic behavior may have been due to their gas content. Yet, the tube could not operate without gas. At the same time, Dr. Irving Langmuir, at the General Electric Company, was studying electron emission from hot tungsten filaments while seeking a stable source of electrons in a high vacuum. He found that the emission was stable and reproducible, even in the highest vacuum obtainable at the time. One of the most important laboratory notebook entries was made on December 12, 1912, when W. D. Coolidge wrote,

> "IL (Irving Langmuir) tells me that in his study of the Edison Effect, current from hot cathode is greater with vacuum of 0.1 or 0.2 microns than at higher pressure (except in case of argon). I will try this at once in an x-ray tube in which I can heat the cathode." (Miller, 1963)

And thus was born the hot cathode x-ray tube, more commonly known as the Coolidge tube, an x-ray tube of great stability and enormous output and reliability.

Nearly 1 year later on December 27, 1913, Dr. Lewis Gregory Cole in New York became the first radiologist to have his office equipped with the new type of tube. To introduce the new tube and its inventor, Cole entertained at a dinner in a New York hotel, in the dining room of which was installed a powerful high-voltage generator. The generator was built by Dr. Harry Waite of the old firm of Waite and Bartlett, who was a pioneer and inventor in his own way. It was noted in a description of this affair that

> "Coolidge opened the machine up wide and, with a limited amount of protection which the open lead glass bowl of that time afforded, the audience must have received much more X-ray radiation than they were accustomed to." (Miller, 1963)

It was the use of this tube during the first World War that severely exacerbated the already worrisome problem of radiation injuries, leading to measurement and protection activities nearly a decade later which overshadowed everything that had gone on in the past (Coolidge, 1913, 1926).

With that background, we now turn briefly to the Bureau's ionizing radiation programs from 1913 until the early 1920's. The first program objective was to establish a facility for making comparisons between the radium standard and the radioactive preparations being sold on the open market for use by the medical profession. The need here was twofold. It was necessary that the doctors receive the quantity of radium that they were paying for (the price during that period was some $70,000 a gram), and that the doctors know the amount of radium in order to apply the proper exposure or dose to the tumor.

This measurement service, started at the end of 1913, continued at an increasing rate until after artificial radioactive materials became available in 1945. One point of interest is that the first measuring system for comparing the unknown against the standard preparation consisted essentially of a gold-leaf electroscope inside a brass-lined lead container some 4 inches on a side, with the electroscope also constituting the ionization chamber. That same system continued in active use into the 1950's. A similar system was also used by the National Physical Laboratory in Teddington, England, and the Physikalisch-Technische Bundesanstalt in Braunschweig, West Germany (which succeeded the original Physikalisch-Technische Reichsanstalt (PTR) in Berlin). At the time of this writing, the demand for such comparisons has disappeared for all practical purposes but, should such a comparison be called for again, the same gold-leaf electroscope is still in the NBS laboratory (see photo No. 50, ch. 16).

The Bureau's first radioactivity program was started in December 1913 under Dr. Noah E. Dorsey, one of the original Bureau staff members. Until then, Dorsey had been a staff member in the section on Inductance and Capacity--one of the six sections in the Electrical Division under Dr. Edward R. Rosa. At that time such a shift from electricity to ionizing radiation was not so unusual. If one were trained as a physicist, one should be capable of taking on any job in the physics field. It was not until the late 1930's that the specialization of physics research evolved. In any case, Dorsey set up the gold-leaf electroscope and proceeded with the necessary intercomparison of radium preparations. He also did research on the measurement techniques for very weak radioactive materials containing on the order of 10^{-6} to 10^{-8} grams of radium, and published the results in two papers (Dorsey, 1919A, 1922). Meanwhile the United States became involved in World War I and the Bureau was asked to study protective materials primarily for medical x-ray use

(Dorsey, 1919B). The first problem was to determine the effectiveness of lead glass used in fluoroscopes and protective screens. At that time, glass was purchased on specifications which merely required that it be "adequate" or "sufficient" for protection under unspecified conditions.

A system was set up for comparing the lead glass with sheet lead of various thicknesses and judging the lead equivalence of the glass by the blackening of a photographic plate. Some of the samples submitted were found to be plain window glass, but the better grades of material showed a lead equivalence in the range of 1/2 mm of lead. For the study, the Bureau acquired its first x-ray equipment in 1917, a Waite and Bartlett transformer and a high-voltage mechanical rectifier. Dorsey described the equipment as having a "voltage ranging from 3" to 9" spark gap." He was referring, presumably, to a so-called needle gap, in which case the range would have been in the order of 65- to 135-kV peak.

When reporting the results of this work at a meeting of the Western Roentgen Society in Chicago, the Bureau made its first public request for the assistance of the radiological societies to set up its x-ray programs. At that time (1918 or 1919), the Western Roentgen Society was just forming. In fact, the Society was the forerunner of the Radiological Society of North America.

Dr. Dorsey's career with radium measurements ended in 1919, as a result of severe finger and hand burns caused by the handling of radium in the course of his intercomparisons. He left the Bureau in 1920 for a period of several years.

However, Dorsey's assistant, Dr. W. S. Gorton, continued the radium work and extended the program to x-ray protective materials. In addition to the protective characteristics of glass, he studied the efficiency of lead rubber which was used for protective aprons and protective gloves. As in Dorsey's work, the comparisons were made using a lead step wedge and the blackening of photographic film. Gorton discovered that there could be misleading results with this procedure because the photographic plate was subjected to different amounts of scattering from the lead and the glass. He also suspected that there was a "luminescence" from the glass. In the course of his studies, he introduced the term "protection coefficient" for protective materials, a term which is still in use today (Gorton, 1918).

Available records indicate that following Gorton, Dr. Franklin L. Hunt joined the staff in the early 1920's. He also concentrated on studies of the protective characteristics of various materials, with special attention to protective plasters containing as much as 85 percent of barium sulfate (Hunt, 1925). Hunt had been one of William Duane's students and was well known in the x-ray field for establishing the so-called quantum cutoff in the x-ray spectrum, that is, the wavelength which was characteristic of the maximum potential applied to an x-ray tube. Hunt's studies covered the range of unfiltered x rays produced by 50 to 200-kV. He used the mechanically rectified high-voltage machine purchased in 1917 for measurements up to about 140-kV. For measurements in the higher range, he used the x-ray therapy apparatus that was then available at nearby Walter Reed Hospital.

A radiograph of barium sulfate plaster presents a considerably mottled appearance. Because it was not possible to obtain an absorption coefficient, Hunt was careful to express the value of the plaster in terms of "protection effect" or a protective coefficient. Hunt defined the protective coefficient as the ratio of the lead equivalent thicknesses of the samples. Values of the protective coefficients at that time were on the order of 5 to 13 percent, with a maximum at approximately 110-kV peak. Although there is no other published record of Hunt's work in this area, there is evidence of his interest in very long wavelength x-ray spectroscopy.

While these activities were going on at the Bureau of Standards at an obviously low level of effort, there was increasing activity in the field of medical radiology, and a growing concern about the adverse effects of the radiologists' exposure to large amounts of radiation. As mentioned above, the Coolidge tube had come into active use during the war years, especially in connection with military radiology. Even under the best present-day conditions, military radiology carries substantial risks to the operators. But the situation during the period from 1914 to 1918 was deplorable, and literally hundreds of doctors and technicians were severely injured or died as a result of their exposures (Brown, 1936).

Of course it had been known since 1896 that x rays could produce adverse biomedical effects and efforts to minimize these were made, but this was before the days of the powerful Coolidge tube, and under the pressure of military conditions, precautions were often neglected. By the end of the war, the problem was so severe that the medical profession was deeply concerned that it might have to forego the use of x rays unless better and more effective means for protection could be established. This situation ultimately led to extensive national and international efforts in the fields of radiation protection, an

in-depth accounting of which is contained in the book Organization for Radiation Protection (Taylor, 1979).

Prime leadership in the quest for x-ray protection in this country was that of Dr. George E. Pfahler of Philadelphia, a radiologist who in 1916 began urging that greater attention be given to radiation protection (Taylor, 1979, 2-008). Pfahler's efforts were heavily supported by the physicist J. S. Shearer from Cornell, through whose efforts the first radiation protection recommendations in this country were adopted by the American Roentgen Ray Society in 1922. Actually, the U.S. recommendations were very closely patterned after those of the British, which had been drafted before 1922 but not formally promoted by the British until a year or two later (Taylor, 1979, 2-008). During this period Pfahler put great emphasis on the protection of not only the physician, who was noticeably injured, but also on the protection of the patient on whom injuries were very rare and even more rarely serious. He was particularly concerned about the increasing use of the new Coolidge x-ray tubes.

CHAPTER 2. STATE OF THE ART (1925)

Another turning point occurred in the 1925 period. In Europe and the United States there was further growing concern over the lack of adequate x-ray measurement standards and about the protection of people against the harmful effects of ionizing radiation. Concern in the latter area included both gamma rays from radium and its daughter products, and x rays. The whole situation was heavily accented by the rapid expansion in the use of the Coolidge tube and the large number of radiologists suffering from radiation injuries.

Until about 1920, radiation was being measured by a variety of methods including barium-platinum cyanide pastiles, strips of photographic films, selenium cells, and chemical coloration. There were only purely empirical relationships between these different methods. In spite of the fact that, in 1908, Villard had proposed the radiation measurement system based on ionization in air that is in use today, this method had scarcely been reduced to laboratory practice before 1920 (First International Congress of Radiology, 1927).

Some half-dozen Germans, led by Professor W. Friedrich, a physicist who later was awarded a Nobel prize for his work in x-ray crystal-structure analysis, were especially active in this field. It is therefore not surprising that the first national laboratory to become involved in the standardization of x-ray measurements was the Physikalisch-Technische Reichsanstalt (PTR) in Berlin. The program there was directed by Dr. H. Behnken, with the assistance of Dr. Robert Jaeger. (There were to be many future contacts between them and the Bureau of Standards.) The system first used at the PTR by Behnken was a pressure ionization chamber operating at pressures up to about 6 atmospheres. The dimensions were small but adequate so that neither the direct x-ray beam nor the electrons it produced struck the measuring electrodes, a requirement under the proposed definition of the Roentgen (Behnken, 1924, 1927). However, shortly afterwards, Behnken realized that he had not allowed for columnar recombination of the ions within the ionization chamber and the readings were up to 15 percent too low.

The National Physical Laboratory in Teddington, England, had not yet installed an ionization chamber standard and was still doing its calibrations in terms of color pastiles. France had no national laboratory at that time and its standards work was in the hands of Dr. I. Solomon at l'Hôpital St. Antoine. His standard was a thimble ionization chamber leading through a sulphur-filled tubular shield to a gold-leaf electroscope. The calibration was in terms of the response of that particular chamber to a 20-mg source of radium enclosed in a thin-walled platinum ampule.

In the United States a standard had been proposed and put into use at Harvard University by William Duane. This was a free-air parallel plate ionization chamber but, due to inadequate design, readings were some 10 to 15 percent too low. A second, and greatly improved, parallel plate free-air ionization chamber had been constructed in the late 1920's by Dr. Otto Glasser, one of Friedrich's students then working at the Cleveland Clinic. (His chamber, according to later comparisons, provided readings that agreed within a small percentage with those of the Bureau of Standards.)

At this time the Bureau of Standards had no means or standard for measuring ionizing radiation other than a gold-leaf electroscope similar to the one used for the certification of radioactive preparations.

In the field of radiation protection the situation was somewhat better. Germany, England, the United States, Sweden, Norway, and several other countries all had radiation protection recommendations which were being adhered to by the radiological profession. There were many differences in detail but they were reasonably close together.

In 1925, A. Mutscheller, in the United States, proposed a "tolerance dose" which was considered to be acceptable for radiation workers. This was expressed in terms of the amount of radiation necessary to cause a threshold skin erythema in a specified period of time. However, there was great confusion and disagreement as to what the dose meant in terms of ionization measurements (Taylor, 1979, 3-009).

In the meantime, the world radiological community was recognizing the potential benefits of x rays in diagnosis and therapy, and was reacting strongly to the existing state of confusion. Under the impetus of the British Institute of Radiology, the first

International Congress of Radiology was held in London in July 1925. The initial scientific sessions of this Congress were devoted almost entirely to discussions of international units of measurement and standards for x-ray work. (First International Congress of Radiology, 1927; Taylor, 1979; Wintz, 1931.)

Encouraged by the International Congress in July 1925, the Standardization Committee of the Radiological Society of North America was established, but it held no formal meeting until March of the following year. The chairman of the committee and the driving force behind it was a radiologist, Dr. Edwin C. Ernst from St. Louis, who knew little about physics but, as a radiation therapist, knew a great deal about what was needed. He remained active in this field for several decades. Other members of the committee were Otto Glasser, a physicist and student of Nobel Laureate W. Friedrich; Wilhelm Stenstrom, a physicist, and former student of M. Siegbahn, a Nobel Laureate from Sweden; N. E. Dorsey and F. L. Hunt, physicists from the Bureau of Standards; William E. Chamberlain, a radiologist from San Francisco who was also an excellent physicist (his son later became a Nobel Laureate in physics); and, Arthur W. Erskine, a radiologist from Council Bluffs, Iowa, with no physics background but broadly experienced in radiology. He also suffered from severe radiation injuries.

Because of the importance of the March 1926 meeting of that committee, its report is reprinted in full on pages 7-13, with some of the discussions during the Annual Meeting of the RSNA in December 1926.

Following an informal meeting in the spring of 1926, the new Standardization Committee held its first formal meeting in December of that year at the same time of the Annual Meeting of the Radiological Society of North America. At this point, Dr. Hunt was expecting to continue with the x-ray standards program at the Bureau. After the meeting, the report on page 14 was published in Radiology (Radiology 10, 70, 1928).

PRELIMINARY REPORT OF THE COMMITTEE ON STANDARDIZATION OF X-RAY MEASUREMENTS

STANDARDIZATION COMMITTEE OF THE RADIOLOGICAL SOCIETY OF NORTH AMERICA

EDWIN C. ERNST, M.D., *Chairman*; OTTO GLASSER, Ph.D.; WILHELM STENSTROM, Ph.D.; N. E. DORSEY, Ph.D.; F. L. HUNT, Ph.D.; WILLIAM E. CHAMBERLAIN, M.D., and ARTHUR W. ERSKINE, M.D.

AT the Mid-annual Meeting of the Radiological Society of North America (1925), a month prior to the International Congress of Radiology held in London, a resolution was introduced and adopted for the appointment of a Standardization Committee. The personnel of this Committee was to include no less than three physicists and three radiologists, for the purpose of studying the problems in relation to the adoption of a standard X-ray unit.

HISTORICAL

It might likewise be of interest to state that at the First International Congress of Radiology held in London (July 1 to 4, 1925), the initial scientific session of this historical meeting was devoted almost entirely to a discussion of international units and standards for X-ray work. The delegates of the various countries at this International Congress of Radiology included Sir William Bragg, Dr. Béclère, Dr. I. Solomon, Dr. Altschul (Prague), Professor Wintz, Dr. Glasser and other members of the present Standardization Committee of the Radiological Society of North America. They critically discussed not only the urgency of adopting a physical unit of dosage, a standard known quantity of X-ray with a given quality of radiation energy common to the radiotherapeutists of all countries, but they also reviewed the many advantages and disadvantages of the various individual units and methods of measurement in use at many of the radiological centers and clinics throughout the world.

At the conclusion of these valuable discussions, Dr. Béclère proposed a resolution, seconded by Dr. Finzi, that an International Committee be appointed to consider the establishment of a *uniform X-ray standard of intensity and an X-ray unit.*

At the meeting of the Physics Section of this Congress on July 3, 1925, this resolution was placed before the Assembly by the Chair, supported by Dr. Béclère and Professor Friedrich. After its final adoption, it was further resolved that at an early date the various scientific bodies throughout the world shall be communicated with in order that an international representative membership might be reached.

In the meantime it was agreed that the following members should act as a nucleus to nominate the International Committee: Sir William Bragg, Professor F. H. Hopwood, Dr. E. A. Owen, Mr. C. E. S. Phillips, Professor A. W. Porter and Professor Sidney Russ. This initial group of eminent physicists and radiologists reasonably assures the probable final solution of one of the most perplexing and difficult problems of the radiotherapeutists throughout the world.

In all probability this International Committee on Standardization will not be in a position to make its final recommendations prior to the next International Congress of Radiology, to be held in Stockholm in 1928.

Your Committee, therefore, realizes the need and urgency of continuing the present organized effort in this country of establishing an X-ray unit without further delay, since the problems involved are most complex and will require painstaking study over a long period of time. The scientific consideration of the theoretical physical aspect of a standard X-ray unit is simple indeed, compared to the adoption of a method of measurement that is practical and applicable to routine X-ray therapy.

MEASUREMENT PROBLEMS

First of all, the various quantitative determinations of the output of X-ray machines were given careful consideration; under the different voltage conditions, and with the various minimum and maximum filtration factors. Each individual method of measurement has distinct advantages and disadvantages, as observed and established by means of the well known photographic changes of the Kienbock strips; the color changes of barium platino-cyanid; the calomel and iodochloroform chemical reactions; the determination of absorbed energy by means of heat measurements; the physiological effects of radiations on the skin; the effects upon animal carcinomatous or sarcomatous tissues, germinating beans, fruit flies, plant tumors, fish eggs, ascari eggs and others. As emphasized above, all of these methods have their individual characteristic advantages, but as a whole they lack uniformity and accuracy, especially when comparing X-ray radiations of different wave lengths.

The selenium cell conductivity changes, calibrated in Furstenau Intensimeter "F" units, is an extremely simple method for checking the constancy of the transformer output. However, from the standpoint of a standard unit of measurement, the fatigue changes which do occur in the selenium cells may cause inaccurate readings.

IONIZATION METHODS

On the other hand, the ionization method, as described by Villard in 1908, in which he defines the quantitometric unit as "*that quantity of radiation which produces one electrostatic unit per cubic centimeter of air under normal conditions of pressure and temperature*," has been most critically analyzed.

Because of its relative sound values, this same unit was taken up by Szilard in 1914, and further developed by Friedrich, Duane and Behnken. The latter three physicists improved the reliability of this unit by using larger air ionization chambers. By adopting this method they avoided the "wall" radiation, and thereby employed all of the electrons throughout their total ionizing path.

Dr. Behnken of the Physikalisch-technische Reichsanstalt, Berlin, has perhaps given us the most accurate and theoretical definition of the "e" unit, as described by him at the recent International Congress of Radiology. He changed the name of this "e" unit to the "Roentgen" unit (1 R).

The definition of this unit is as follows: "*The absolute unit of the roentgen-ray dose is obtained from that roentgen-ray energy, which, by fully utilizing the secondary electrons produced, and by avoiding secondary radiations from the wall of the ionization chamber, produces in one c.c. of atmospheric air of 18° C. (64.4 F.) and 760 mm. atmospheric pressure, such a degree of conductivity that the quantity of electricity measured by saturation current equals one electrostatic unit.*"

The German Bureau of Standards has taken further steps to bring into practical use the unit "R," on the basis of the definition given above.

Without going into detail as to the relative merits of the various methods of measurement, the Committee feels at this time that in all probability there are fundamental advantages in adopting the iontometric unit of X-ray measurement. The weak point of this method, as emphasized by Béclère, is that the present type of measuring apparatus will necessarily require further standardization.

In order to overcome some of these difficulties, Dr. Solomon in 1920 described an ionization unit which he called a "Roentgen" unit, and designated it by the letter "R," defining it as "*that amount of roentgen rays producing the same ionization as one gram of radium element at a distance of two centimeters from the graphite ionization chamber, in the same axis after filtration through 0.5 millimeter of platinum.*"

Fricke and Glasser, in 1924, defined the "R" unit as described by Szilard, Friedrich, Duane, and Behnken, by constructing

a small ionization chamber made of materials having the same effective atomic number as atmospheric air.

It is important to remember that commercial substances such as aluminum, horn, ivory, graphite, paper, etc., are some of the materials employed in the manufacture of the various measuring apparatuses. The individual values of these materials largely depend upon their purity and the differences of their effective atomic number from that of atmospheric air.

In 1923 Beets and Arens described an ionization chamber and an electroscope consisting of two circular parallel conducting plates. The filtered X-ray beam, in passing between these plates, traverses no substance other than air.

The comparison of the French with the German unit is as 2.25 is to 1. The (German) Behnken "R," therefore, is equal to 2.25 (French) Solomon "R" units, but this ratio changes with different wave lengths. All of the other present measurement units might be so converted, but unless an international unification is finally adopted, confusion will always be paramount to the simplification of our dosage problems.

SUMMARY

In attempting to solve these problems, the Committee is being guided by the individual observations of the members of this Committee, and the reported researches of other investigators in this country and abroad.

Primarily, a standard unit for the radiation dose must be defined; preferably an international one. The ionization in air as a means of determining radiation intensity has apparently proven the most satisfactory method thus far developed. It is therefore suggested that the electrostatic unit "e," as suggested by a number of investigators, Villard, Friedrich, and Duane, is the most practical standard unit of measurement. A most exact definition of this unit is given in the "R" unit of the German Roentgen Society.

It might be mentioned that the Committee realizes the fact that in connection with the quantity measurements of the X-ray output of any machine in standard units, it is equally essential to determine the quality of the radiation in terms of either the effective, the average wave length, the half value layer, or the coefficient of absorption. Charts for the deep intensity distribution of the human body should be very accurately determined. Standard methods and instruments must likewise be devised for the determination of the dose in the above mentioned unit. It must likewise be possible to measure and reproduce accurately such a standard unit, by employing an apparatus or simple device, practical in construction, to meet the demands of the average radiologist. In addition, the output of the machine must be constantly checked, either by a small ionization chamber in combination with an electroscope or by employing a large ionization chamber in combination with a robust galvanometer. Such an ionization chamber ought to be in a permanent position beneath the filter towards the patient, and the galvanometer or electroscope should be mounted so that it can be easily read by the operator.

Furthermore, it is essential that the different qualities employed must be taken into consideration, together with determinations of the biological effects produced in the above measured standard unit doses. Such measurements must be made upon the skin of the patient to include the back scattering. When employing fairly hard rays in the average deep therapy treatment, thirteen hundred "e" units have been observed to represent a dose which produces a skin reaction of the first degree. Radiation quality, however, has an important influence upon the number of "e" units necessary to produce the first skin reaction. For standardizing the output of the different apparatuses, it is advisable to determine the number of "e" units in air, *i.e.*, without back scattering, and to take the number of "e" units for the production of a skin dose

from tables worked out for the special conditions in use.

CONCLUSIONS

Therefore, the immediate problems under consideration by this Standardization Committee might be divided as follows:

(1) To study and establish a standard X-ray unit, physically defined.
(2) To determine the comparative variations of the X-ray dose measured in this unit for the different qualities of radiation energy.
(3) To devise ways and means of transferring such a unit of measurement from a standardization center or centers (preferably the United States Bureau of Standards) to different Roentgen institutions or private laboratories.
(4) To further study the proposed physical X-ray unit in relation to its equivalent biological effect or value.

The initial steps have been taken by your Committee to arrange for a conference with the officials of the Bureau of Standards at Washington, through the courtesy of its Director, Dr. Paul D. Foote, and the Director of the Department of X-ray Physics, Dr. Franklin L. Hunt, relative to the possibilities of future standardization researches. We have also been informed by Dr. Foote and Dr. Hunt that a new deep therapy X-ray equipment has been purchased for this special work. As soon as the necessary instruments are installed, the Bureau of Standards will welcome the cooperation of this Committee towards solving the problem of standardization by the establishment of a practical and uniform X-ray unit.

The fact that Dr. Hunt has accepted an appointment on this Committee further assures the success of our efforts towards a practical solution of this problem, provided, of course, our National Government in Washington will be able to co-operate with the Bureau of Standards in furnishing the necessary funds to permanently equip this department, so that neither the best interests nor the fullest development of the future scientific medical advances of the cancer problems may be thereby jeopardized.

DISCUSSION

Dr. Edwin C. Ernst (St. Louis): I might briefly add at this time that an X-ray unit which might be adopted should be carefully worked out so as to conform in every way with the probable recommendations of the International Standardization Committee unit of X-ray measurement to be adopted in 1928. We will endeavor to co-operate with the International Committee in every way possible. If the ionization unit is finally adopted and established in Washington at the Bureau of Standards, it will likewise be necessary to plan secondary standard instruments, portable in type, so that this unit can be transferred to other X-ray laboratories, either for scientific or therapeutic application. Such a standardization of an X-ray machine in standard units should preferably be done by physicists or representatives of the Bureau of Standards, who will take into consideration the quality as well as the quantity of X-ray radiation employed.

It is equally essential to be able to check the uniformity of the output of such installations by means of either an iontoquantimeter in a fixed position in relation to the tube, or by installing a robust galvanometer in circuit so that the operator may at all times either calculate or actually see the constancy of the output of the X-ray tube. Both types of checking instruments may be kept constant by a known quantity of radium. It must be remembered, however, that, in addition to the above electrostatic standardization unit or its equivalent, the sphere gap, voltage and milliampere readings should continue to be given the same consideration as in the past. It might be found desirable to have the Bureau of Standards recommend or actually employ

physicists for checking the output of the individual installations and perhaps issue a certificate to that effect to the radiologist in charge of the department. From a litigation point of view, that may be most valuable.

DR. A. W. ERSKINE (Cedar Rapids, Iowa): This is, of course, a preliminary report, but it is worthy, I believe, of some discussion and possibly some action. To summarize the report, the Committee has made only two recommendations: the first is that, for the present at least, some ionization method is probably to be preferred; and second, for the present, or until an international unit shall be adopted, the Committee feels like recommending the use of the electrostatic "e" unit in this Society, or the "R" unit, which is the same thing.

DR. LEO E. PARISEAU (Montreal): I would like to correct a slight misconception in the statement of Dr. Ernst. It lies more in what he said in his comments than in his written paper. He said that the one gram of radium was rather a large quantity necessary to standardize a Solomon iontoquantimeter. It is true that the "R" unit of Solomon is defined in functions of one gram of radium, but Dr. Solomon does not use at all such a large quantity to standardize his iontoquantimeter. As a matter of fact, in Liege two years ago, I was present at the convention of the French association, and Dr. Solomon showed us how he standardized his iontoquantimeter. He uses only ten or twenty milligrams of radium. He casts a block of lead with a hole in the side, and he shoves this over the ionization chamber and the radium is suspended at a certain fixed distance from the chamber, so that when he speaks of one gram of radium, it is merely by calculation that he has standardized it, and, as a matter of fact, he has used only twenty milligrams, and that is what he advised. It is a practice that most of us can employ in an ionization chamber. It means nothing as a true physical unit, but it is a very, very good check-up for us to use on the ionization chamber by employing a block of lead with the radium needle inserted at a fixed distance. You should then always obtain the same rate of fall of the index needle if everything is right in your chamber.

DR. H. J. ULLMANN (Santa Barbara, California): Was that the distance from the outer edge of the chamber or the center of the chamber, that two centimeter standardization?

DR. PARISEAU: Apparently it was from axis to axis in both directions, so as to obtain the most homogeneous radiation.

(The question was repeated by Dr. Ullmann.)

DR. PARISEAU: From the wall of the chamber in this case.

DR. A. MUTSCHELLER (Long Island City, N. Y.): As a measure for the quantitative destruction of radiation, I believe the Committee has recommended the use of the effective wave lengths. It seems to me that the average wave length would be much more useful,—the effective and average are not the same thing. The effective wave length is obtained by determining the transmission of rays through two different metals, and that differs with the filter thickness. If you determine with one filter, you obtain one value; if you put in another filter, you obtain another value, and with a third filter you obtain still another value; therefore the statement that the average wave length is of such a value does not convey anything definite. The average wave length is the same as the half value determined by Meyer and Glasser, which gives one value for a given radiation, and is comparatively easy to determine, and if it is to be determined accurately, it can be accurately obtained from an absorption curve in copper. An absorption curve in copper has the advantage that the several determinations are necessary, and they in turn check up the accuracy of the method. If the values,

when properly plotted, do not lie along a straight line, then there is an indication that an error has been made or that there is a leakage or something which is incorrect. In that respect, therefore, I think that the average wave length would be a much better descriptive term and in fact it is used to a much larger extent than the effective wave length, and I believe it would, in general, be much more useful.

Dr. R. A. Arens (Chicago): I rise to a point of order, or rather to ask a question; is this a permanent Committee, or is the Committee to be disbanded at this meeting, or can we take further action now or at a further meeting?

Dr. Erskine: Answering Dr. Arens' question: at the next Executive Session, the Chair will entertain a motion to continue or discharge the Committee and to approve its work.

Mr. Kegerreis (Chicago): I just want to say a word about two things of which Dr. Ernst spoke. I know both these men in the Bureau of Standards personally, and the type of work that the Bureau does is certainly such as to recommend it for a job like this. The other thing I would like to take out is the one word he put in when he said that perhaps the doctors should have an instrument. I want to put in the word "positively." What is the use of having a Bureau of Standards and all these things to check up, when things will change so tremendously? The only way to do this, if you are going to do it right, is to have an instrument there to tell you what you are getting all the time. There is no use in calibrating these things out in the third figure, meaning perhaps a thousandth, when you have, as is well known, variations during the treatment that will change things a great deal. You should be able to read this as you go along.

Dr. W. E. Chamberlain (San Francisco): There is one way we can help the Bureau of Standards. When I visited Dr. Hunt a year and a half ago, he showed me very quickly that he was fully aware of the fact that the U. S. Bureau of Standards is not living up to its full opportunity until it establishes those standards of X-radiation similar to the standards of the metric system, the meter, the year, the gallon, etc. He showed me very plainly that the whole question there at Washington is one of governmental economy, and that the Bureau of Standards can go on with the electric light, photometric measurements for the General Electric Company and for the U. S. public because they have been doing that for some years, but that the economy program of our President and the administration does not allow the institution of new ventures,—things which have not been done in the past few years. As a result of his plea to me and to every one of us who has visited the Bureau of Standards, some of us have let our senators and representatives know that we feel that the Bureau of Standards should not be handicapped by the fact that this is new work, that we should not be penalized and have the Bureau of Standards kept out of this important field merely because the Bureau of Standards did not start it ten years ago when we were a little more extravagant in our budgets at Washington. As a matter of fact in a faculty meeting at Stanford, I had a chance to talk to Secretary Hoover, who has the immediate supervision of the budget from the Bureau of Standards, and he said he would welcome as many letters as radiologists cared to throw at him suggesting that the budget be amplified by the amount of money which Dr. Hunt and Dr. Foote feel would be necessary to enable the Bureau of Standards to take hold of the situation and put there in that beautiful vault at the Bureau of Standards some ionization chambers and instruments which should be there as experimental exhibits, or permanent instruments to which we can go for a standard, if necessary, a hundred years from now.

Dr. Ernst (closing): I was glad to hear Dr. Chamberlain emphasize the individual problems of the Bureau of Stand-

ards. . . . This Committee has taken cognizance of these conditions and in order to help the officials in Washington, it might be necessary at a later date to send individual letters from representative radiologists and physicists to certain influential officials so that the necessary funds for this department may be obtained at an early date. I have been informed by Dr. Foote and Dr. Hunt that in the past they have been very much handicapped by the lack of funds, and that the future is even less promising. Therefore, under the present conditions, after completing their trip in Europe, the necessary appropriations for the Bureau of Standards available for continuing this work will be insufficient. In the meantime, however, this Committee expects to make every effort to help obtain the necessary funds for their department by urging the necessary appropriations through the regular channels in Washington.

Both Doctors are extremely interested in this whole matter and have arranged to attend this meeting for a conference with your Committee. Immediately after the first of the year it is the intention of the Standardization Committee to visit Washington for a combined conference at the Bureau of Standards. It is further planned to likewise co-operate with the committees of the other societies interested in this subject, together with the individual directors of all scientific institutions studying this therapy question, and thus, co-operatively, hope to get somewhere at the end of the next twelve months.

In reference to the subject of measuring the quality of X-rays, the Committee has been considering this matter from a practical standpoint. At this time I do not believe that we can discuss in detail the relative and the practical values of the average and the effective wave length methods of measuring the quality of X-ray radiations, but I might state that the half value method of measuring the quality of X-rays has appealed to some of the members of this Committee because of its simplicity. Perhaps it is not as scientific as it should be and therefore will be given further careful consideration. Individual groups of this Committee are planning to work on special problems in which they have been interested so as to expedite our progress.

In the meantime I wish to thank all of the members of this Standardization Committee and the many other radiologists and physicists for their helpful suggestions and sincere co-operation. We all realize the many complicated phases of this problem of standardizing the X-ray unit, and the relationship of such a unit of measurement to the many variable biological conditions. We will strive, however, to present for your consideration, a more definite or perhaps semi-final report of our efforts in behalf of humanity, at the next annual meeting of our Society.

EDITORIAL

THE PROGRESS OF STANDARDIZATION

At its summer session in 1925, the Radiological Society of North America authorized the appointment of a committee to study various phases of the problem of measuring X-rays, and to make recommendations for the guidance of the members of the Society. The President appointed E. C. Ernst, M.D., and Otto Glasser, Ph.D., as chairman and sub-chairman of the committee, and directed them to select such other practising roentgenologists and physicists to serve on the committee as seemed advisable. At the next meeting of the Society the committee made the following recommendations:

1. That measurements of X-rays be made by one of the ionization methods.

2. That the electrostatic unit be used to express intensity.

3. That quality be expressed as the effective wave length, the average wave length, the half value layer, or the coefficient of absorption.

The recommendations were adopted by the Society until such time as international units and standards shall be agreed upon. At the present time, therefore, members of the Society and contributors to Radiology should express the *intensity* of both the skin and depth doses in terms of electrostatic units. They should express *quality* in one of the four ways mentioned in the foregoing paragraph.

The members of the committee believed that the United States Bureau of Standards could aid most effectively in the solution of many of their problems. On visiting the Bureau of Standards they learned that a definite program looking toward the standardization of methods of measuring X-rays had been planned several years ago, but had not been carried out on account of the lack of available funds. However, at its last session Congress appropriated sufficient money for the necessary equipment and salaries, and work on the program is now being done.

During the Washington convention of the American Medical Association, the standardization committees of the Radiological Society of North America and the American Radium Society, and a representative of the American Physical Society, met informally with Dr. Franklin L. Hunt, who has had charge of the X-ray work at the Bureau of Standards. Dr. Hunt said that the electrostatic unit had been accepted by the United States Bureau of Standards as the official national unit of X-ray intensity. He and other officials of the Bureau have written a new definition of the unit which agrees with the *R*-unit as defined by Dr. Behnken, of the German Reichsanstalt, in every particular with the exception of a minor change in the method of correcting for variations in temperature.

Dr. Hunt and the director of the Bureau, Dr. George K. Burgess, outlined the contemplated activities of the Bureau as follows:

1. To design and construct instruments

for the measurement of X-ray intensity to be kept in the Bureau as a primary standard.

2. To construct less sensitive, more rugged portable instruments, as secondary standards, and to train men in their use in the field so that the output of X-rays from various apparatus throughout the country can be calibrated in terms of absolute units.

3. To calibrate measuring instruments in absolute units for manufacturers.

4. To test the efficiency of various types of apparatus and tubes used in producing X-rays.

5. To study methods of protection against stray radiation under actual working conditions.

6. To study the efficiency of various protective materials, such as lead rubber, lead glass, and barium plaster.

7. To develop the possibilities of the use of X-rays in the industries and arts.

With the Bureau of Standards lending its support, and with the help so generously being given by many physical laboratories throughout our country, it seems that the solution of the physical part of the standardization problem is in sight. The correlation of physical and biological effects will require prolonged study and innumerable data. To this end, every roentgen therapist who can do so should keep accurate records of the physical characteristics of each dose of X-rays administered to his patients, and supplement them by notes on the effects produced on both healthy and diseased tissues.

CHAPTER 3. PUBLIC PRESSURE FOR AN NBS X-RAY PROGRAM

NOTE: Much of the material in this chapter is based on correspondence files dating back to 1922 which strongly urged the Bureau of Standards to undertake an active program to develop suitable standards for radiation measurement, protective materials, and clinical radiation measurement techniques. These files came principally from the NBS Archives and from the records of Dr. Howard P. Doub, at one time Editor of Radiology and historian for the Radiological Society of North America. The files reveal that there were also demands for the Bureau to establish a laboratory for x-ray crystal structure analysis. A few of the many pleas are cited below. These are important because they came from well-established individuals and organizations, and greatly influenced the direction of the Bureau's programs in later years.

In November 1922, Dr. Vernon Kellogg, Permanent Secretary of the National Research Council, sent the Bureau the recommendation of its division of Chemistry and Chemical Technology urging the establishment of a central laboratory for x-ray diffraction analysis. This was accompanied by a detailed description of the kind of service desired. In Dr. S. W. Stratton's reply, he emphasized that the principal problem would be finding the right person to take up this work, but he agreed that the Bureau should be doing it. As events developed, some such plan appeared to have been considered by Dr. Franklin Hunt because, when he left the Bureau in 1927, he left behind a substantial assortment of partially completed x-ray diffraction cameras and related equipment. (The x-ray diffraction question came up again in the early 1930's at which time the Bureau actually employed, for a short period, one of the Nation's outstanding crystal structure experts, a Dr. Sterling B. Hendricks. The inauguration of such a program by the Bureau will be discussed later (see p. 176).)

In March 1924, Dr. Henry K. Pancoast, one of the outstanding radiologists of that period and Chairman of the Safety Committee of the American Roentgen Ray Society (ARRS) wrote to the Bureau. He asked,

> "if it would be possible for your department to develop some apparatus for the detection and measurement of secondary radiation in order that we may sometime in the near future be able to measure the amount of secondary radiation that may be harmful to individuals working with x-rays and especially in the fluoroscopic room?"

This was a very worrisome problem because the x-ray tubes were poorly shielded and the radiologists, who were working in the room with the tube and the patient, were subjected to serious levels of scattered radiation.

The reply to this letter explained that the Bureau did not have the facilities to really undertake the kind of studies requested by Pancoast. The situation was outlined in a letter by Dr. Paul D. Foote, Chief of the section in which Dr. Hunt worked. He stated in part,

> "We have just started an x-ray laboratory with one man, Dr. Hunt, and have almost no equipment for work of the character suggested.
>
> "We have some low voltage apparatus and enough equipment to work with soft x-rays. Dr. Hunt intends to measure the wave lengths of the characteristic radiation softer than 13 angstroms, using a spectrometer which the University of Pennsylvania has kindly loaned to us. This is about the only work in x-rays which we can do efficiently at the present time. Meanwhile, he is designing ionization chambers, spectrometers, and other apparatus suitable for such problems as you suggest, which will be constructed in our shops.
>
> "It is possible within another year the funds will be available which will enable us to purchase a 250,000 volt apparatus complete, and secure an assistant for Dr. Hunt in this work.

"It is our hope eventually to cooperate in every way with the American Roentgen Ray Society confining our work, of course, to the purely physical problems, which may be suggested--exactly of the type described in your letter. If we are able to secure a modern installation for hard x-rays and sufficient personnel, I feel certain that we may be of considerable assistance to your committee."

Included with the March 1924 letter from Pancoast was a copy of the paper by G. E. Pfahler entitled "Protection in Radiology" (Pfahler, 1922). The recommendations of the Roentgen Ray Society were outlined by Dr. Pfahler (Taylor, 1979, 2-008).

A letter in April 1924 from the South Carolina Baptist Hospital outlined another serious difficulty which the staff was encountering in their deep therapy work in the 200-kV region. They were finding, as a result of reactions of patients' skin to the therapy, that their tube outputs were varying by as much as 40 percent because they had no control mechanism for determining when the equipment was functioning normally and uniformly.

In May 1924, a letter from Professor William Duane of Harvard University also called attention to the problem of scattered radiation in x-ray rooms and suggested that the Bureau

"design a portable electroscope that could be easily calibrated in terms of x-ray intensity units. This electroscope could be placed in positions that had been occupied by x-ray operators for a number of years and thus an estimate of the intensity of the scattered radiation that is <u>not</u> dangerous could be determined."

Such an instrument, one of the first of its kind, was subsequently developed by Taylor in 1929 and was used routinely for checking the radiation installations at the Bureau for the next decade (Taylor, 1967).

In April a request came from the Picatinny Arsenal in New Jersey emphasizing the critical need for analysis of the lattice structure of crystals and the determination of "the actual configuration of the atoms in the molecules of the various nitro-aromatics." In another letter from Pancoast in May, he emphasized the need for information on scattered radiation and for the study of the properties of protective materials which were substituted for lead. These would include such things as barium plaster, lead glass, and lead rubber. He pointed out that the only means for detecting stray radiation was with dental films placed in the areas of concern. Though dental films had been used for such purposes, they were clearly recognized as being only roughly quantitative in nature. In responding to this letter, Dr. Burgess, Director of the Bureau, again emphasized the lack of adequate personnel and equipment, but indicated that an appropriation for the purpose might soon be made. Meanwhile, he said that the Bureau would start both of the investigations requested by Pancoast and would carry them as far as facilities permitted. It was becoming pretty clear at this point that the pleas of the radiologists were taking effect and that the American Roentgen Ray Society was spearheading the drive for help in the area of radiation protection.

In a letter of May 1924, Dr. Wilhelm Stenstrom, a physicist at the State Institute for the Study for Malignant Disease, in Buffalo, added emphasis to the pleas by Pancoast. His letter indicated some of the trends of the time when he said,

"The crest voltage used for deep therapy lies usually between 180 and 200 kilovolts. Some physicians use the old machines at 140 kV. While it is probable that some will soon go up to 250 kV, which is the maximum voltage Coolidge gives for the water-cooled tube, tube current used per tube lies between 4 and 8 milliamperes for the ordinary tube (air cooled) and between 25 and 50 milliamperes for the new water-cooled tube. In order to carry out standardization of the kind mentioned above (this related primarily to scattered radiation) it seems to be necessary to use a voltage up to 250 kV peak. A direct current machine is the best one for pure physical measurements (200 kV with this machine ought to give about the same effective wavelength as 250 kV crest). However, I doubt that such a machine can be bought which will give more than 10 ma at 200 kV. Transformer and mechanical rectifier is the equipment commonly used in hospitals. For fluoroscopic work the voltage is kept below 70,000 volts rms and the current is, as a rule, below 5 ma."

Stenstrom was correct in his recommendation for constant potential equipment. He was also correct in that it could not be purchased. In fact, when the Bureau finally did install one in the late 1920's, it had to be designed and built from scratch.

Dr. George E. Pfahler had, for the preceding 10 years, been advocating more attention to problems of x-ray protection. In a letter of May 1924, he expressed concern for the protection of the general public, as well as for radiologists and their associates, because the new high-powered tubes could develop stray radiations in adjacent laboratories and rooms occupied by people.

On May 28, 1924, Dr. Hugo Fricke, a physicist at the Cleveland Clinic, called attention to the fact that the German Radiological Society had recently established a program for the standardization of x-ray dose, and that there were four laboratories, including the Physikalisch-Technische Reichsanstalt, that would participate. He also outlined three problems as follows:

> "Problem 1. The standardization of the measurement of dose demands (1) the establishment of a unit dose; (2) the standardization of a method by means of which this unit can be obtained; and (3) a plan whereby x-ray apparatus in the different medical institutions may be calibrated according to this method."

He then went on to briefly describe a free-air type of standard and a thimble ionization chamber of a type suitable for working in the field.

> "Problem 2. Quite a few investigations had been made regarding the problem of the penetration of the radiation into the body. In this case, also, because of the great differences among the results of the different investigators a standardization of the data is very much needed."

In this Fricke was referring to the standardization of depth dose tables, a problem which was being worked on in various parts of the world and was still considered something of a problem as late as the 1970's.

> "Problem 3. As for the question of proper protection, the radiation which the man who gives the treatment receives is partly that which reaches him directly through the protective screening between him and the x-ray tube, and partly (usually principally) that which reaches him directly from the scattering of the direct radiation."

He then went on to describe some of the Cleveland Clinic's procedures in this field.*

> *It was Fricke who developed the first clinical ionization chamber and electrometer unit manufactured in this country, then known as the Fricke-Glasser Dosimeter (see photo No. 1). The device had a thimble chamber made of graphite and used a string electrometer which was quite stable in holding its calibration. This soon led to the development of the condenser R-meter by Victoreen in about 1930, which for many years was the standard clinical dosimeter in this country (see photo No. 2).

On May 27, 1924, Dr. C. M. Jackson, Chairman of the Division of Medical Sciences of the National Research Council, joined the ranks of those urging studies leading to the control of stray radiation. Then on May 29th, W. D. Coolidge, inventor of the Coolidge tube, wrote to Foote pleading for early Bureau attention to the radiation protection problem brought about by his powerful tubes. In the letter he stated,

> "The results of undue exposures to the x rays are so grievous that it seems hard to overestimate the importance of this matter of x-ray protection. The subject is unquestionably one that should be under Federal control and the obvious and logical agent is the Bureau of Standards."

However, no one realized that the Bureau of Standards was never a regulatory agency. Dr. Burgess, in his reply, agreed with the concept that radiation protection should be under Federal supervision, but it was to be some 3 decades before this would, in fact, come about. (The first controls were introduced by the Atomic Energy Commission in the mid-1950's, based on the recommendations of the National Council for Radiation Protection, followed in 1959 by the government-wide regulations of The Federal Radiation Council (Taylor, 1979, 8-226).

In mid-June 1924, a detailed communication was received from Dr. N. E. Dorsey, who had been the first person to work with ionizing radiation at the Bureau and who had suffered some degree of radiation injury. Since leaving the Bureau in 1920, he had been engaged in

the preparation of parts of the International Critical Tables. He outlined 12 areas of activity in the x-ray field for the Bureau as follows:

"(1) Study and certify protective materials and devices; (2) develop a means for specifying the quality and intensity of radiation emitted by an x-ray tube; (3) establish the relationship between the radiation quality and the type of machine exciting the radiation; (4) study of ancillary instruments such as spark gaps, voltmeters, timers, etc.; (5) establish a means for measuring the quality and intensity of stray radiation; (6) study the amount of radiation scattered laterally by a patient; (7) identify and prescribe means for avoiding errors in installations; (8) test and certify filters; (9) study screen factors, graininess and other properties of intensifying screens; (10) study operating characteristics of x-ray tubes and their idiosyncrasies; (11) study the correlation between the quality and quantity of radiation and the several biological effects produced; (12) establish a core of field workers to inspect and test x-ray equipment."

Note that Dorsey's proposal was very comprehensive and beyond the general recommendations made up until then. (Looking ahead, it is interesting to note that practically all of Dorsey's recommendations were followed and most of them carried through to fruition. However, this was despite the fact that the correspondence did not come to Taylor's attention until after he had retired from the Bureau of Standards.)

The mechanics of handling Bureau correspondence at this time is worth noting. In 1924 a letter addressed to, say, Dr. Franklin Hunt, would be routed first to the Director, then to Hunt through the Division and Section Offices for possible comment. Hunt's reply would have to be initialed by his Section Chief, Dr. Paul D. Foote, in turn by the Division Chief, Dr. Clarence A. Skinner, and then routed to the Bureau Director, Dr. G. K. Burgess, for his signature and mailing. Moreover, many of the letters were addressed to the Secretary of Commerce, Dr. Herbert Hoover, and were routed down the line and then back to his desk for his own personal signature. Amazingly enough, this could be accomplished routinely in about 10 days, and faster, if necessary.

On June 18, 1924, the following memorandum from Secretary Hoover to Dr. Burgess was sent down the line:

(Hoover to Burgess)

Requests that the Bureau undertake the work in x-ray investigations, as outlined, have been very numerous and urgent ever since this section was organized.

In personal conversation with me, dozens of prominent scientists and roentgenologists such as Dr. W. R. Whitney, Director of the General Electric Research Laboratory, Dr. S. W. Stratton, President, Mass. Inst. Tech., Prof. Bergen Davis, Prof. Arthur Compton, Prof. Anthony Zeleny, Prof. W. F. G. Swann, Prof. A. F. Kovarik, Dr. Wheeler P. Davey, etc., have urged that this Bureau undertake such work.

Were we to ask for an expression of opinion from the American Medical Association, we would be literally swamped with letters.

I append a few of the more specific letters received lately, together with one or two older letters which I happen to recall. Prior to two months ago no record was kept of such correspondence.

These letters represent the opinion of some of the most influential manufacturers and roentgenologists in the country: for example, the General Electric Co., and the Victor X-Ray Corporation, the largest producers of x-ray apparatus in the world; the American Roentgen Ray Society representing all the roentgenologists of the country; Prof. William Duane, a leading biophysicist and consulting physicist for numerous hospitals; the Cleveland Clinic, the Buffalo State Institute, the American College of Radiology, the Harvard Biophysical Laboratory, the National Research Council, and other institutions of the highest standing.

I believe that it is our duty to give attention to these urgent requests.

Dr. Burgess got the message. About a month later, he had another letter from Dr. Pancoast, again urging action on the part of the Bureau. In his reply he said,

(Burgess to Pancoast)

"Your letter has confirmed our opinion that a satisfactory investigation of the problems under consideration cannot be made without the installation of higher voltage apparatus than the Bureau of Standards now has available.

"Pending the provision of this equipment, we are assembling as much of the auxiliary apparatus as can be prepared so that there will be a minimum delay when the higher voltage generating plant becomes available."

Working plans were in fact prepared, estimates made, and such equipment as could be ordered was obtained from general Bureau funds, rather than awaiting a special appropriation.

By the spring of 1925, a second campaign on the part of radiologists began to build up, due to the oncoming First International Congress of Radiology mentioned earlier. In preparation for this, the Radiological Society of North America (RSNA) organized a Standardization Committee whose initial objectives were primarily those concerning the dosimetry of therapeutic x rays. Whereas the drive for Bureau activity in the field of protection had been largely spearheaded by the American Roentgen Ray Society, this new effort on dosimetry was to be spearheaded by the RSNA under the leadership of Dr. Edwin C. Ernst. On November 9, 1925, Ernst notified Dr. Foote that he (Dr. Ernst) had been appointed Chairman of the Standardization Committee and that he was looking forward to collaboration with the Bureau of Standards. In his letter he stated that he understood that the Bureau was installing a high-voltage machine which would enhance the cooperation between the Bureau and the RSNA Committee. Apparently, this equipment had been purchased the preceding year following Hoover's memo urging the Bureau to act.

On January 6, 1926, Dr. Ernst sent a detailed letter to Secretary Hoover outlining the plans of the RSNA and its desire to work with the Bureau of Standards. This letter, accompanied by the Preliminary Report of the Committee on Standardization of X-Ray Measurements (p. 7), follows:

(Ernst to Hoover)

"I wish to take this opportunity of briefly acquainting you with a few of the most important facts relative to the X-Ray dosage measurements, with special reference to the present essential needs of the Standardization of the unit of X-Ray, and the logical position and relationship the Bureau of Standards should have to this problem of standardization in the United States:

"(a) In May, 1925, I presented the enclosed scientific contribution to the members of the Radiological Society of North America at their Mid-Annual Session in Atlantic City. In this communication I emphasized the fact that our present methods of measuring the X-Ray doses were most inaccurate, and that they were almost entirely due to the absence of an X-Ray unit as a standard. All of these facts were then substantiated in the discussion that followed by radio-therapists and physicists from all parts of the United States.

"(b) The Society realized the present haphazard methods of designating intensive X-Ray treatment doses, and a Committee was immediately appointed by the Radiological Society of North America, and instructed to make an extensive study of this problem, and present a report to the members of the Society at their next annual meeting.

"(c) As Chairman of this Standardization Committee, I presented a preliminary report to the members of this Society at Cleveland, early in December of last year, and this report was not only accepted, but a resolution was presented and passed by the members of this Society to the effect that not only shall every effort be made to expedite the designation and adoption of such an X-Ray unit, but that it is equally essential to have this unit standardized as such by the Bureau of Standards in cooperation with this Standardization Committee and those scientists interested in radiation therapy throughout the United States. And furthermore, that a copy of this original resolution shall be sent to the Secretary of the Department of Commerce at an early date.

"(d) In the meantime this Standardization Committee has communicated with all of the Physical and Radiological Societies in the United States who might be interested in this

problem, so that the most efficient cooperation can be given to the Department and the Bureau of Standards.

"(e) Furthermore, this Committee shall be pleased to cooperate with the Department of Commerce in informing the representatives in Congress of the need of the necessary immediate and the future appropriations and thus materially aid the cancer control problems.

"(f) The members of the Radiological Society sincerely hope that the Department of Commerce will be in a position to immediately give the X-Ray Department of the Bureau of Standards the necessary cooperation to begin an early study of these problems.

"(g) This Standardization Committee of the Radiological Society of North America, will pledge itself to cooperate with the Bureau of Standards, even to the extent, collectively and individually of doing a great deal of research work in the various scientific laboratories throughout the United States, which information will be presented to this Department from time to time.

"(h) On January 11th, this Committee will meet in Washington, at 10:00 A.M., in the office of Mr. Hunt of the Bureau of Standards for the purpose of reviewing the important phases of the standardization problems; to advise the Bureau of Standards as to the present plans of this Committee towards standardizing an X-Ray unit; and to divide the work of the members of the Standardization Committee so that collectively their researches might be continued in the individual laboratories and universities to help expedite the final solution of our dosage problems. This Committee received resolutions, adopted by the Medical Staffs of some of our largest scientific and medical institutions, informing them that they heartily supported and welcomed this movement towards the standardization of an X-Ray unit, and sincerely hope that it will be possible to accomplish this fact at a very early date because of its close relationship and intimacy to cancer etiology and treatment.

"As Chairman of this Standardization Committee of the Radiological Society of North America, I shall be pleased to give you further information upon request or during my stay in Washington, on January 11th, 1926, will welcome the opportunity of giving you or your assistants a personal interview.

"This committee has been informed that the Bureau of Standards at present has insufficient funds to meet the needs of their department, and since this is truly an emergency measure, in the interest of Medical Science, procrastination, at this time, would be most unfortunate to the many important cancer studies nearing completion in many of our research institutions, and most essentially a serious handicap to the practical routine administration of X-Ray treatments in cancer and other diseases.

"In the interest of continued advancement of Radiological Science and Therapy, realizing that the adoption of such a standard unit would be, not unlike the epoch making heat and light standard unit factors, I sincerely trust that you will be in a position to help us solve the present problems by standardizing the X-Ray unit of measurement."

On January 9, 1926, Dr. Ray Lyman Wilbur, President of Stanford University, wrote to Mr. Hoover forwarding a suggestion by Dr. W. Edward Chamberlain of Stanford University Medical School. In his letter he stated,

"The roentgen ray is being used more and more in the treatment of many human ailments, particularly for malignant diseases. The human body is apparently capable of resisting a certain definite quantity of x-ray exposure, but is liable to definite damage (i.e. x-ray burns, etc.), when the point of saturation is reached. It seems likely that everyone that has had x-ray exposures should have a record of what those x-ray exposures were, if he is to be safely exposed at a later date."

This was an interesting statement in view of the fact that individual dose recording has been proposed several times by various groups or individuals over the past 5 decades. The principle is sound, but even today when we can measure and describe radiation dose with substantial accuracy, we still do not know how to properly and usefully add doses to

different parts of the body at different times so as to arrive at a significant meaning for "total dose." This would be particularly so in the case of diagnostic irradiation. It would be much more meaningful in the case of very large doses of therapeutic radiation which, if repeated, are most likely to involve the same parts of the body.

During the month of January 1926, numerous letters came to the Bureau urging one kind of attack or another on the problem of x-ray measurement standards. These included communications from such individuals as Professor David L. Webster from the Stanford University Physics Department, one of the five or six leading x-ray physicists in the country; Dr. W. E. Chamberlain from the Stanford University School of Medicine; and F. K. Richtmyer from the Cornell University Physics Department, also one of the top x-ray physicists. At about this same time, Ernst had the meeting he had requested earlier with Secretary Hoover to discuss funding for the Bureau's x-ray programs. In the course of the meeting he was referred to General Lord, Director of the Bureau of the Budget. In a letter of January 22, 1926, Ernst made reference to his meeting with Lord from which he emerged with some encouragement for success. His letter included the RSNA Standardization Committee's recommended program for the Bureau of Standards, and estimated funding on the order of $60-$70,000 for the first year. The purpose of the funding was described as follows:

RECOMMENDATIONS MADE TO THE BUREAU OF STANDARDS BY THE STANDARDIZATION COMMITTEE OF THE RADIOLOGICAL SOCIETY OF NORTH AMERICA:

"Briefly, the Bureau of Standards, in the estimation of the United States Standardization Committee, at their conference on January 11th, 1926, should supplement its present very meagre X-ray apparatuses, scientific instruments, personnel, towards establishing a standard (electrostatic) X-ray unit.

"(a) One standard high voltage X-ray machine.
"(b) Secondary experimental X-ray transformer.
"(c) Instruments for transporting known wave length calibration of X-ray radiations in the above electrostatic standard unit.
"(d) Quantity and quality laboratory precision X-ray measuring instruments, such as electroscopes, iontoquantimeters, galvanometers, spectroscopes, etc.
"(e) Develop and organize a personnel for the routine calibration of secondary portable measuring instruments in standard X-ray units in addition to the present departmental directors.

"Such a standardization is not practically possible unless supervised and established by a national bureau of standards.

"Efforts should be made to begin this work at the earliest possible date.

"In the estimation of this Committee, the amount of the appropriation for equipment and personnel should not be more than $70,000 nor less than $60,000 for the coming year."

As mentioned earlier, Dr. Ernst, though not strong in physics, certainly had drive, initiative, persistence, and a willingness to approach anyone once he was convinced that he was doing something worthwhile. An example was his approach to General Lord, who was noted for being a hard-hitting bureaucrat. Years later, Dr. Ernst started to write an autobiography, chapter 9 of which dealt with the Standardization Committee activities in the RSNA. A draft of this chapter is excerpted below by permission of Dr. E. C. Ernst, Jr.

I was appointed by our committee to follow through with a three-pronged rather than a two-pronged program, including obtaining the necessary funds for the National Bureau of Standards through Congress and political senators, if the higher officials of the government are in full agreement with our objectives.

Dr. Burgess, the director, promised to immediately give me a preliminary outline as to the available facilities, future scientific instrument requirements and needed personnel, but the total cost for this venture was difficult to estimate at this time.

However, I was advised that politically it is advisable to accept even a modest sum when faced with an initial attempt to obtain funds from our government. At a later date additional finances can more easily be obtained from the Budget Bureau and Congress.

I was then authorized by our Committee to explore the situation for obtaining both political and otherwise official views relating to our scientific proposal before returning home, which I did.

My Congressional friends suggested that I should initially contact the United States Secretary of Commerce, Mr. Herbert Hoover, and follow his advice. They also promised to contact him personally the following day. Fortunately, I was able to obtain an appointment with Herbert Hoover, then Secretary of Commerce, later, President of the United States, the following morning. However, I had initially sought the advice of our Missouri State Senator Williams as to how I might approach the Secretary of Commerce.

Secretary Hoover appeared most cordial and expressed unusual interest in our program and seemingly highly favored our effort to cooperate with the Bureau of Standards, especially in the field of scientific unit standardization.

I also identified the member scientists of my committee and their scientific backgrounds who had made this special trip to Washington in the interest of scientific programs and future advances in the field of radiation therapy for cancer.

Mr. Hoover, after a brief interview, then advised me to obtain an appointment with General Lord, the Director of the Budget of the United States, and had his secretary contact the General's office. I later realized that this proved to be helpful, phoning from Mr. Hoover's office.

Nevertheless when I spoke to General Lord's secretary the following morning and briefly outlined my mission in the interest of the public, and request for an appointment with General Lord, I was rather coolly informed that his highness was extremely busy but perhaps she could find a vacancy for a brief interview in his schedule the following week.

I then also described the scientific achievements of the individual members of my committee and their professional University backgrounds, coming here to meet with officials of the Bureau of Standards on an unselfish mission solely in the interest of our advancing science and the welfare of the public at large.

Furthermore, I personally could not remain in Washington for another week, since I had no associates to take care of my radiological practice, and, after all, my single mission here was in the interest of advancing radiation science toward cancer control measures which involve the interest of the public.

After all, Mr. Hoover, who suggested that I contact General Lord, was sufficiently interested in our proposed program, and it was he who advised me to obtain this interview.

I am at present in Mr. Hoover's office and perhaps General Lord could arrange for an earlier appointment, preferably tomorrow morning. May I await your reply and please assure General Lord that my interview will be brief.

A return call was received by Mr. Hoover's secretary that I could have an interview the following morning at three minutes before nine, adding that all of General Lord's subsequent appointments began sharply at nine.

When I arrived bright and early at the office of the Bureau of the Budget after spending most of the early morning attempting to brief my story within the allowed three minute limit, General Lord's first secretary met me with the exclamation: "I believe you are Dr. Ernst?" Of course I replied in the affirmative, with a smile, but she replied without so much as a return smile, but tersely admonished me that "May I inform you in advance that General Lord will be happy to see you for three minutes."

I was then directed into the waiting room where again a second secretary informed me that I could see the General for only three minutes.

When I was finally ushered into General Lord's office, having arrived early, a third male secretary whispered in my ear: "Your appointment, sir, is limited to a three minute interview" and added, "the General is very punctual and very busy today." I must admit that I initially received a cordial reception from the General, but from then on it was another story.

After speaking for a full minute, I casually dropped my watch on the table with a "thump," since I noticed that General Lord wasn't paying much attention to my story, but instead was reading and sorting the voluminous mail on his desk.

He then suddenly looked up for the first time and asked me why I had dropped my watch on the table. Of course, I was momentarily flabbergasted for a reply.

However, I decided to add a little humor if it could be designated as such, to clear the apparent foggy atmosphere, and replied, "I sincerely appreciate that you are extremely busy. Three of your secretaries individually limited my interview to three minutes, before ushering me into your office, which would total nine minutes. Sir, I

will complete my interview in the remaining two minutes, of course with some difficulty. Nevertheless, I promised to remain within the time allotted to me.

Thus far, I have spoken but one minute. In brief, then, my mission is in line with the probable future programs to be anticipated in the field of radiation sciences at the Bureau of Standards toward improving the present methods of applying and measuring the radiation dose treatment of cancer and allied diseases.

You know, General Lord, we are both personally interested in the final outcome of this proposed program." Then I looked the general straight in the eye and said, "You are going to die of cancer and--pause--so am I, perhaps."

"However, if in the future it would be possible to administer more effectively the required x-ray or radium dose with greater safety due to the added availability of a National Standardized x-ray unit of radiation measurement, the chances for both of us percentage wise, will be more favorable in having our suffering from cancer cured in a hospital or elsewhere--wouldn't that be worthwhile? The need for obtaining the cooperation of the Bureau of Standards for a standardized unit is just about as simple as that."

Then for the first time he looked me straight in the eye, with a shocked expression, then called in his secretary and asked her to be prepared to take notes. He also suggested that I repeat my earlier remarks. I remember one question in particular which he asked. "You mentioned cancer, Dr. Ernst. Should I contract this disease and x-ray treatment was administered here in Washington, could other radiation laboratories in hospitals accurately administer the x-ray dose by employing other measurement tests than the proposed standardized x-ray unit you have discussed as a hopeful substitute?" I replied in the negative. "Most of the radiation laboratories have their pet measurement programs or methods of supposedly checking their radiation dose, none being nationally standardized." I have been experimenting with various older test methods but none were found to be reliable. Expressing the radiation x-ray dose in milliampere minutes technique by measuring the electric current for the application of superficial x-ray therapy frequently employed, could in limited cases be reasonably safe when applied by experts in my office or hospital, but may not be accurately reproduced by other radiation laboratories expressing the identical dose for use by other laboratories.

Neither the radiotherapy departments in your Washington hospitals, other United States hospitals nor the National Bureau of Standards have available reliable methods in any form for designating an x-ray dose standard with the same degree of accuracy, not unlike the Bureau here in Washington, which has the facilities for standardizing the unit of quality and strength of the electric current which we use and pay for, namely the "volt" and "watt."

Throughout the United States and abroad the radiologists and physicists in charge of x-ray laboratories, cancer institutes and hospitals for the treatment of cancer and allied diseases have in most instances relied upon their individual measuring methods, even the popular milliampere minutes mathematical calculation procedure, for comparing or controlling the radiation dose, none of which procedures have been standardized nor could they be except for an accepted proposed standardized unit under discussion today. In our area, toward the latter months of the First World War, portable x-ray machines were furnished to the Base Hospital Unit 21 in France. The 110 line voltage varied 5 volts either way. Therefore those units were almost worthless for even diagnostic service near the front lines. The x-ray non-portable coils which I employed could be more easily regulated.

The electrostatic x-ray unit "e" suggested by Professor Duane of Harvard and who was present at our meeting last Sunday, will be discussed at our coming meeting in London. This is the type of standardized unit which should be given consideration even at this late period in the history of our National Bureau of Standards.

I also emphasized that our Standardization Committee of the Radiological Society of North America which met here in Washington and the officials of the U.S. Bureau of Standards will receive an invitation to attend the First International Congress of Radiology's scientific meeting to be held in London this year.

We will thus have the opportunity of officially meeting with the members of other Standardization Committees representing the major countries abroad. These may number several dozen or more.

In the meantime I have been in communication with several of the chairmen of the English, German, French and Italian and Swedish Standardization Committees. In my Delegates report representing the United States I will include mention of our future x-ray unit deliberations and accomplishments of our Standardization Committee here in

Washington and sincerely hope it will be a favorable one and in line with our objectives of receiving full cooperation of the Government and the National Bureau of Standards. We of course favored that the Roentgen Unit could logically be designated by the symbol--small "r."

Appropos: Sir, what will happen to the radiation treatment of disease should more powerful higher voltage x-ray sources be developed in the not too distant future? Certainly the public might not be adequately protected from excessive radiation therapy."

I thanked General Lord for being patient during my interview. He then in turn promised to let me hear from him prior to the International Congress of Radiology meeting in London.

I was beginning to feel more and more optimistic that an early solution was in the offing for eventually obtaining full cooperation with the National Bureau of Standards and the Government officials who are at present involved in our discussion.

I discussed with Dr. Burgess and others of the Bureau what might be the approximate cost of outfitting such a scientific radiation laboratory, including standard dosage chambers for calibrating smaller portable dosage meters in which Dr. Glasser of Cleveland had been interested.

The cost of added personnel for studying the entire x-ray unit problem, including the protection from stray radiations to the community and the relative effectiveness of certain materials which are employed in radiation research in the practice of our profession, which includes lead, glass, barium, plasters, rubber, and steel, would be considered and should be investigated by the Bureau.

Following the meetings between Ernst and General Lord, the normal Congressional wheels were put into motion. For example, Dr. A. U. Desjardins, radiologist, on April 3, 1926, wrote to his Congressman Walter H. Newton, backing up the position taken by Ernst in his conversation with Lord. Since the proposals from Desjardins and Ernst were in the medical field, Newton referred the Desjardins' correspondence to Dr. H. S. Cummings, Surgeon General of the Public Health Service. Newton expressed the opinion that NBS, which had been proposed as the agency to conduct the program, was, in his opinion, well qualified for the purpose. He thought that the desired appropriation would serve the very highest purposes and thus facilitate treatment of recognized benefit to large numbers of sick people in this country and throughout the world. Cummings also supported an appropriation for the Bureau.

At the same time, Newton referred Desjardins' correspondence to the Bureau for comment. In replying, Burgess stated,

"From a consideration of the urgent problems proposed by the International Congress of Radiology at London, the Pan-American Standardization Conference at Lima, Peru, the Radiological Society of North America, The American Roentgen Ray Society and various prominent physicians and manufacturers of x-ray equipment, this Bureau was convinced of the need for the standardization of x-ray equipment and dosage, both from the standpoint of the public and the medical profession.

"A considerable number of requests have been made that an appropriation be sought for this purpose. During the past two years requests have been submitted by the Department of Commerce to the Bureau of the Budget for funds to carry on the work. This Bureau will be very glad to renew its request that provisions be made for this work in the 1928 budget."

Similar letters were written in response to communications from Senator Coleman DuPont, Senator Thomas F. Bayard, and Senator Frederick M. Sackett, as well as a number of Congressmen.

On June 18, Burgess in a letter to Ernst said,

"You will understand that the question of budget items is confidential and cannot be released for publication. I can say to you, however, in confidence, that I am submitting in the preliminary estimates for the fiscal year 1928 an item of $30,000. From my conversation with representatives of the Budget Bureau, I gather the impression that this may be favorably received."

On March 29, 1927, Dr. Paschen, President of the Physikalisch-Technische Reichsanstalt, in responding to a letter from Dr. Burgess, listed a number of radiation instruments which

he thought might be of use to the Bureau's radiation program. By this time it was clear that the radiation program would proceed on the $30,000 per year basis. Also, it had been decided to appoint the author, Lauriston S. Taylor, to the position of Assistant Physicist under Dr. Hunt in the Atomic Physics, Radium, and X-Rays Section. The efforts of the American Roentgen Ray Society and the Radiological Society of North America had paid off.

The respective committees of these two societies chose to go their separate ways for several years, but this was not serious. There were scarcely enough radiological physicists in the country to adequately comprise two different committees. Conversely, it would have been even more difficult to find enough radiologists with suitable backgrounds in physics to staff two committees effectively. As a result, there was a fair amount of overlap of the committees' memberships which provided a good level of cooperation and no basic policy differences between the two societies. Gradually the two committees began meeting together more formally, and in 1936 combined operations with Taylor as chairman of a joint committee of the RSNA and ARRS (see p. 200).

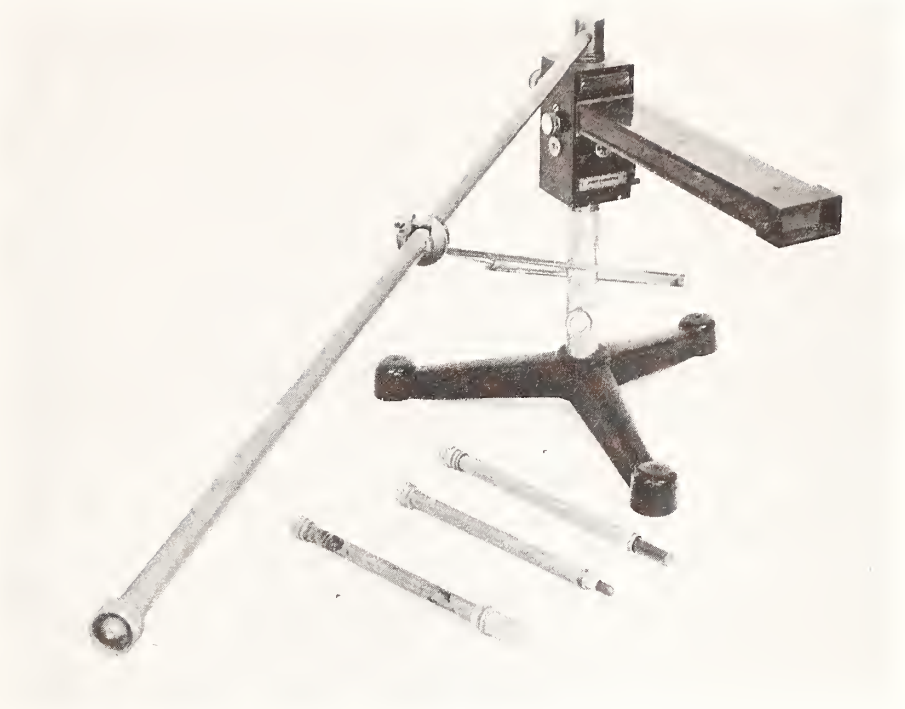

Photo No. 1. Fricke-Glasser clinical dosimeter (1927).

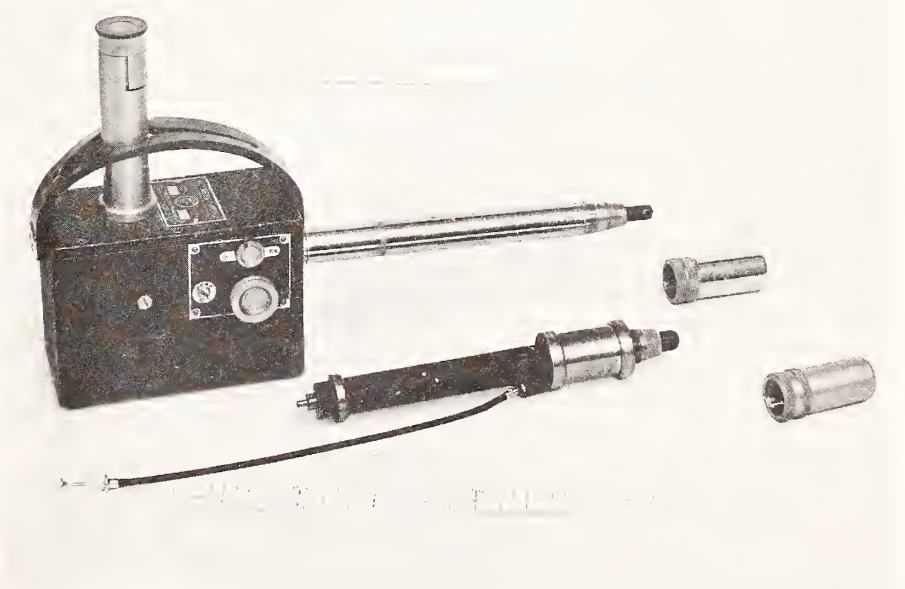

Photo No. 2. Victoreen clinical r-meter, with NBS modified components (1930).

CHAPTER 4. X-RAY RESEARCH STARTS AT NBS (1927)

NOTE: On July 17, 1927, the author joined the NBS staff and took charge of the Bureau's x-ray measurement and protection program. This chapter and subsequent ones will reflect his direct involvement in efforts to develop such a program, despite the many problems he had to face, initially as well as in the years to follow.

On arrival at the Bureau from Cornell University, Taylor was faced with a situation that would greatly influence the very role he would play in the Bureau's x-ray measurement and protection program:

- Attracted to NBS for the opportunity to work with Dr. Franklin L. Hunt, he learned that Hunt was leaving NBS that week to join the Western Electric Company in New York (where Taylor had once worked and where he could have returned on leaving Cornell).

- Dr. Paul D. Foote, Chief of the section and the only person Taylor had met previously, was also leaving the Bureau within 2 weeks to join the Gulf Research Laboratories in Pittsburgh.

- Dr. Fred Mohler, who was to succeed Foote and with whose work Taylor was familiar, had left the previous week for a month's vacation in Maine.

- And finally Taylor found he was to work in an area quite different from that of x-ray spectroscopy as he had expected, and with which he had been involved at Cornell.

Consequently, Taylor's introduction to NBS and orientation on the work program was less than satisfactory and obviously disturbing. Hunt had very little time to brief him on the myriad of details about procedures and program plans, about which Taylor had little knowledge. Hunt mentioned a program he was expected to set up. It was a system for calibrating and standardizing ionization instruments for use in hospitals.

Hunt handed Taylor a brass box about 8 inches square and 3 inches thick. One side was open, the other side was lead with a 1- x 5-cm slot. He identified the device as a Duane-type standard ionization chamber which he had developed for Bureau of Standards' use in calibrating x-ray beams and secondary ionization chambers. All Taylor need do, he explained, was to put a few hundred volts on one plate, connect the other plate to a galvanometer, and then measure the current produced when an x-ray beam passed between the plates. After that, Taylor could devote whatever time he wished on vacuum spectroscopy and crystal structure analysis.*

*At this point, Taylor felt compelled to go to the Division Chief, Dr. Clarence A. Skinner, an older, more experienced person. Taylor explained his predicament, but promised--having accepted the job--to stay on for 1 year to get the program organized. Meanwhile, he suggested that Dr. Skinner locate and hire a replacement. The conversation was easy and reassuring, as were many others that were to follow. A year later, Taylor asked Skinner if he had found a replacement. Skinner expressed hope that having observed Taylor's interest and enthusiasm in the work, he would be willing to stay on longer. This was indeed also Taylor's hope--his 1-year agreement ended 37 years later.

The facilities and equipment that Taylor inherited had considerable influence on his immediate program plans. The work space consisted of two rooms, about 20 feet square, on the third floor of the East Building. One room was jammed with a miscellaneous assortment of old x-ray equipment, including a Waite and Bartlett transformer with a peak voltage of about 140 kV, a Coolidge universal x-ray tube that would operate up to that voltage, a single Kenetron and filament transformer, together with the associated gear (rheostats, etc.) for operating the transformer (see photo No. 3). Hunt had obviously planned to build a small constant potential generator since there was a battery of some dozen glass Leyden

jars and a homemade oil condenser, consisting of glass plates about 3 feet square with lead foil glued to each side.

The second room was relatively empty and was obviously destined to have been the spectroscopy facility. There were parts of two vacuum spectrometers, a partially constructed x-ray camera, and an x-ray tube for crystal structure analysis. A third vacuum spectrometer was in the process of construction in the instrument shop.*

*Sometime later, on close examination, Taylor decided that the spectrometer equipment would not operate as designed.

Other equipment in the Northwest Building consisted of a 300,000-volt mechanically rectified Wappler generator, using double rectifiers and two transformers, each with one end grounded (see photo No. 4). These were contained in a heavily constructed lead room approximately 10 x 12 feet and some 12 feet high, similar to the facility built at the Watertown Arsenal for the radiographic inspection of heavy metal parts. Also in the room were two x-ray tubes, one a 200-kV, 8-mA air-cooled tube and the other a 200-kV water-cooled tube capable of operating up to 50 mA (see photo No. 5). In addition, the room contained auxiliary equipment, all of which had to be insulated for 100 kV above ground, including the water cooler, switching gear to change from one tube to another, stabilizers for the tube current, and the tube current milliammeters. The controls were outside of the room but there were no windows or other means of observing the equipment during operation.*

*Since no one on the staff knew how to operate the machine, the supplier finally had to be brought in to make the facility usable. A lead-glass window was later installed in one side so an operator could view the control meters. Control strings were led out of the room through bent lead tubes to operate the sphere gap for measuring the voltage.

Taylor, recognizing that he knew little about x-ray dosimetry and that there were insufficient resources to work with in any case, spent 2 months researching the existing French, German, and English literature back to the 1890's. While there was indeed a widespread interest in the problem, the literature was not very extensive. However, it soon became obvious where the problems lay, and ideas began to develop as to how one might go about solving some of them.

His next step was to visit the four physicists in the United States, who, judging from their publications, had substantial background in the field of x-ray dosimetry. These were Dr. G. Failla, Physicist at the Memorial Hospital in New York, Dr. William Duane, Professor of Physics at Harvard, Dr. W. D. Coolidge, Assistant Director of Research at the General Electric Company, and Dr. Otto Glasser, Physicist at the Cleveland Clinic. He spent 2 or 3 days with each of these individuals, all of whom were encouraging and helpful. There soon emerged a clearer picture as to what the Bureau of Standards' program should be, where the primary emphasis should be placed, and what initial experiments should be undertaken.

Of the four, Failla was the most helpful. In fact, a close professional relationship developed between him and Taylor which lasted until Failla's death in 1961. Although close working relationships were also developed with Dr. Glasser, Glasser's interests shifted to other areas once the Bureau of Standards' programs were underway. However, contacts with him were maintained through common membership on committees and attendance at technical meetings. Coolidge was a great source of help and guidance, primarily in connection with the tube and general equipment problems. Taylor had met him earlier through Professor Richtmeyer at Cornell, under whom he was a research fellow working on x-ray absorption spectra. Duane was the oldest of the group and nearing retirement at that time. When he learned that the Bureau did not intend to use his ionization chamber as a standard, he became somewhat antagonistic to the programs.

And so, by the end of October 1927, the problems were fairly well understood, and there was a reasonably clear indication how to proceed.

The Situation, October-November 1927

Clearly, the prime objective should be to develop and test a suitable free air standard ionization chamber for use up to 200 kV. The Duane chamber was obviously inadequate. (It was not yet suspected that the pressure chamber at the PTR was also inadequate.) The Solomon chamber technique, using radium seed calibration, could not be rigorously defined for reproduction. The Küstner cylindrical standard chamber was good only up to 100 kV. Moreover, considerable work was necessary to acquire more sensitive, more reliable, and

readily reproducible systems for measuring the ionization current in whatever ionization chamber would be used as a standard.

Studies would need to be made of protective materials, especially those requiring heavy elements other than lead for their protective properties. Particular attention should be paid to barium plasters, barium glasses, and the general procedures involved in their application. Radiation scattered from the patient, as well as apparatus and tube shields, would need to be evaluated and analyzed to determine the proper shielding requirements. Special attention would need to be given to shielding of the operator using fluoroscopic equipment. The same would be required for shielding on radiographic equipment. Since scattered radiation is less penetrating than direct radiation, shielding requirements for x-ray rooms could be made more economical with a better understanding of the radiation and shielding characteristics.

Voltage sources for exciting x-ray tubes should be evaluated and compared, and a system for reproduction of radiation quality should be developed. There were at least a half dozen kinds of mechanical rectifiers in use, all giving different wave forms resulting in different radiation qualities. Constant potential, high-voltage sources were being considered for therapy, as well as for laboratory use. The design and operating characteristics were unsettled. Valve-rectified high-voltage sources yielding half-wave and full-wave rectification were in use. They should be compared with constant potential and mechanical rectifiers. Line voltage variations were troublesome and stabilization should be sought. Consideration should be given to the use of a special motor generator set, using a synchronous motor to drive the generator and providing the supply voltage for the x-ray laboratory. X-ray tube current stabilization should be made more sensitive, probably through the use of storage batteries to light the filament.

Where the x-ray standards were used, a 200-kV constant potential set with a ripplage of not more than about one-tenth percent per milliampere should be provided. The 300-kV mechanically rectified installation in the lead room should be studied for possible laboratory use other than ionization measurements. This would probably limit its usefulness to some of the protective material studies. But even then, means for external control would have to be devised.

X-ray tubes rated at 200 kV frequently became very unreliable and unsteady at about 150 to 160 kV. Practically all of these were made by the Victor X-Ray Corporation. Earlier tubes and a few special tubes made by the General Electric Company in Schenectady were usually of much better quality. A few German tubes were in use, but their performance was not consistently better than U.S. tubes. Parameters for the comparison of various types and varieties of x-ray tubes should be worked out.

Radiation quality of therapeutic radiations were measured by one of three methods--effective wavelength (Duane), average wavelength (Mutscheller), and half-value layer. Advantages and disadvantages of each were not clear, nor were the relationships between them.

There was great need for depth-dose data, that is, the distribution of radiation levels within a body. As a part of this, it was important to have a better understanding of the quality of the scattered radiation originating at any point within the body (see p. 174).

The only proposals for permissible tolerance doses for radiation workers were expressed in terms of threshold erythema doses. This had been studied in many places, and the results were widely scattered. NBS should try to evaluate the threshold erythema dose in terms of field-size and the quality of the radiation.

Comparisons should be made between the radiation and operating characteristics of different clinical instruments for measuring ionization. The comparisons should include selenium detectors (Furstenau), photographic films used as dosimeters, chemical compounds used as dosimeters, and a wide variety of "pastilles" which had been in use for over 2 decades (as developed by Holzknecht, Sabouraud, Noiré, Kienbock, and Holfelder). Pastilles were still being used in some parts of Europe, but had very little usage in the United States at that time.

Coolidge had recently developed a high-voltage tube for allowing electrons to pass through a thin window into air, then called Lenard rays. Since these had potential for superficial therapy, standardization of Lenard rays would be needed.

Grenz rays (5-20 kV x rays) were used for superficial therapy. The few tubes in use were radically different, and there was no standard method for measuring the x rays. Voltage measurement and description for high voltages were essential for reproduction of treatment conditions. Voltage measurement techniques, other than the sphere gap or the needle gap, were critically needed. It was also necessary to make voltage measurements possible at remote points.

Again, by instinct as a physicist, Taylor felt that absolute energy measurements of the x-ray beam should be made by calorimeter or other such means. Calorimetric devices had been worked on in two laboratories in Germany (see p. 173).

Again, by instinct, it was felt that complete spectra should be obtained for x rays produced under the wide variety of conditions indicated above. (However, as the researches continued, the need for these last two items appeared to lessen. Most of the above items were subjected to study over the succeeding years, and all but a few studies were completed prior to 1942. Following the war, new needs developed which will be discussed as appropriate.)

Explorations and Plans

It was quite obvious by early fall (1927) that at least one additional person was needed to assist in the control of equipment and other matters involving experimental work. The first person brought in was Mr. Cyrus G. Malmberg at a sub-professional grade. (Under the Bureau's programs for assisting the staff to complete their education, Malmberg obtained his Bachelor's Degree soon afterwards.) Later in the year, another professional person was added to the staff--a Mr. George Singer from Iowa State University.*

*Singer's appointment proved to be a great asset to the section. He made valuable contributions to the technical program as well as assisting in the selection of future staff members up to his sudden death in 1946.

After extensive reading research, it was decided to check out some of the ideas that had developed, to determine to what extent the available equipment could be used, and to initiate some exploratory studies. It was immediately evident that these could only be exploratory because the mechanical rectifier could not provide sufficiently steady outputs. To solve this problem a drumhead type ionization chamber was placed near the x-ray tube inside the x-ray room to monitor the ionization currents being produced. Fortunately these were large enough to be read by a meter of only moderate sensitivity and hence not be unduly sensitive to building vibrations. Thus, the water-cooled tube, yielding a moderate output at some 20 mA, seemed to be satisfactory.

A hole through the lead lining of the x-ray room permitted a beam of about 2-inch diameter to emerge and to be diaphragmed as required for experimental use. The first tests used the Duane chamber that Hunt had left, together with the most sensitive L & N galvanometer then available. The plan was to take galvanometer readings and monitor readings simultaneously, and use the difference between the two as an indication of the ionization current in the Duane chamber. However, the maximum indication for the ionization current was only 2 or 3 mm on the galvanometer scale and the measurement technique was abandoned.*

*It was impractical to use a galvanometer simply mounted on one of the concrete columns of the building. In the basement directly beneath the x-ray laboratory was a heavy-duty forging hammer or press belonging to the Metallurgy Division. When the press was operated the building shook so severely that the effect was much like a small earthquake. This was overcome with a special spring-suspended shock-mounting for a heavy counterweighted table upon which the galvanometer was mounted (see photo No. 6).

To make some exploratory measurements while awaiting better instrumentation, a simple gold-leaf electroscope, similar to the one used in the radium laboratory was enclosed in a 4-inch cube lead box and was insensitive to vibration. Even with such a crude arrangement, the shortcomings of the Duane chamber were evident (see photo No. 7).

The next tests involved the Dolezalek quadrant electrometer, a piece of fiendish, but useful equipment, which Taylor had used at Cornell. It worked well on the shock-mounting and, fortunately, did not need to operate at its maximum sensitivity (which was estimated to be as high as 35,000 mm per volt). By operating at a few thousand millimeters per volt, the instrument was more stable, much easier to adjust, and took only a month to adjust into suitable operating conditions. The first rough experiment was to learn something about field distortion and plate separation of the Duane chamber. The Duane chamber was disassembled to permit the collector electrode plates and the high-voltage plates to be placed in various positions with respect to each other and with the grounded surfaces at the ends. A large sheet of lead with a 1- x 5-cm diaphragm, essentially the same as that in the

Duane chamber, was placed as close as possible to the target--about 1 meter. This gave an adequate radiation level for ionization measurement by the Dolezalek electrometer.

Crude as they were, the preliminary experiments demonstrated that the errors introduced by inadequate plate separation and inadequate distance from grounded surfaces would cause an overall uncertainty of 25 percent in the value of the standard.

At about this time Dr. Hermann W. Behnken, of the German Physikalisch-Technischen Reichsanstalt (PTR), visited the Bureau with the hope of checking the Bureau of Standards' measurements against a small ionization chamber that he had brought with him. In fact, he was in the country for the purpose of checking standards with Duane, Glasser, and the Bureau, and then reporting the results at the December meeting of the Radiological Society of North America. Though the Bureau, at that point, had no suitable device for comparison, Behnken's visit proved to be an extremely valuable one. Behnken was in charge of the x-ray standardization programs at the PTR, and he had been in the field for at least 5 years. He was able to provide Taylor with a great deal of background information and experience that could not have been gained from reading the literature. The visit established a strong base for future cooperation between NBS and the PTR.

Another unexpected visitor was Dr. G. W. C. Kaye, Director of the Physics Division of the British National Physical Laboratory (NPL) in Teddington. Kaye, one of the best known British scientists in the x-ray field, was in charge of the NPL x-ray standards program and was the author of a couple of well-known books. Kaye was not particularly interested in the NBS standards program, for neither organization had one yet. He was, however, very much concerned with the problem of radiation protection in his capacity as the Honorary Secretary of a temporary committee charged with forming a permanent committee for developing recommendations on radiation protection for international adoption in 1928 at the Second International Congress of Radiology in Stockholm. Kaye was soliciting the cooperation and backing of NBS in these radiation protection activities and urged Dr. Burgess to send an official representative to the upcoming Congress. (Taylor did attend the meeting and, en route, visited Behnken's and Kaye's laboratories and some dozen others where x-ray standardization measurements were being studied.)

Profiting by the experience and advice of Behnken, a more manageable, but crude parallel plate ionization chamber was made for obtaining further design information. In this, the guard plates on both sides of the 10-cm-wide collector electrode were some 15 cm wide and about 30 cm high (see photo No. 8). Opposite this assembly was the high-voltage plate of roughly 30 x 40 cm. These were contained in a lead box which was large enough to permit the electrodes to be placed no closer than some 15 or 20 cm from the inner surface of the box (Ref. 4). These particular dimensions were chosen partly on the basis of literature, partly on the basis of some experiments which Dr. Failla was carrying out at approximately the same time (Failla, 1929), and partly on advice from Dr. Brooks in the NBS Electrical Division, who was of great assistance many times in the electrical measuring aspects of the x-ray program.

Measurements made with this chamber at various plate separations and distances from the grounded box provided enough information for the design of a free-air chamber, which would satisfy the requirements of the definition of the roentgen as it was then proposed (see photo No. 9).

However, tests made with a 1- x 5-cm diaphragm, to limit the beam passing through the chamber, as in the case of the original Duane chamber, introduced errors that could not be evaluated.

It was during this period that there were new interactions between the Bureau of Standards and the Radiological Society of North America, the organization most responsible for this particular NBS program. Taylor attended the annual meeting of the RSNA in New Orleans in December 1927 to meet with their Standardization Committee. At the meeting he discussed the effort the Bureau was putting into the program and how it was progressing. At the same time, he was made a member of the Committee, a position which he held until the committee was disbanded in the late 1940's.

The meeting also gave Taylor the opportunity to become acquainted with many of the radiologists of that time, all of whom were enthusiastic about the Bureau's programs and anxious to help. Through their scientific and technical exhibits, he also became acquainted with the latest developments in x-ray apparatus and techniques, as well as medical procedures.

In late fall, the necessity of a constant-potential x-ray generator was recognized. One of the two rooms originally available for x-ray work in the East Building was cleared and prepared to house the new generator, despite the fact that the space was accessible only through another division's laboratory. The new installation required a large amount of

electrical wiring, which the Plant Division could not provide for at least 3 months. To prevent delay, Taylor and his staff located some salvageable wire and did the wiring themselves.

To justify projects and their funding at that time, "research authorization" statements were required. However, unlike present-day programs, some four or five lines of descriptive text were adequate for each project. In the late 1920's, the work of the x-ray laboratory was authorized and carried out under the following numbers and titles.

4626 X-Ray standardization
4628 X-Ray quality determination
4629 Cathode ray dosimetry
4632 X-ray protection
4633 X-ray protection materials
4634 Functioning of x-ray equipment
4636 Functioning of x-ray tubes

Others were, of course, added later, but the notable thing about the research authorization process was that these assignments remained in force from 1927 to 1947, when Dr. Edward Condon became Director of the Bureau of Standards.

At that period, monthly reports were required. A 10-line report on a project was considered excessive, and frequently the entire section report (Mohler, Taylor, and Curtiss) would be on one page and rarely more than two. A random sample follows:

MONTHLY REPORT, APRIL 1933 (RESEARCH)

4642. Radiation from discharges. A paper on Spontaneous Recombination and the Effect of Vapor Pressure in a Cesium Discharge, by F. L. Mohler, was given before the Physical Society, April 28.

A paper on The Electric Discharge in Cesium Vapor, by F. L. Mohler and C. Boeckner, was given before the Washington Philosophical Society, April 22.

4626. X-ray standardization. Considerable time has been spent in preparing specifications for an international agreement on X-ray units and standards between the United States, England, Germany, and France. Satisfactory progress seems to be made thus far. At the same time, we are assembling data for presentation at the next meeting of the International X-Ray Units Commission both as from the Bureau of Standards and from the American Standardization Committee. Remodeling of the permanent x-ray standardization equipment is being continued.

4643. Biological comparison of X-radiation. Preliminary measurements necessary to the comparison of skin erythemas under standardized conditions were practically completed when two of the manufacturers found it necessary, on account of the depression, to withdraw apparatus loaned us for this purpose. This work is being postponed until we can purchase for ourselves a few pieces of equipment necessary to carry on the work. The physical part of this work has met with approval by the standardization committee and our method of specifying radiations will be recommended by the committee.

4634. Functioning of X-ray equipment. In expressing the effective voltage of half-wave X-ray generators, it has been found possible to correlate the measured voltages with the measured voltage on full-wave generators by the use of a derived multiplying factor. Experimental tests have adequately checked the calculations. Two papers on this work are in the course of publication - (1) "Comparison of X-Ray Generators" and (2) "Comparison of X-Ray Tubes".

4627. Geiger counter. An investigation of the recently announced radioactivity of beryllium has been started, using the Wynn-Williams type of amplifier and ionization chamber. The specimens of beryllium used showed activity but further study is necessary to determine whether this may be due to impurities.

4622. Beta ray spectra of iron. In an attempt to devise diffusion pumps operating with oil filling and obviating the necessity of using liquid air, several models have been made and a considerable amount of oil distilled for use in them. Satisfactory results have not been obtained. The elimination of liquid air will effect a considerable economy when operating pumps for long periods.

Respectfully submitted,
F. L. Mohler, Chief
IV-6

This account of the initiation of the Bureau's x-ray measurement and protection program would be incomplete without due credit to the very important contribution of a superb NBS instrument shop. Headed by a German instrument maker, Otto Lange, the shop was staffed with a number of highly-skilled instrument makers and machinists, who often became active and effective partners with the professional staff in creating the research tools not available commercially and on which program progress rested. Moreover, the services of the shop staff were available as an overhead NBS service, with minimal paper work and without charge against the research projects.

Available to the x-ray staff, for example, were two excellent instrument makers, one of whom, George A. Rheinbold, worked continuously on x-ray research projects for about 12 years. Rheinbold could work from rough sketches or verbal instructions, and, with Taylor, developed and applied many new techniques involving metal working, metal-glass bonding, and exotic insulating materials. They published a joint paper entitled "A remotely operating switch for roentgen dosage meters" (Ref. 33).* (See also photo No. 2.)

*The Instrument Shop at that time (1927) was also capable of metric design and construction. In fact, the Bureau had earlier decided to set a pattern for the country and "go metric." Substantial quantities of brass and steel bar stock were purchased in metric sizes close to English dimensions. Metric machine screws were stocked in a wide range of sizes, as were various other stock items. Several machine tools were re-equipped with metric lead-screws and/or lead-screw gears. However, there were difficulties in simultaneous use of the two measuring systems because metric bar stocks became mixed with English sizes, as did the machine screws. By the end of the 1920's, the futility of the project was recognized and several tons of unused metric supplies were surplused as scrap.

While the role of the Instrument Shop was a significantly positive one, there were problems in another service area that the x-ray research staff had to face, the solution of which was indicative of the resourcefulness of the NBS research staff at that period. At Cornell, Taylor had observed that the flashover potential of a neon glow lamp was changed when exposed to an x-ray field. To check out the possible application of this finding to his present work, he placed an order for one three-element vacuum tube and two different neon glow lamps. To his amazement, the order was returned along with a memorandum stating that the purchase of radio components had to be approved by the NBS Radio Section, headed by Dr. J. H. Dellinger, and that such experiments had to be carried out under the direction of someone from that section. Not being able to change the procedure, Taylor simply contacted a former colleague at the Edison Lamp Works at Harrison, New Jersey, and obtained a supply of tubes that, though rejected for minor reasons for commercial purposes, were completely satisfactory for the NBS experiments. Fortunately, by the early 1930's, this peculiar purchase barrier was finally resolved.

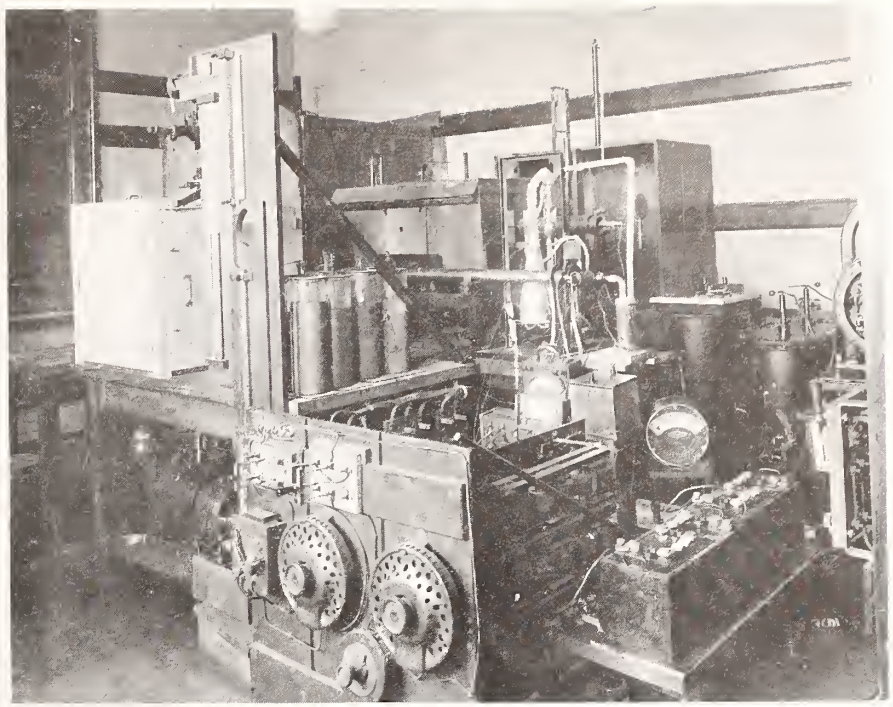

Photo No. 3. NBS X-Ray Laboratory (July 1927).

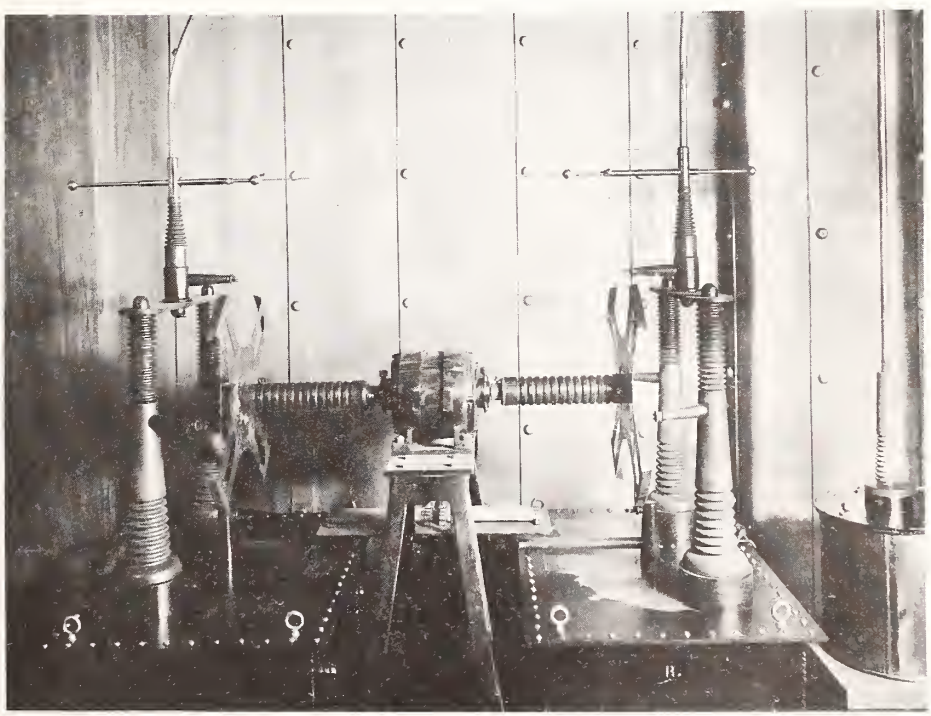

Photo No. 4. 300-kV mechanically rectified voltage source for x-ray tube (1927).

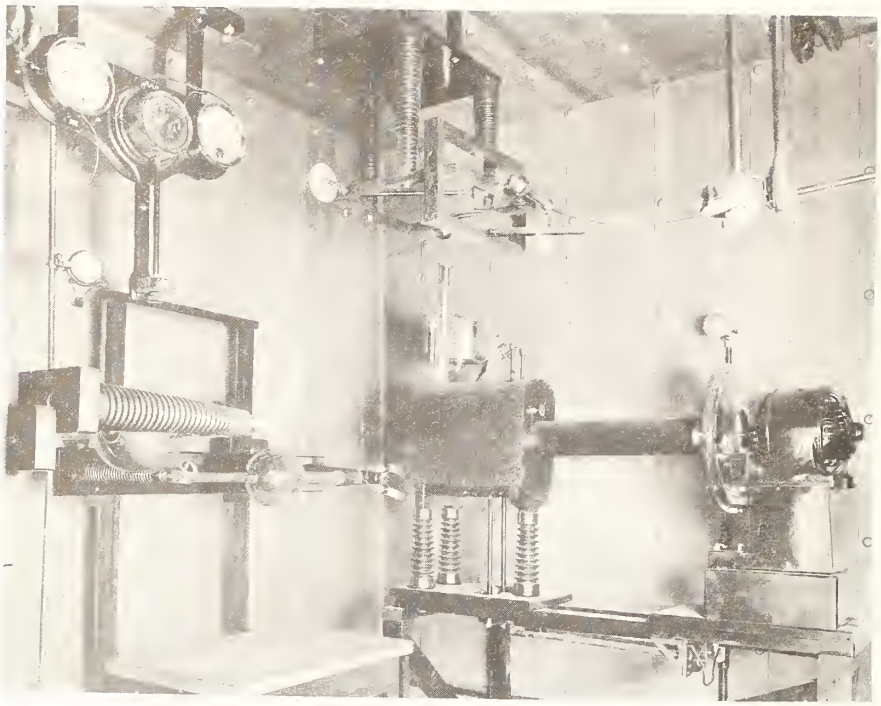

Photo No. 5. Two x-ray tubes, insulated water cooler, and control equipment inside room lined with 1/4-inch lead (1927).

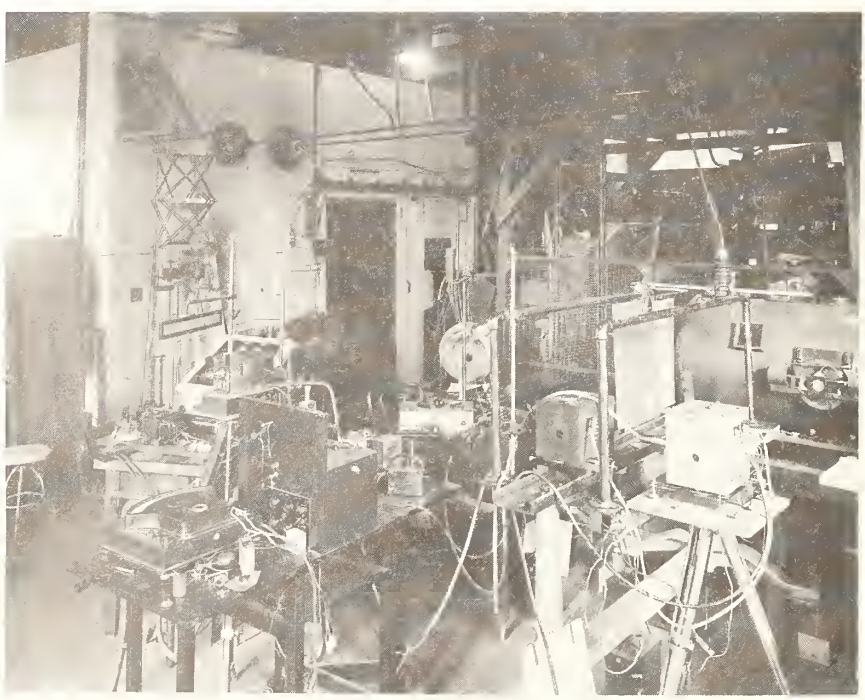

Photo No. 6. NBS X-Ray Laboratory in 1933 during comparison of NBS and Canadian x-ray standards. Note shockproof instrument suspension in left background.

Photo No. 7. Gold-leaf electroscope (in black shield) and Duane-type ionization chamber (in background). The x-ray source is in lead-lined room behind chamber (1933).

Photo No. 8. Initial standard ionization chamber plate system at NBS (1928).

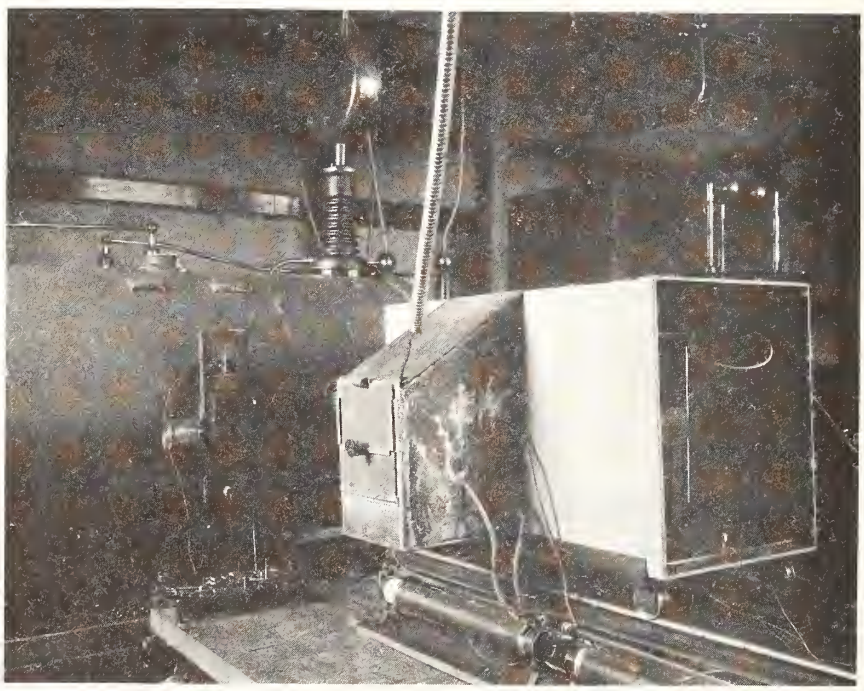

Photo No. 9. First complete x-ray standard ionization chamber for 200-kV radiation. Note string electrometer mounted on side, x-ray tube shield and shutter in background (1928).

CHAPTER 5. THE MEASUREMENT OF X-RAYS

By mid-1928, sufficient exploratory work had been done to warrant moving ahead with the building of a preliminary standard ionization chamber, high-voltage system, and electrometer system. While the exploratory measurements had been in progress, planning had also gone forward to the procurement of parts for the constant potential generator.

Programs (1927-1940)

The building of a high-voltage rectifier and constant potential generator was not new to Taylor. He had constructed one while working for Richtmyer at Cornell, but, as noted below, under circumstances which were much more difficult than those facing him at the Bureau of Standards. The principal problem was one of obtaining suitable capacitors for smoothing out the voltage. These had to withstand the high voltage (100 kV) as well as have sufficient capacitance. The filter network required four units, each with a minimum capacitance of 0.05 microfarads.

The unit at Cornell had consisted of banks of 15-kV radio transmitter capacitors in series and in parallel to take the maximum voltage which was in that case only 75 kV per bank. The difficulty with any such arrangement was that if one capacitor had more leakage than the others, it would put over-voltage on the others and cause a sparkover. One sparkover caused the whole bank to sparkover; hence, the staff had to get used to periodic lightning bolts and thunder crashes when something went wrong. In principle, this was well known in the "Marx Generator," a problem that had finally been solved by putting water leakages across each of the condensers. However, when looking into the capacitor question in 1927, it was discovered that some large single-unit capacitors at voltage ratings up to 100 kV or more were available in Germany, known as Pertinax capacitors. Manufactured by the Meirowsky Company, each unit was roughly 25 cm in diameter and about 2 m long. They had a dry-type construction with a phenolic dielectric, evidently wound up in a cylindrical form very much like the small paper capacitors for radio applications. With these capacitors available, the biggest problem was overcome.

The next problem was to design a layout for the equipment, providing adequate space between various components to prevent sparkover, either between components or to parts of the building. In the final layout, which took up approximately half of a 20- x 20-ft room, the four Meirowsky condensers were supported horizontally by a framework near the ceiling above the high-voltage transformer, filament transformers, kenotrons, etc. (see photo No. 10).

The 200-kV x-ray tube was suspended on insulators on the axis of a lead-lined steel cylinder about 30 inches in diameter and about 5 feet long. A lead lining of 1/4 inch, provided to protect the operator a few feet away, was considered to be adequate based on initial tests using the common dental film/paper clip technique. The cylindrical tube-shield was mounted on rails approximately 3 1/2 feet above the floor, so as to provide a horizontal beam for the ionization chamber. Thus the tube could be moved into different positions for different experiments, rather than the usual way of having to move different lots of apparatus in and out of a fixed beam. Although the complete apparatus was designed for 200-kV operation, it was rarely operated above 180 kV after experiencing sparkover in an attempt to go higher. However, enough data could be obtained at that voltage without risking damage to the equipment.

The preliminary standard chamber was built along the lines indicated by the exploratory studies made the previous fall. It consisted of a collector plate 10 cm wide, and guard plates 15 cm wide with a plate spacing from the high voltage plate of about 12 cm (see photos No. 8 and 9). The ionization current was measured with a new string electrometer system constructed along the lines of a Townsend current balance, similar to the balance Taylor had built at Cornell though unaware of its origin.*

*A number of years after this versatile system was described in a 1931 paper (Ref. 19), Taylor received a friendly note from England calling his attention to the prior development by Townsend.

The idea of using a galvanometer with such a system had long since been abandoned. An important feature of the electrometer was that the indicating meter, used primarily as a null indicator, did not need to be calibrated; it was only necessary to determine that it had adequate sensitivity and stability. These features overcame the shortcomings of the quadrant electrometers which tended to be fairly stable in the zero position but very unstable as the vane deflected. The staff was thus spared the agony of using a quadrant electrometer in any form.

When Dr. Behnken visited the laboratory in 1907, he brought a small string electrometer made by the Edelmann Instrument Company in Germany. This particular instrument was designed for idiostatic use which was not suitable for NBS purposes. To determine its possible NBS use, it was first necessary to obtain a more sophisticated instrument known as the "Lutz-Edelmann" electrometer, which was very flexible and could be used in a variety of ways. As a result of this study, Taylor designed a much simpler, but less flexible, modification of the instrument which was well suited as a sensitive null indicator (Ref. 19). It also used the ready-mounted Wollastan fibers with quartz loops obtainable from the Edelmann Company. About a half dozen of these instruments were made in the Bureau shops and were used in the 1931 and 1953 comparisons at the National Physical Laboratory in England. They continued to be used up until 1955, and in fact still have some advantages over the much more sophisticated electronic equipment now available.

The critical measurement in the current balance was the capacitance of the condenser, one plate of which was connected to the collector electrode in the ionization chamber and the other to a variable voltage source. Capacitances on the order of 50 to 200 picofarads were such that, in an attempt to calibrate the capacitors, the connecting lead capacitance could introduce large and undetermined errors. The NBS Capacity Section was simply unable to do the calibration. However, by means of the current balance itself, the condenser could be calibrated by comparison against a variable condenser in which the capacity differences between different settings could be determined with considerable accuracy, a task which could be done by the Capacity Section. This technique, described in reference 19, was also useful for calibrating small capacitors that the x-ray group was often asked to calibrate for the NBS Capacitor Section.

During the early use of string electrometers, it was frequently necessary or desirable to remotely locate the current balancing system with respect to the ionization chamber. In the original setup (Ref. 4), the electrometer and capacitors, shielded in lead, were fastened to the side of the ionization chamber so that the observer's head was near the x-ray beam as it passed through the chamber (see photo No. 9). This was thought to be a safe situation for the observer; later measurements indicated an exposure on the order of 0.5 R/day, which, in view of the daily workload, was adequate.

Connecting leads from the electrometer to the ionization chamber had to be well insulated and free of any air cavity. One could not use a simple wire through a hollow tube because such an arrangement would, in itself, be an ionization chamber which would introduce spurious currents into the system. As in the construction of the early Fricke-Glasser clinical dosimeter (see photo No. 1 and p. 18), this problem was overcome by stringing a wire through a 1/2-inch aluminum tube with suitable amber end-fittings and then filling the tube with ceresin wax. These could be interconnected using special "knuckle joints" which allowed bends in the electrometer line. Because there was air space in the connectors, lead shields had to be provided after they were in position.

Other than for null usage, this type of system had a disadvantage. If the potential between the wire and the case differed by an appreciable amount (sometimes it was on the order of 100 volts), an "electrical soakage" phenomenon would take place in the ceresin wax. Thus, even after the voltage was removed, the electrometer would still give false indications. This, of course, did not happen when using the current-balance null-indicating method which led to the adoption of a very convenient and inexpensive expedient. Ordinary automobile ignition cable, with fairly thick rubber insulation, was found to make a suitable insulated conductor when equipped with proper amber fittings and standard copper braid shielding. From 1930, this type of cable was used routinely but with the following precaution: the cable could not be flexed violently during or shortly before the readings because frictional charges, which were temporarily stored in the insulation, would gradually leak off, causing disturbances in the electrometer's balancing.

It had been noted earlier that the large rectangular diaphragm on the ionization chamber as used by Duane led to a variety of uncertainties. Behnken, Glasser, and others had already appreciated this problem and were using beams through the ionization chamber with circular cross sections on the order of 1 cm in diameter. Even then, additional diaphragms had to be inserted between the x-ray tube and chamber to prevent stray radiation from

entering the chamber in any way. It was also necessary to place a preliminary diaphragming system close to the tube to ensure the production of an adequate and uniform radiation field for calibration of a thimble chamber. To ensure a uniform field 5-10 cm in diameter at a given point, considerable attention had to be given to the shielding and diaphragming. This seemed like a simple requirement but its solution proved to be tricky at times.

To obtain a 1-cm field and a 5-cm field at the same distance from the x-ray tube target, as required, it was necessary to vary the position of either the standard ionization chamber or the x-ray tube. Fortunately, the NBS installation had been designed with this provision in mind; the x-ray tube had been mounted on rails and could be moved for calibration purposes to permit different set-ups to be kept in place. The description of the high voltage plant and the ionization chamber system is contained in a paper entitled "The Precise Measurement of X-Ray Dosage," by Taylor (Ref. 4).*

*The processing of this paper was to be an educational experience for the author, reflecting a considerable difference in the review prodedures at a university and those followed by the Bureau. At Cornell he had experienced no particular difficulties in gaining prompt approval of some 10 papers by the Physics Department. At NBS he discovered that his manuscript must first clear his section chief, Dr. Mohler, then the division chief, Dr. Skinner, then an Editorial Review Board, before it could be sent to the Bureau's publication office. The paper passed Mohler intact, but Skinner spent several days on the manuscript making changes and completely rearranging the text. Several conferences followed between Skinner and Taylor, and finally an agreement on a revised manuscript and approval for publication was reached. Despite the frustration of his first experience with the Bureau's review process, the author concluded that the paper had been improved by the treatment. (This rigorous editorial process, which became a permanent part of the Bureau's operation, has played a very important role in the quality, accuracy, and integrity of papers published by the NBS staff.) The time Skinner had spent on the paper had an important additional advantage. It made him fully aware of the nature and progress of the x-ray program, and of the problems being encountered. This was particularly helpful because Skinner managed the finances of the division, shifting funds as necessary to meet changing requirements. Interestingly, he controlled the money by the use of a small package of 5 x 7 cards, each reflecting the funding status of a division project. So simple and effective was this system that the author later adopted it for his own use in managing his divisions, which employed an accountant to cope with the hundreds of detailed computer print-out sheets that soon dominated the budget and accounting system.

While the current-balancing system of ionization current measurement continued in use in the x-ray laboratories for many years, the string electrometer was gradually replaced by other null-indicating devices when convenience and circumstances indicated the need. One of the first of these was the vacuum tube amplifier, which used a special tube made by the General Electric Company known as the FP-54 (see photo No. 11). It was the first of such tubes to be made and was indeed a great asset to measurement technology. Some 5 years later, it was replaced by vacuum tubes made by the Victoreen Instrument Company, which were very small in size, on the order of 1 cm in diameter and 2 cm long. Actually, both the FP-54 and the Victoreen electrometer tubes could be used in systems measuring the currents through a very high resistance--sometimes up to 10^{15} ohms. Originally these resistors were just an ink line on a piece of paper, but were not reproducible nor reliable. It again remained for Victoreen to develop a family of these high resistors which could be manufactured in quantity and which provided for a wide range of reliable current measurements. This technique for current measurement was useful for a great many purposes, since in an ionization chamber it was not necessary that the collector electrode reach a potential very different from zero, the basic requirement for a null system.

A later device of high sensitivity and stability was the "vibrating-reed electrometer," based on the generating voltmeter principle developed by Ross Gunn at the Naval Research Laboratory in the early 1930's. It also had the capability for being usable in remote locations with respect to the ionization chamber. A difficulty, however, was that the central part of the instrument cost about $1,000--a sign of the times. As the apparatus and instruments became easier to read and more centered around electronics, they became vastly more expensive.

With the free-air standard ionization chamber completed and tested, it was next necessary to make intercomparisons between the Bureau of Standards and the European Laboratories. If possible, this should be accomplished before the next meeting of the

International Congress of Radiology, which was to be held in Paris in the summer of 1931. Also, the Standardization Committee of the Radiological Society of North America was anxious to demonstrate that the radiologists in this country were indeed working within the accepted standards system.

An obvious method to achieve an intercomparison would have been to follow Behnken's earlier procedure employing an electrometer and thimble chamber, but possible changes in the calibration would have been difficult to evaluate. However, the ideal procedure was to actually move the standard free-air chamber itself, but this would have been an extremely difficult, awkward, and impractical task.

Fortunately, Taylor, while visiting a laboratory of the NBS Electrical Division, observed the work of Drs. H. B. Brooks, F. B. Silsbee, and F. M. Defandorf, who were experimenting with an absolute attracted-disk electrometer for absolute high-voltage measurements of alternating currents* (Brooks, 1938).

*The electrometer was being designed to measure approximately 100 kV rms, with the high potential plate at one end and a movable disk at the other end attached to a sensitive balance. This would measure the actual force of attraction between the high-voltage plate at the top and the grounded plate at the bottom. A 3-foot separation between the two plates meant there would be uncertain errors in the measurement because of the stray and bulging electrostatic field between the two plates. The bulging field problem was solved by Dr. Chester Snow, also of that Division, by mounting a series of disks with center holes between the two plates. By placing a resistor between each of the remaining disks, which were some 3 or 4 inches wide, the potential was uniformly graded from the high-voltage end to the lower grounded end, thus providing a uniform electric field down the center of the apparatus.

Taylor saw an immediate application of this concept to the design of an ionization chamber, with the use of wires instead of sheets around the edge of the electric field. It was thus possible to make an ionization chamber system with a 5-cm collector electrode, two 5-cm guards, and a high-voltage plate 15 cm square (see photo No. 12). Furthermore, it was now possible to place the necessary lead shielding around this assembly, only an inch or so away from the electrodes. Fine aluminum wires were strung around the open faces of the ionization chamber plate system and the potential between the wires graded by a voltage divider placed on top of the chamber. These modifications reduced the overall size of the chamber by a factor of 3 or 4 in most dimensions, and its weight was reduced to something that could be readily moved about with one hand.

Using probes with various potentials applied in a manner similar to that used by Failla, it was possible to verify the adequacy of the field correction. When completed, the chamber was compared with the original free-air ionization chamber of much larger dimensions and found to be in good agreement up to 180 kV (Ref. 13). (In the early 1950's (Kemp, 1953) it was discovered that agreement at the higher tube voltages varied by as much as 2 or 3 percent.)

Steps were immediately taken to develop the remaining equipment necessary for the intercomparisons being planned with the National Physical Laboratory in Teddington, the Physikalisch-Technischen Reichsanstalt in Berlin, and l'Hôpital St. Antoine in Paris. The principal need was a portable current balance system, using an electrometer that could be calibrated at any point during the trip. This was accomplished with a string electrometer mounted on a 2 1/2- x 8-inch aluminum box containing batteries, the standard capacitor, and the potentiometer for the current balancing system (see photo No. 13). For the chamber itself, a beam shutter system was required that could be electrically timed with a combined switch and stopwatch system. An electrically operated switch in the lead between the collector plate and the electrometer was provided in case it was not feasible to use the timed shutter system. Because the specific arrangements at the national laboratories for mounting the chamber were unknown, two sets of tracks that could be mounted on collapsible surveyor's tripods were constructed (see photo No. 14). Finally, a 2,000-dc volt source, required for the ionization chamber plate, was contained in an aluminum package about 4 x 6 x 8 inches. (In 1931 the components were not catalogue items.) All units could be contained in three boxes, two of which could be carried by one person, the other by two (Ref. 22). The intercomparison plan called for a 3-week stop in England, in Germany, and in France; after which Taylor would report the results to the Paris meeting of the International Congress of Radiology (Refs. 22 and 24).

The first comparisons were made at the National Physical Laboratory in Teddington where the NBS apparatus had arrived via ship and train (total weight: 2 hundredweight, 6 stone and 3 pounds) without mishap or detectable change in calibration.*

*At that juncture, Taylor met a new figure in the field, Walter Binks. All earlier dealings had been through the Division Director, Dr. G. W. C. Kaye. Thus, a long professional friendship began between Taylor and Binks which continues today.

Within a short time the NBS apparatus was set up on the NPL optical bench with the NPL Standards and a few immediate comparisons at about 100 kV showed close agreement (see photo No. 15). However, the next day different components of the two systems were intercompared and found not to be in agreement. The largest single difference involved the determination of the cross sectional area of the NPL diaphragm on the ionization chamber which defined the cross section of the beam. Each diaphragm was approximately 8 mm in diameter. The NBS aperture had a very slight taper conforming to the approximate spread of the beam and had been burnished and measured with a taper gage. The British aperture had simply been drilled in lead. When the error attributed the aperture measurement and several other small differences were corrected, agreement between the two standards was on the order of 1 percent. At that time this was considered to be adequate, even though the highest voltage then available at Teddington was only 143 kV.*

*Some 25 years later, after a great deal of experience and using more sophisticated measuring equipment, H. O. Wyckoff and his associates at the Bureau of Standards found that the small guarded field chamber, which Taylor had used in 1931, agreed satisfactorily only up to about 160 kV. At that point it began to read low, reaching a discrepancy of some 2 percent at 200 kV, the maximum energy for which the chamber had been designed (Ref. 225).

The next stop was at the Physikalisch-Technischen Reichsanstalt (PTR) in Berlin where the NBS apparatus arrived in good condition by train, following an exceedingly rough channel crossing. On arrival at the laboratory, Taylor learned that the pressure chamber being used by Behnken at the time of his NBS visit in 1928 had been replaced by a large cylindrical ionization chamber roughly 30 cm in diameter and some 60 cm long.* (See photo No. 16.)

*Apparently, by that time Behnken had discovered the deficiency in ion collection in the pressure chamber, although this was not mentioned. Such a deficiency had not occurred to Taylor either, and did not come to his attention until the mid-1930's when he began to work with a much larger pressure ionization chamber operating up to 10 atmospheres.

After the usual component comparisons, agreement between the NBS and PTR chambers was close from the outset. Using voltages up to 180 kV, the final differences were a little more than 1/2 percent, whereas standards themselves were probably no better than on the order of 1 percent.

There was an interesting difference in the operating principles of the PTR and NBS current measuring systems. Behnken was using two kinds of uranium oxide current compensators. One of these consisted essentially of two fair-sized parallel plates with coatings of uranium oxide on the facing sides (Behnken, 1927). By changing the voltage across the plates, the ionization current could be varied to compensate that from the ionization chamber. Once the uranium oxide chamber had been calibrated, this technique was a very convenient way to measure the current. Behnken's second scheme used two sets of plates which could be slid in and out of each other, thus varying the volume in which ionization would occur. This allowed the instrument to be calibrated in terms of the current at various positions of the sliding plates.*

*Upon his return to Washington, Taylor tried the fixed plate method but the current yield of his chamber varied very slightly with time in an erratic manner. He also tried a similar source using a large iris diaphragm to control the volume of air ionized which also proved to be erratic. Both schemes were abandoned. Failla (1935), in New York, had tried a similar scheme using a very small amount of radium in a tightly sealed ionization chamber which he also eventually gave up as impractical.

The x-ray standard situation in Paris was completely different from that at the NPL or the PTR. Since France did not have a central standardization laboratory, the standards were

maintained by Dr. Iser Solomon at l'Hôpital St. Antoine. The x-ray source was one of the hospital's clinical therapy units which had to be taken out of clinical use during the intercomparison measurements. Since the beam could not be brought out of the tube container horizontally, the measurements had to be made with the ionization chamber track at a slope of 40°. There was no provision for a suitable diaphragm system so one had to be rigged up at the time (see photo No. 17).

It was not practical to change the position of the NBS free-air ionization chamber once it was in place for the measurements. This required that the Solomon chamber and the free-air chamber be exposed and read at the same time with enough space between them to avoid any undesirable scattered radiation. Since the two systems read in entirely different units, only a ratio between the free-air and the thimble chamber measurements could be obtained. This was on the order of 2.2 at energies up to 190 kV.

Following the comparisons, Dr. Solomon announced his acceptance of the open-air ionization chamber as his fundamental standard. Later the Bureau made and sent him a duplicate of the chamber used in Paris (Refs. 41, 22, 24, 26, and 48).

While still in Paris, Taylor attended the Third International Congress of Radiology and presented a report of the comparisons to the Congress as well as to the X-ray Units Committee of which he was a member. The success of the intercomparisons prompted Taylor to propose that the NBS ionization chamber be adopted as the International Standard. This was obviously premature and the proposal was withdrawn. (However, the guarded field principle has remained in use up to the present time.)

An important result of the international comparisons was a set of agreements and joint recommendations by the Bureau of Standards, the National Physical Laboratory, Physikalisch-Technischen Reichsanstalt, and the Le Service D'Etalonnage de l'Hôpital St. Antoine. These outlined the general principles to be followed in the standardization of thimble ionization chambers in terms of ionization measured free in air (Ref. 41).

The x-ray standards problems were obviously most critical in the energy range from some 50 to 200 keV. However, even at this time there were two other ranges being explored, one in the very low energy region from 5 to 20 keV, commonly referred to as "grenz rays," and the other in the higher energy direction from 200 keV and up. The first of these ranges to be examined in terms of standardization measurements was in the grenz-ray region.

Because grenz rays are of such low energy, they are used primarily for superficial therapy and, as such, are delivered in fairly high skin doses since the radiation is strongly attenuated in tissue. Also, air absorption is very important. Whereas in the 200-kV range, air absorption corrections applied to standard chamber readings were on the order of a fraction of a percent, in the grenz-ray region they could run as high as 50 percent. Moreover, because of the absorption variation along the path of the beam, it was essential to measure the absorption for exactly that path length and position of use along the beam. Absorption in 10 cm of air measured close to the tube could be substantially different from the absorption in 10 cm of air 30 cm from the tube. Also, within the ionization chamber itself, the absorption along the path of the beam between the limiting diaphragm and the collector electrode has to be determined for the exact position of the diaphragm-electrode system with respect to the x-ray tube target.

The quality of the grenz rays could also change radically with type of tube. For example, the window of the tube, through which the grenz rays passed, had to be of some low absorbing material and as thin as possible. Early attempts to use beryllium windows were unsuccessful until the technological advances of the late 1930's, when satisfactory high-vacuum beryllium window tubes were made for grenz-ray purposes as well as for x-ray crystal structure analysis. The tube most often used by the Bureau was made by Westinghouse, employing the Slack bubble window (Ref. 30). (This was a re-entrant bubble window which was extremely thin, but because it acquired its shape under pressure while hot, it held that shape with considerable tensile strength when the tube was evacuated.)

The standard ultimately constructed for measuring grenz rays followed the free-air chamber design, but its dimensions were only 5 cm on a side. Four rings of guard wires were used instead of 10 for the larger chamber, and the voltage divider was again placed on top of the assembly. The electrode assembly was mounted so that it could slide back from the diaphragm to determine air corrections, which were obtained by measurements at different positions of the electrode within the containing box (see photo No. 18).

The grenz-ray standard chamber could be used at energies as high as 40 or 50 keV which overlapped the lower end of the range of the 200-keV standard. Thus, it was possible to make direct comparisons between the two and to prove the correctness of the grenz-ray chamber system. Similar systems were used in a number of other laboratories but grenz-ray techniques are rarely used today.

By the mid-1930's, the high-voltage and x-ray tube technology had advanced to the point that it was possible to produce x rays at energies of a million keV. Though these sources were being developed primarily for nuclear physics studies, it was clear that they would have applications in the field of radiology. One of the first such tubes was built by C. C. Lauritsen at the California Institute of Technology (Lauritsen, 1928). At the same time, the General Electric Company and the Kelly-Koett X-Ray Company were moving to higher energies, and x rays on the order of 400 kV, produced by reliable sealed-off tubes, were becoming routine.

Since it was obvious that there would soon be a demand for x-ray standards for higher energy radiation, the Bureau decided to lead the way in this respect rather than come from behind as in the past. This meant prompt moves in two directions at once. The Bureau had to acquire a high energy source (to be discussed later) and develop the necessary high energy x-ray measurement system capable of measuring radiation energies up to at least 1 1/2 MeV, into the lower energy range of the gamma rays from radium.

Until then, gamma rays were measured only by means of cavity ionization chambers or thimble chambers. There were, however, widespread uncertainties in such measurements and disagreements as to their interpretation.

One approach would have been the use of a very large guarded-field ionization chamber based on the same principles used for chambers operating at lower energies. Calculations and some exploratory measurements indicated that such a chamber had to be a cube some 12 feet on a side and carefully shielded from electrostatic or other such influences. Because this was definitely impractical, another approach was necessary.

Early in the 1930's, Taylor and Mohler had investigated the problems of columnar ionization and recombination as reported by Jaffé. Though Jaffé had worked with the properties of ionized liquids, the problem was comparable to results that might be obtained in dense air as, for example, air under high pressure. From a study of his work, it was shown that allowance for the loss of ionization due to recombination could be obtained by an extrapolation method when true saturation could not be reached. Plotting the reciprocal of the ionization current against the reciprocal of the applied voltage should yield a straight line at the higher field strengths. This could be extrapolated to "infinity," thus yielding the value that would have been obtained for saturation. The approach showed enough promise for Taylor to proceed with some preliminary designs for a pressure ionization chamber of fairly large dimensions.*

*The problem as usual was cost, the solution to which came after an interesting bit of by-play between Taylor and Dr. Skinner, his Division Chief. Skinner was less than enthusiastic with the pressure chamber concept and suggested that Taylor find a place to set up and try the large open free-air chamber. After failing to find space on the Bureau grounds, Taylor proposed to Skinner that the nearest suitable space was a medical installation in Lincoln, Nebraska, where he (Taylor) had made x-ray absorption curves up to 600 kV using a thimble chamber. Skinner suddenly favored the pressure ionization chamber idea, developed a persuasive case for it, and came up with some money to launch the program.

The biggest single item of cost was the pressure cylinder for containing the ionization chamber. This consisted of a tank with one removable end and a total inside length of 7 feet and diameter of 2 1/2 feet. The most complicated part of the complete system was the ionization chamber itself which had to operate within the tank, to work at various plate separations and at various positions along a track inside the tank, and so arranged that the width of the collector electrode could be varied. All of these requirements had to be met because it was not known what the optimum sizes and positions would be for any of them and it had to be possible to verify the significance of any measurements made. Fortunately, the ionization chamber system was made in the Bureau's Instrument Shop, a truly fine piece of engineering by George Rheinbold, at no cost to the laboratory project (see photos No. 19 and 20).

By means of pressure-tight controls leading through the tank, it was possible to vary the plate separation from approximately 5 to 40 cm. The collector electrodes could be varied from 5 to 25 cm, bringing them very close to the guard wires so as to evaluate the uniformity of the electric field between the plates (Ref. 62).

Of course, to properly utilize this pressure ionization chamber, it was necessary to acquire a high energy x-ray source. How the building of a 600,000-volt x-ray generator was achieved will be described in Chapter 11.

Initial studies indicated that the feasibility of the pressure chamber measurements and the concepts proved to be reliable. However, it was not as simple to make a meaningful ionization measurement, as with the completely free-air chambers operating at one atmosphere pressure. For every point determined, a series of measurements had to be made, plotted, and then extrapolated. Reproducibility was good but it was not until 1940 that the chamber would be used in conjunction with a one and a half million volt x-ray source, thus putting it to its fullest test.

Meanwhile, another line of measurements was undertaken, partly as a test of the utility of the pressure chambers, and partly as a means of comparing the measurements of gamma rays made with a cavity chamber, against those made with a pressure chamber under free-air conditions.

In measuring gamma rays, it had become customary to express the emission from gamma rays in terms of the number of roentgens at 1 cm from 1 mg of radium shielded by 1/2 mm of platinum. This was sometimes referred to as Eve's Constant, after A. S. Eve, who had used such a figure in Rutherford's Laboratory. Though various measurements of this constant had been made using different kinds of ionization chambers under different conditions, none had been made under free-air ionization conditions. Therefore it was hoped that by measuring radiation from a suitable radium source, not only could a check of Eve's Constant be made, but also a check on the air ionization chamber for measuring gamma rays in terms of roentgens, a very unsettled question in the mid-1930's.

To accomplish this, a special source of about 500 mg of radium was obtained on loan from one of the radium companies and was contained in a right platinum cylinder 0.5 cm long by 1 cm diameter, with flat ends 1/2 mm thick. This in turn was contained in an elaborate collimating system to prevent stray radiation from reaching the measuring volume in the pressure ionization chamber. To avoid the handling of large weights, a new kind of shielding system was introduced. This consisted of various containers with any desired shape and thickness which could be filled at the top with very fine lead shot and drained at the bottom when it was necessary to make some structural change. Several tons of #12 lead shot were used for this purpose; it was a manageable and effective method of dealing with the situation (see photo No. 21).

To protect the personnel from the radium source as well as from the beam as it passed through the chamber and air space, the current measuring system was changed from the simple capacitance compensator and string electrometer to the use of an FP-54 electrometer tube supplied by the General Electric Company (see p. 41). By means of this general system, it was possible to shield the standard capacitor and the electrometer tube, which were small in volume, whereas to shield a person it would have been impractical. Having demonstrated that the FP-54 made a satisfactory electrometer, most of the measuring equipment thereafter employed the electronic, rather than the electrostatic system, of null indicating. This continued until sometime after World War II, when the vibrating reed electrometer came into being.

There were several major sources of difficulty in the measurement of gamma rays. The right cylinder containing the radium was about 1 cm in diameter and 1 cm along its axis. The axis of the radium capsule lay along the axis of the diaphragm system and the ionization chamber. Corrections had to be made for the absorption of the gamma radiation by the radium itself. These corrections were both calculated and also measured experimentally, with reasonably close agreement.

There was also the problem of beam divergence, and therefore a determination of what really constituted the cross section of the beam which was allowed to enter the ionization chamber. The accuracy of the measurement was directly proportional to the accuracy of the cross sectional area of the aperture, but with a divergent beam some radiation could pass through the edge of a cylindrical limiting diaphragm, which in this case was 15 cm of lead. The problem was not avoided by a tapered diaphragm because, at the distances involved, the 1-cm-diameter radium capsule could not be regarded as a point source. Also, corrections needed to be made for radiation scattering from the inner surface of the diaphragm as well as from the shielded platinum capsule of the radium. The radium capsule was contained in a thin copper tube leading through the periphery of a garbage can to the radium at the center of the can, which was filled with lead shot (see photo No. 21). The method developed by the x-ray group to construct the limiting diaphragm hole in lead, 1 cm in diameter and 15 cm long, that was perfectly round and perfectly straight, turned out to be useful to the NBS Gage Section when it was later challenged to measure the cross sectional area of small orifices. The method was to employ a strong x-ray beam located at some distance from the diaphragm, and compare the x-ray transmission through the 15-cm hole with that through a diaphragm about 1/8 inch thick and of nominally the same measured diameter.

Despite all of the problems, the procedure appeared to be sufficiently accurate. After making the corrections, a figure of 8.16 roentgens per hour was obtained for the emission constant of the radium.

Much of the same general concern and reasoning was going on independently at the National Physical Laboratory in Teddington where Binks was using a large guarded-field free-air ionization chamber operating at atmospheric pressure. His chamber was on the order of 10 feet cubed, and located in the middle of a large room. It was also adjustable, as was the NBS apparatus, in order to determine the design characteristics for general purposes. Binks arrived at the figure of 7.9 roentgens per hour at a centimeter for the emission constant of a 180-mg radium preparation as the gamma-ray source (Kaye, 1937). Actually, it was not until some time later that Binks and Taylor learned about each other's work.

As it turned out, the evaluation of the emission constant was being made by five different laboratories, all reporting their results during 1938. Values ranged from 7.9 to 8.6 roentgens per hour at 1 cm.

Research and development efforts to achieve a well understood and highly accurate measurement of x rays continued. Beginning in the late 1940's, Dr. Harold Wyckoff, who had joined the section in 1941, and his associates Attix, DeLaVergne, and Ritz, studied the measurement problems intensively for the next decade. Using a large guarded-field free-air ionization chamber with about 1-meter plate separation, he and his co-workers performed very sophisticated studies that led to important improvements in standard chamber design and measurement. These will be reported later.

Diaphragm Systems

The principal purpose of developing a free-air ionization chamber standard for the measurement of x rays was for the basic purpose of calibrating the dosage measuring instruments used in hospitals or private practice, especially in connection with therapeutic applications of x rays. In making such calibrations, there were essentially three methods involved. The first was to measure the beam at a given point, move the standard, and then calibrate the thimble ionization chamber put at the position of the limiting diaphragm of the standard. The second was to measure the beam with both the standard and the thimble chamber simultaneously by placing them in different, but uniform, portions of the beam. In this case, it was necessary to make corrections for inverse square law. The third was to measure the beam with a standard and then replace it with the thimble chamber to be calibrated, but at some other distance, and again apply the inverse square law correction. The third method was essentially the same as the second except for some differences due to the scattering from the standard chamber.

As already noted, it was important to sharply diaphragm the beam entering the standard chamber so as to avoid uncertain radiation scattering, either from air or other sources that entered the measuring volume of the standard. Unnecessary radiation, especially off-focus radiation, would come from the face of the target, the x-ray tube outside of the focal spot, or from the back surface of the target or the target stem. These latter two had to be especially shielded from the standard chamber. This was difficult because, if the radiation entering the standard chamber was too carefully restricted, it would often peak at the center and fall off rapidly to either side. Under these circumstances the portion of the beam subtended by the thimble chamber would be non-uniformly exposed; in other words, the thimble chamber would see a different beam, or sample of the beam of radiation, than that subtended by the standard chamber.

To overcome these difficulties, it was necessary to have a beam adequately shielded from stray radiations, and at the same time, sufficiently uniform across the beam so that the thimble chamber saw the same radiation flux as the standard chamber. Though this could be accomplished with adequate attention, it was frequently overlooked. For example, if the thimble chamber and standard chamber readings were made simultaneously, care had to be taken that both were in a uniform part of the beam.

Another difficulty in the simultaneous measurement plan could be introduced by use of the inverse square law, which could only be applied when the geometry was such that the radiation source was essentially a point in comparison with the other distances involved. This was not usually the case for x-ray calibration setups, except when the source could be located at the position of the restricting diaphragm nearest to the target. This was a point frequently overlooked, and yet one which could make a difference of several percent in the calibrations (Refs. 8, 11, and 18).

Another difficulty, not encountered at the Bureau but sometimes encountered elsewhere, was the result of the x-ray beam being calibrated in one direction with respect to the

target face while the thimble chamber was calibrated in quite a different direction. Under such circumstances the radial distribution of radiation intensity around the face of the target varied considerably with angle and could lead to gross errors. Such measurement conditions often occurred in clinical applications.

The problems of thimble chamber calibration and diaphragming systems were studied in some depth (Refs. 11 and 18) during the early 1930's. Later studies by Wyckoff (Ref. 519a) repeated some of the earlier studies and extended them further using the more sophisticated equipment and understanding of the problem that was available in 1956.

Programs (1946-1960)

Since the prime reasons for the existence of the Radiation Physics Laboratory or Division were the study of x-ray measurements and protection procedures for the radiological profession, the post-war phases of those programs will be dealt with in some detail. While most of the division's programs related directly or indirectly to those subjects, encouragement was given to pursuing other researches of both a theoretical and experimental nature.

Wyckoff, in picking up the program following Singer's death in 1946, realized that the high-energy protection and measurement program had been forced temporarily to a lower priority. Also in the lower energy range--below 200 kV--there had been no advances since the early 1930's, when the guarded-field ionization chamber was installed as the NBS primary standard. Meanwhile, exploratory measurements by Taylor and Singer with the high-pressure chamber indicated the possibility that not enough was known about the design details of the low and intermediate voltage free-air ionization chambers.

To obtain answers to these questions, Wyckoff set up several teams of young physicists to study the design characteristics of the whole family of free-air ionization chambers. These studies were carried out by F. S. Kirn, R. J. Kennedy, W. Miller, F. H. Attix, L. DeLaVergne, and others.

In 1950, Dr. G. E. Roth, from the national laboratory of New Zealand, carried out an informal comparison of primary standards in a number of laboratories. For the purpose he employed a Victoreen R-meter, using a radium source to correct for any deviations in the sensitivity that might occur during transport. Discrepancies as large as 10 percent were found. The National Physical Laboratory (NPL) and the National Bureau of Standards (NBS) were the only laboratories using free-air ionization chambers which had been intercompared, and that had last occurred in 1931 (Refs. 22 and 24). Roth's measurements, while not being taken too seriously, indicated a difference of some 3 percent between the NBS and NPL measurements for energies in the 200-kV range.

J. W. Boag, in early 1953, found similar discrepancies, again using a secondary instrument that had been calibrated at the NPL and NBS. These discrepancies prompted L. A. W. Kemp (1953) in England to investigate the dimensional requirements for standard chambers. He came to the conclusion that both the NPL and NBS chambers should read a few percent low at the higher energies. With this background in mind, Wyckoff arranged for a new comparison to be made between the standards at the NPL and the NBS. Meanwhile, he designed a larger ionization chamber, permitting plate separations up to 20 cm (see photo No. 22). The collector plates were roughly 27 cm tall and 18 cm wide, making a total length of the plate assembly of about 54 cm. This was surrounded with the wire guarded-field system used in the earlier standards (Ref. 13).

Using this chamber, the NBS working standard gave values approximately 2 percent less with the deviations in the direction predicted by Kemp. It was also found that a plate separation of 15 cm in the new chamber would be adequate for 250-kV radiation filtered with 0.5 mm of copper, whereas the 1931 chamber with a plate separation of 12 cm read 2 percent low.

The new chamber was substantially larger and, since it was surrounded by lead, was very much heavier. Nevertheless, Wyckoff did, in fact, take it to the NPL in Teddington where new comparisons were carried out in the summer of 1953. After making the proper corrections for air absorption between the NPL and NBS chambers, there was satisfactory agreement between the two laboratories. As it turned out, if narrow beams of x rays were used in the region of about 170 keV, the 10- or 12-cm plate separations in 1931 were found to be adequate (Refs. 225 and 281). Upon completion of the measurements in Teddington, Wyckoff proceeded on to Copenhagen to take part in the affairs of the International Commission on Radiological Units, of which he later became the Chairman for a period of over 10 years.

Following the 1953 intercomparison of standards, the International Commission on Radiation Units (ICRU) developed considerable interest in furthering intercomparisons

between other countries and the United States. One of the first of these was a comparison between the Swedish free-air ionization chamber equipment which was brought to the National Bureau of Standards in early 1956 by a Dr. R. Thoraeus (see photo No. 23). The Swedish chamber was a cylindrical type, 63 cm in length and 19 cm in diameter. The collector and guard electrodes were on a single axis located eccentrically but parallel to the axis of the chamber and also cylindrically shaped with a 2-cm diameter. Final results after all corrections were made showed agreement on the order of 0.5 percent. Earlier comparisons between the Swedish chamber and the German free-air chamber at Frankfort showed about the same order of agreement. This, effectively, provided a comparison between the United States and West Germany (Ref. 323).

The next set of comparisons took place in July 1956 between the French and U.S. standards. The French standard, used in the intercomparisons of 1931, had been replaced by a duplicate of the NBS guarded-field standard, which had, in turn, been replaced by an instrument of the Laboratoire Central des Industries Electriques (LCIE). Another primary instrument, whose characteristics were calculable, had been developed independently by A. Allisy and installed at l'Ecole Normale Superieure (ENS).

The 1956 comparison was carried out following the ICRU recommendation that the various primary roentgen standards be periodically compared. To accomplish the comparison of the ENS standard with those of other countries, Allisy had designed a suitable portable transfer instrument that required minimum set-up time at each laboratory and whose precision was on the order of 0.1 percent. During the period of July to October 1956, a series of comparisons were carried out, first at NBS in July, then in September at l'Ecole Normale Superieure, and again at NBS in October. Final agreements over the energy range of 60 to 200 kV were within 0.2 percent. By this time, it was clear that substantial advances had been made in the design characteristics of free-air ionization chambers. With proper attention to details, they could be readily made to meet any requirements in the low energy range--up to about 250 kV. Meanwhile, further studies by Wyckoff and his associates led to a better understanding of the measurement problems in the region above 250 kV. These included measurements by Miller and Kennedy of the distortion produced in guarded electrostatic fields by the closeness of grounded external plates.(Ref. 231). Additional studies of plate separation requirements for standard free-air ionization chambers were performed by Attix and DeLaVergne (Ref. 233), and of field distortion and free-air ionization chambers using analog methods by Miller and Kennedy (Ref. 279).

To obtain design parameters for a 500-kV free-air chamber, a guarded-field ionization chamber system was built following the pressure chamber design, with provision for varying the plate separation, collector length and height, and position with respect to the limiting diaphragm. Since it operated at one atmospheric pressure, its dimensions were substantially larger. The assembly fitted into a lead box 85 inches long by 40 inches square in cross section, the front of which was 1-inch lead; the sides, top and bottom 3/8-inch lead; and the back 1/4-inch lead. The limiting diaphragm was located on a snout extending about 24 inches from the front of the box. Tests showed, in agreement with findings at lower energies, that an internal scattering diaphragm was not needed. The collector and guard plates were made from a single sheet (90 x 90 cm) of colloidal-graphite-coated polyethylene. The collector (30 cm long x 80 cm high) was defined by lines scratched through the graphite with a sharp stylus. This in turn could be divided into a number of 5-cm strips, any or all of which could be connected to the ionization current measuring instruments. Field strengths across the chamber electrode system were on the order of 100 V/cm. Plate separations up to 80 cm were studied.

The simple one-plane guard wire system as originally designed by Taylor (see p. 42) was used in the 500-kV chamber. However, studies were also made using two sets of guard wires, which were, in effect, equivalent to a flat strip. In addition, guard strips were used. While each of these differed slightly, it was shown that any of the three could be used when proper attention was given to the positioning of grounded plates on the outside.

The final stage of free-air ionization chamber design was carried out by Wyckoff in the late 1950's (Ref. 519a). For this purpose, comparison measurements were made using cobalt-60 and cesium-137 gamma ray sources, with a modified pressure chamber and a special cavity chamber. This investigation included a very critical study of the Jaffé-Zanstra theory for obtaining saturation ionization currents under conditions where absolute saturation is not obtainable and can be reached only by extrapolation methods. (This was the principle that had been used earlier by Taylor and Singer, Ref. 62.)

Using a later extrapolation method suggested by Kara-Michaelova, it was found that, by proper choice of constants in the J-Z method, consistent saturation currents could be computed from ionization measurements made with non-infinite fields for pressures up to at

least 12 atmospheres of air. At the same time, other critical factors in chamber design, not considered before, were studied in detail. Though the free-air measurement of high-energy radiations had arrived at a satisfactory stage, the chamber designs were such that it was no longer feasible to move them between laboratories.

This situation prompted Wyckoff and his associates to consider the use of cavity ionization chambers, both as essentially primary standards as well as transfer standards. Acting on a suggestion by the ICRU during its 1956 meetings, NBS agreed to construct and calibrate a standards intercomparison system and make it available to national laboratories (Ref. 642). A second set of instruments was also constructed by NBS under a grant by UNESCO (see photo No. 24).

The new standards system consisted essentially of three parts. The first was a free-air chamber diaphragm which would be supplied to each laboratory for use during the test and against which its own diaphragms could be compared. As demonstrated as far back as 1931, (see p. 48) the accuracy of the cross-section measurement of the diaphragm area usually is the most critical factor in the free-air ionization chamber (Ref. 22). The second component was a special capacitor to permit the calibration of the laboratory's charge-compensating capacitor as it was being used. The third was a three-terminal cavity ionization chamber in which the collector electrode passed through grounded shields to the electrometer system. Grounded guards prevented leakage of high voltage from the cap to the measuring system.

Of special importance to this project was the work of Dr. Francis Shonka at St. Procopius College near Chicago. He had developed a very stable form of conducting plastic which had essentially the same effective atomic number as air, but which could be accurately machined and molded, and would hold its shape. Shonka's special plastic material was used in the transfer standards designed and constructed by Wyckoff. This work was drawn to a successful conclusion by further intercomparisons between four national laboratories utilizing the three-component system outlined above. A joint paper by Wyckoff, Allisy, Aston, Barnard, Hubner, Loftus, and Taupin in 1963 gave the results of indirect intercomparisons of standard ionization chambers where exposure-dose measurements in roentgens were carried out between the national standards of France, Germany, United Kingdom, and the United States. The estimated maximum uncertainty of the comparisons was about 0.5 percent. Also, the calibrations agreed with earlier results between three of the four laboratories to within the expected uncertainty of 0.5 percent (Ref. 606).

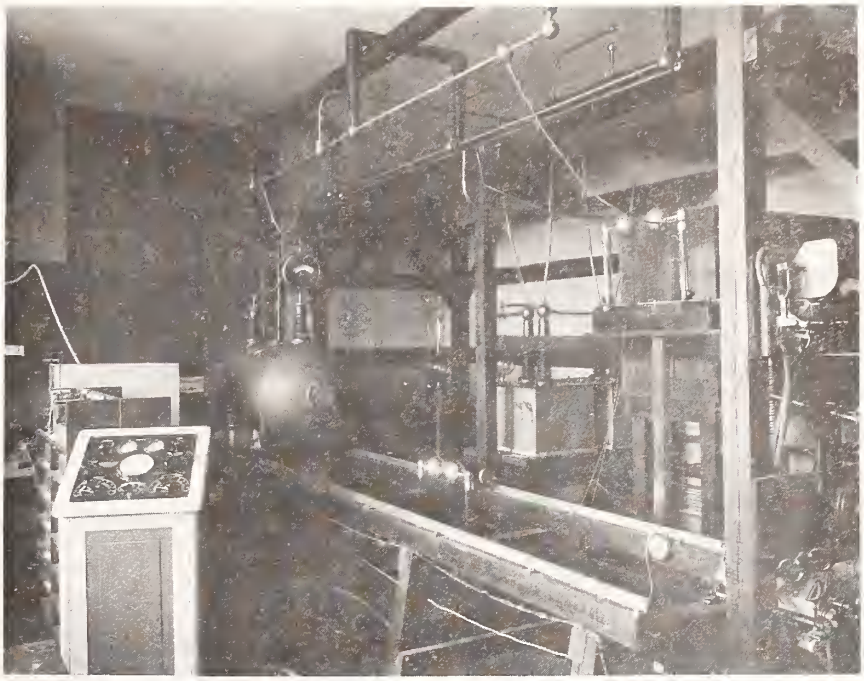

Photo No. 10. First NBS constant potential x-ray generator and x-ray tube shield (center background, 1928).

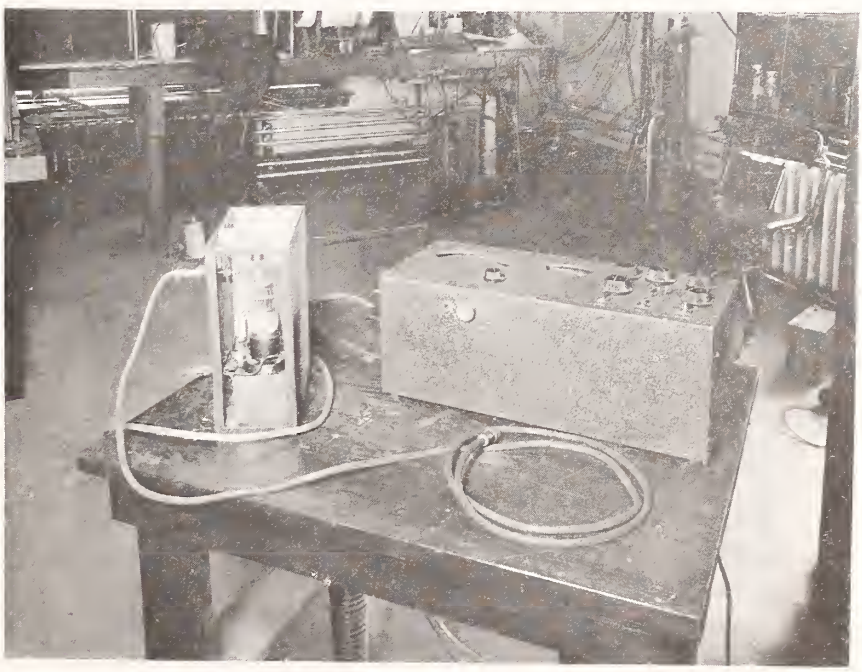

Photo No. 11. FP-54 electrometer tube amplifier (1934).

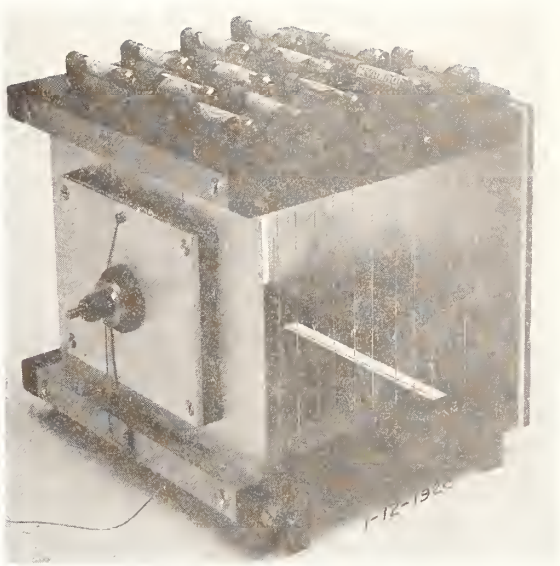

Photo No. 12. Electrode system for first guarded-field standard ionization chamber (1928).

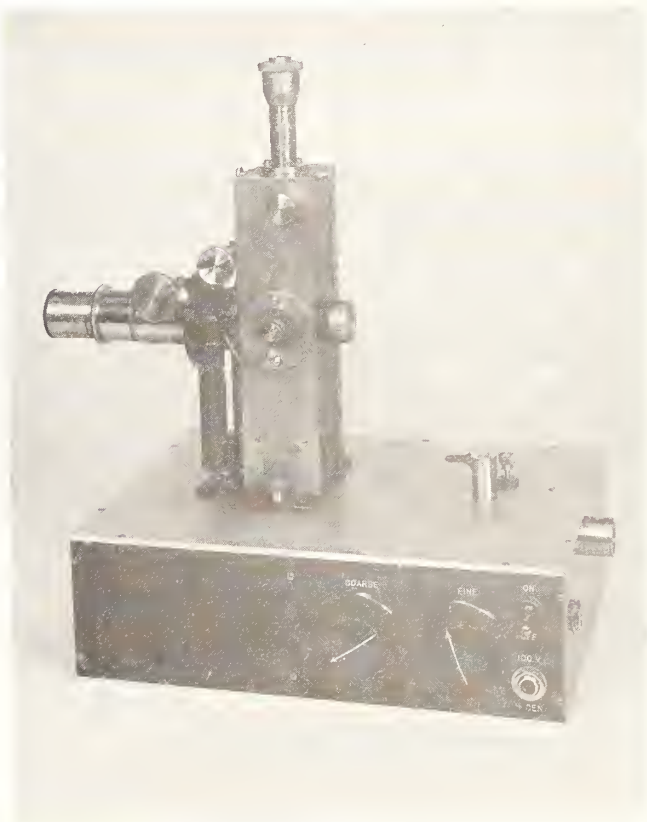

Photo No. 13. Portable, self-contained capacitance compensator for null measurement of ionization currents (1931).

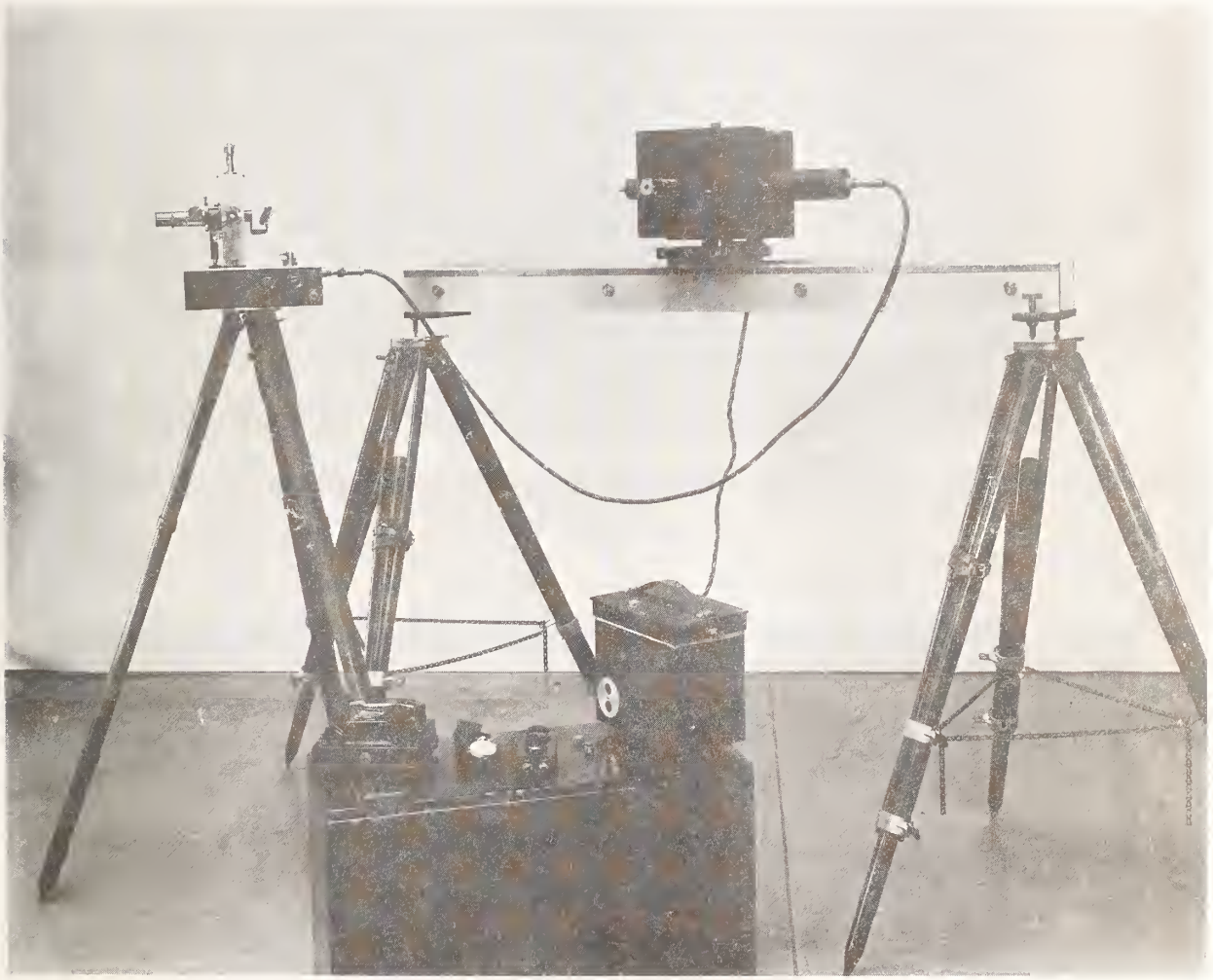

Photo No. 14. Complete, portable ionization chamber system used in international standards comparisons (1931).

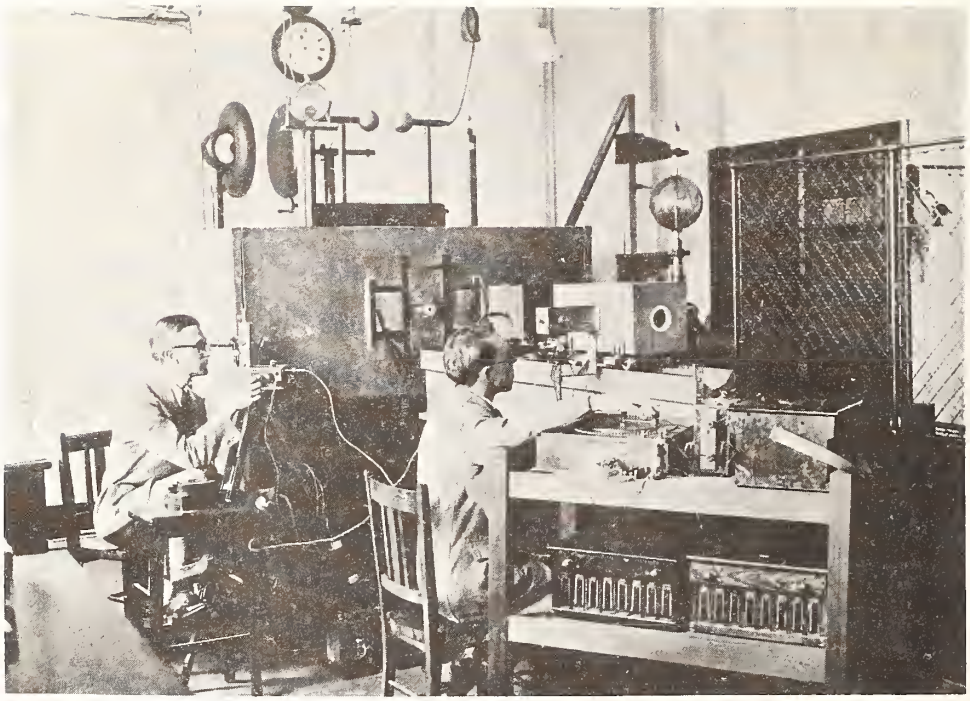

Photo No. 15. Intercomparison of British and American x-ray standards. L. S. Taylor and W. Binks (1931).

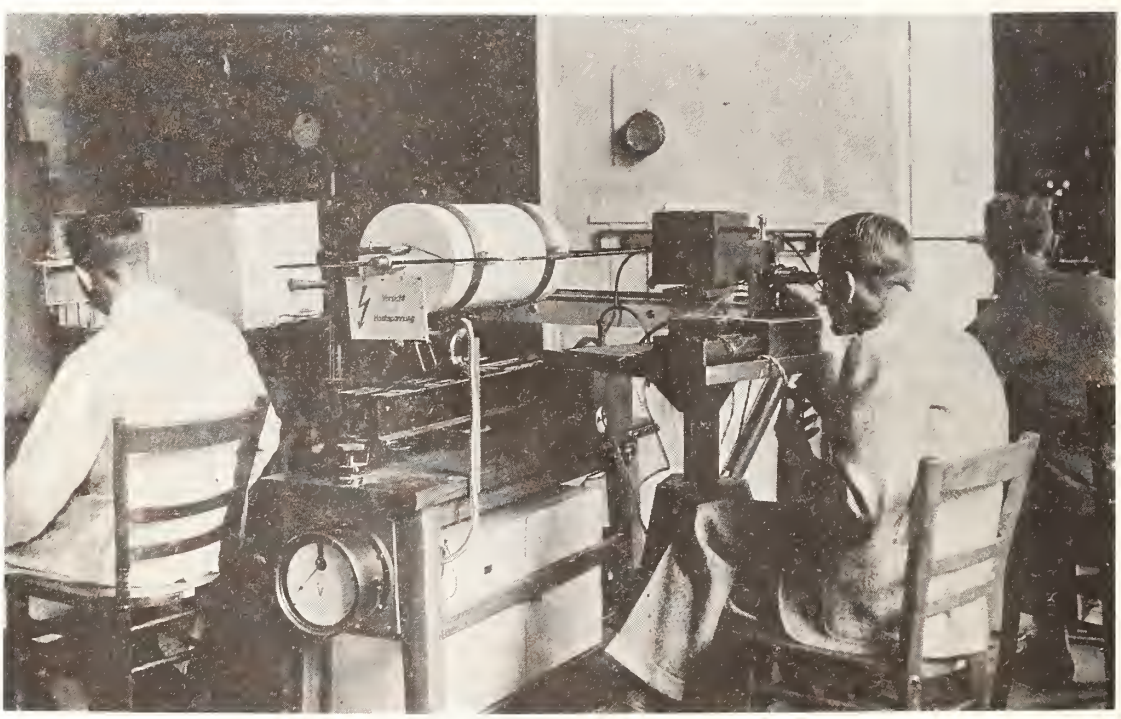

Photo No. 16. Intercomparison of German and American x-ray standards. H. Hermann, L. S. Taylor, and R. Jaeger (1931).

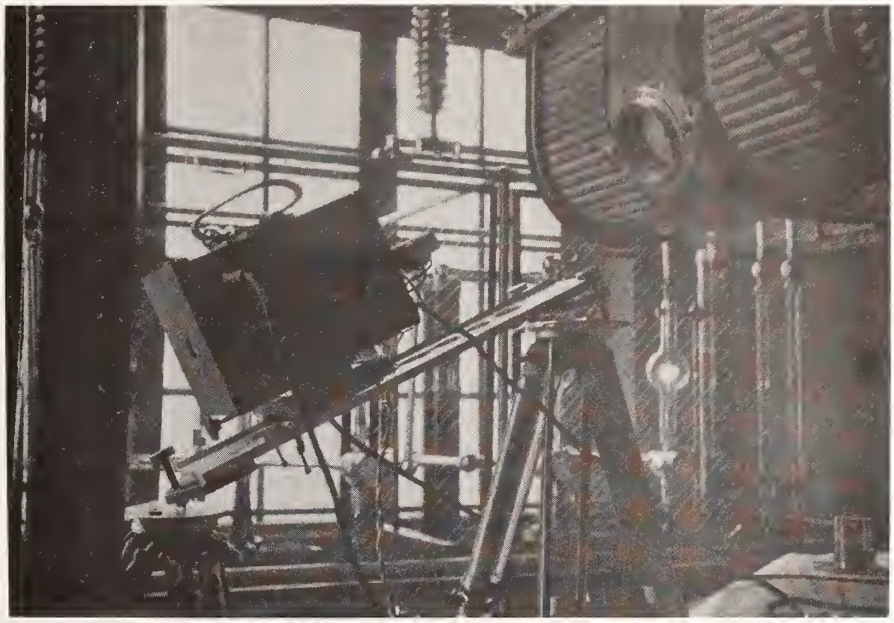

Photo No. 17. Intercomparison of French and American x-ray standards (1931).

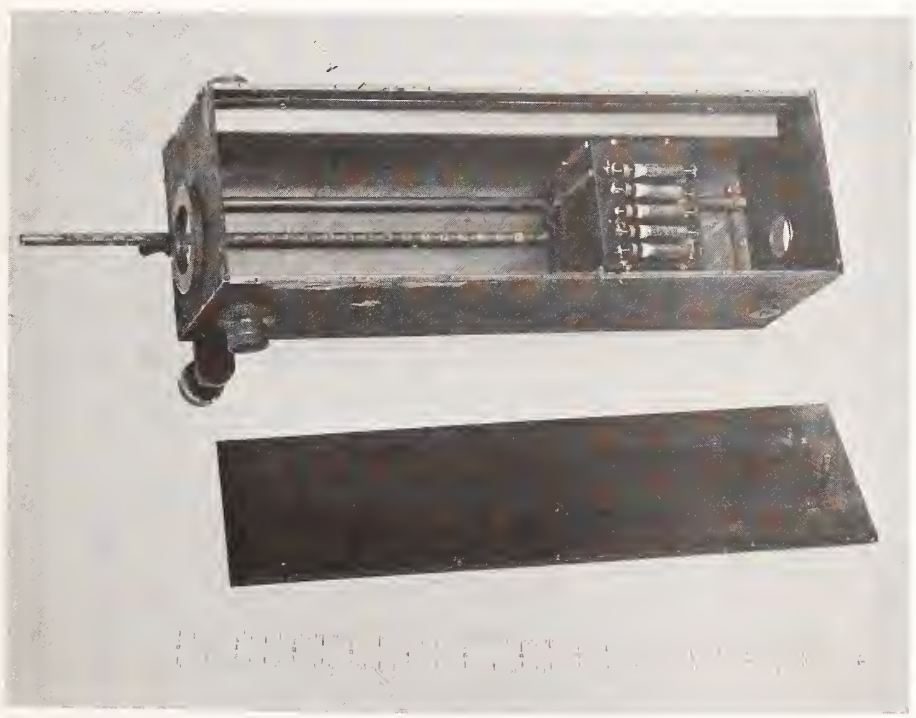

Photo No. 18. Guarded-field standard ionization chamber for measuring grenz rays (1932).

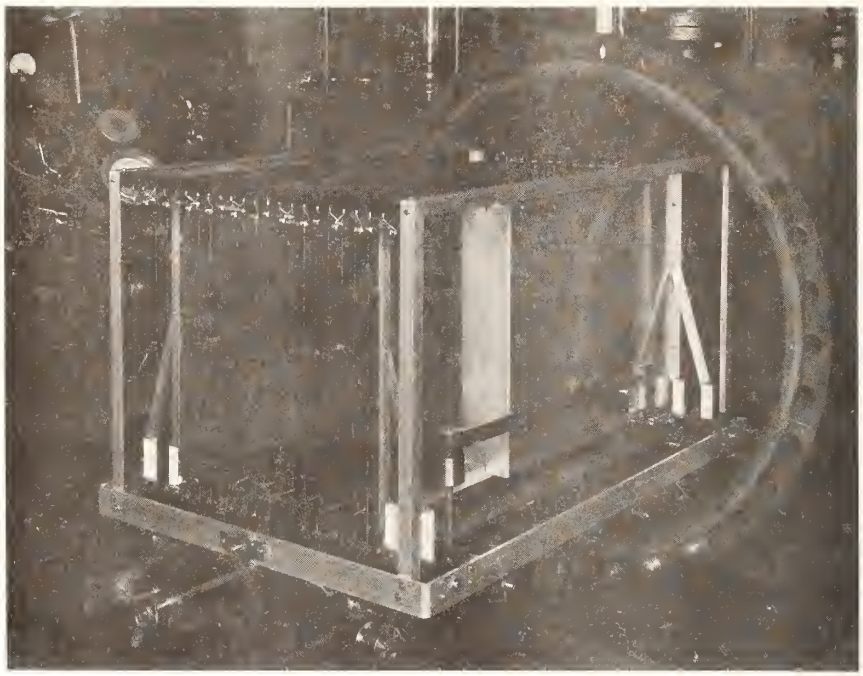

Photo No. 19. Adjustable, guarded-field standard ionization chamber assembly for use in pressure tank (1938).

Photo No. 20. Guarded-field ionization chamber installed in pressure tank. L. S. Taylor and G. Singer (1938).

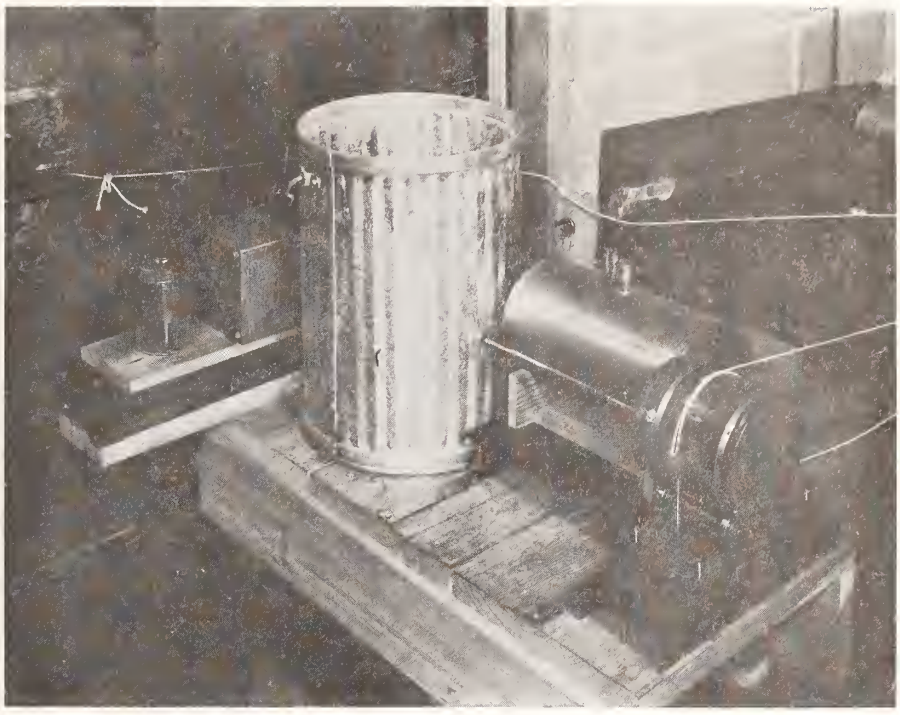

Photo No. 21. Lead-shot collimating system for radium gamma-ray beam (1940).

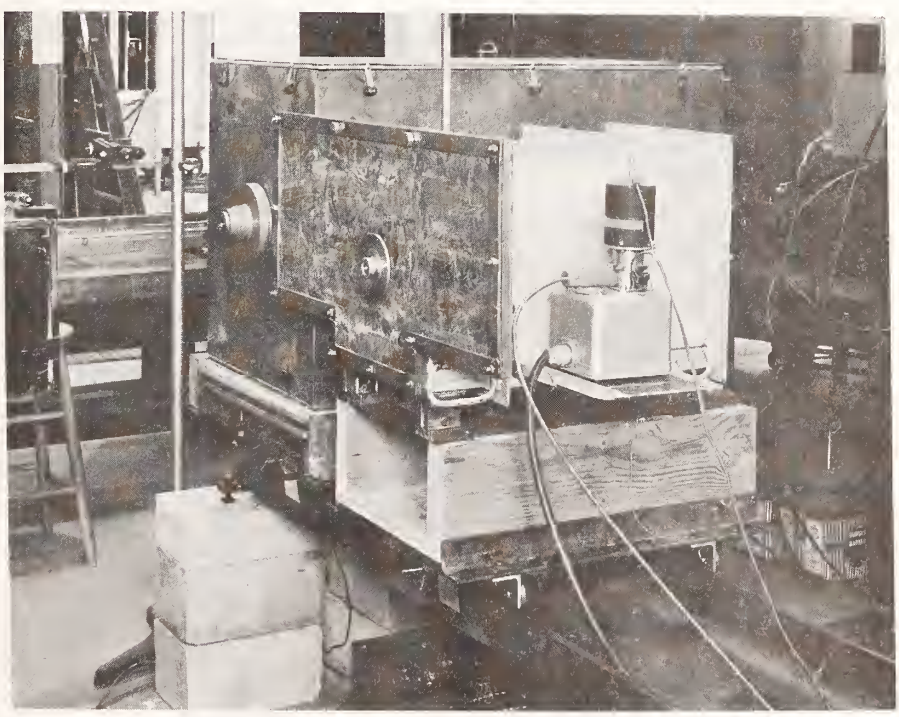

Photo No. 22. Intercomparison of British (rear) and NBS (front) x-ray standards (1955).

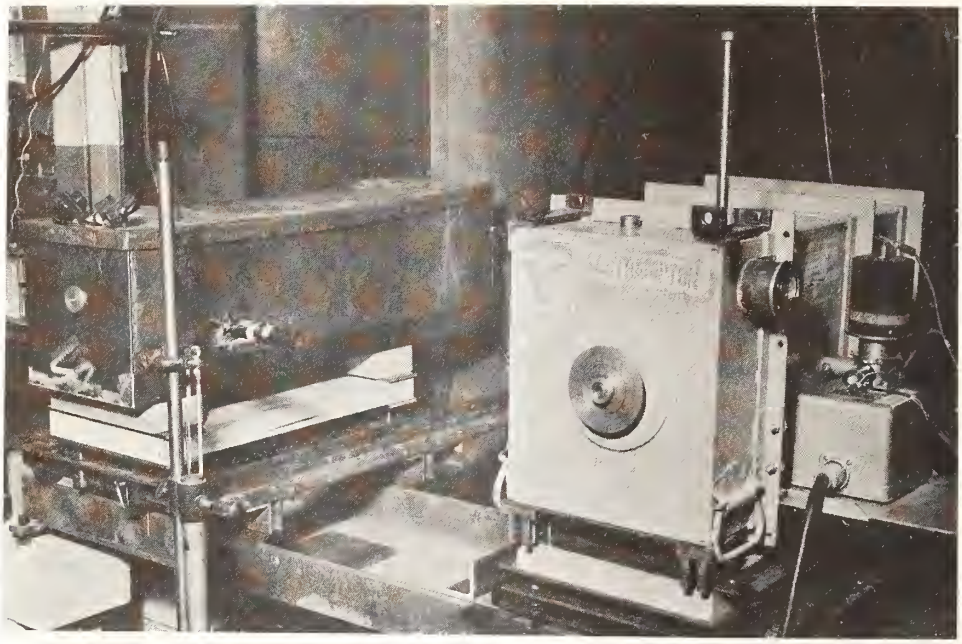

Photo No. 23. Intercomparison of Swedish (left) and NBS (right) x-ray standards (1956).

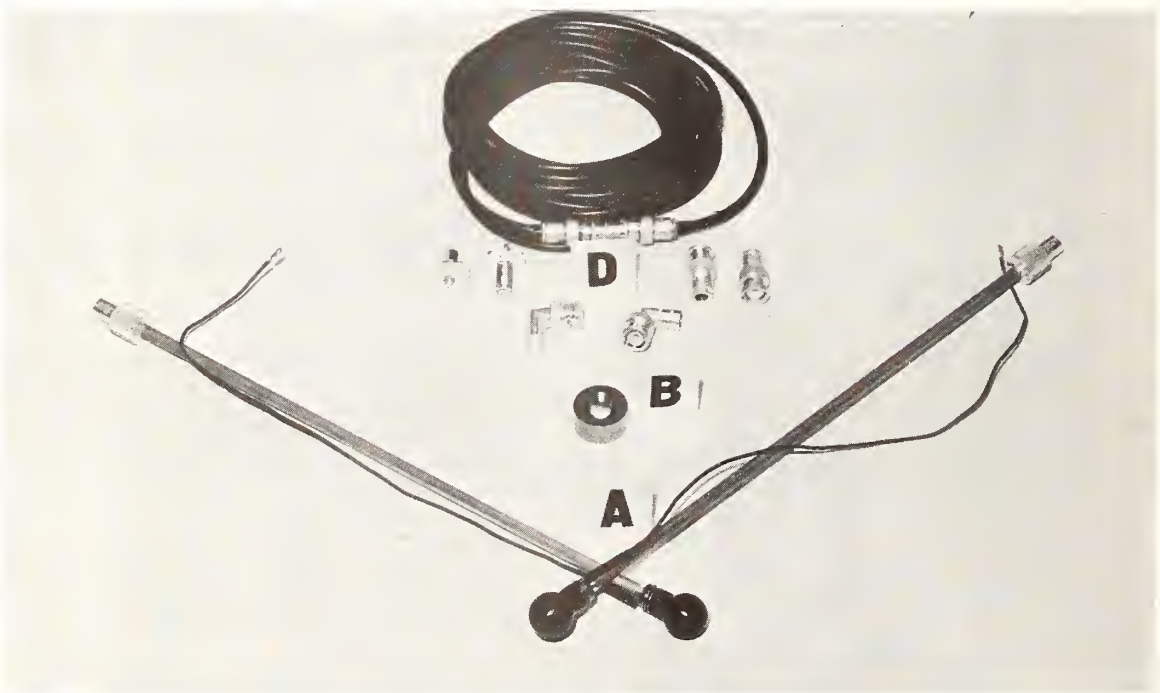

Photo No. 24. Cavity ionization chambers for interlaboratory comparisons (1957).

CHAPTER 6. RADIATION QUANTITIES AND UNITS

As previously noted, NBS and the RSNA began to have close ties with the International Commission of Radiological Units (ICRU) immediately after the Stockholm meetings in 1928 when Taylor was designated to replace W. Duane, while Ernst continued as the radiological representative. Problems before the ICRU centered more around measurement and standards than the basic understanding of the definition and limitations of the loosely defined quantity for which the roentgen was the unit. However, by 1934, when the ICRU was due to meet at the Fourth International Congress of Radiology in Zurich, radiotherapy was moving into the higher energy region. There was already concern as to the applicability of the roentgen to the measurement of the gamma rays from radium. The urgency for resolving these questions was further accented by the immediate need for radiation measurements in the range of 200 to 600 keV and soon up to at least 1 MeV. In addition to the ICRU, NBS and the Standardization Committee of the Radiological Society of North America (SC/RSNA) were actively studying the question.

The ICRU had another problem, primarily of an organizational nature. In accordance with its original establishment, it was authorized to have two member delegates from each participating country--some 50 such countries in the 1930's. While many countries did not name representatives to the ICRU, the number of attendees was unreasonably large (40 in 1928, 39 in 1931, 33 in 1934, and 41 in 1937). Because only a few of the attendees could contribute meaningfully to the discussions, it was decided in Zurich (1934) to establish an Executive Subcommittee to decide which technical problems and their proposed solutions would be presented to the ICRU for approval. The first such Executive Subcommittee was made up as follows:

> I. Solomon, France, Chairman; L. S. Taylor, U.S.A., Secretary; E. A. Owen, United Kingdom; H. Behnken, Germany; E. Pugno-Vanoni, Italy; and R. Sievert, Sweden.

Details of the organizational changes and 1934 recommendations (ICRU, 1934) were discussed by Taylor (1958C). It was in preparation for the 1934 Zurich meetings following the 1931 meetings in Paris that the SC/RSNA began studying the measurement problems.

General Communication #5 was the preliminary report on the Zurich meetings of the International Committee.*

> *Beginning in 1934, the Standardization Committee adopted a communication numbering system so as to keep better track of discussions and problems. Some of these are available in the files but several are missing.

As will be pointed out, copies of the SC/RSNA report (see p. 116) of 1933 (RSNA, 1934) had been sent to all members of the International X-Ray Units Committee. Official replies were received from most of the leading countries, with the exception of England whose members did not wish to reveal their views until just before the Congress. Based on replies and informal discussions with several representatives, a program was prepared in advance of the Congress which was agreeable to France, Germany, Italy, Sweden, and the United States, and which embodied many of the essential principles contained in the report (ICRU, 1934).

The program was adopted unanimously by an ad hoc subcommittee of which Dr. E. A. Owen was the Acting Chairman. However, when Owen presented the report to the Executive Subcommittee, he attacked its conclusions, thus reversing the stand of the ad hoc committee. This created the impression that the other countries were opposed to the report, rather than an expression of the British position only. The principal point of contention was a rewording of the definition of the unit in accordance with the RSNA 1933 proposals, which were acceptable to Germany, France, Sweden, and Italy. After withdrawing this point in favor of the old definition, the remainder of the Subcommittee Report was adopted intact. (As noted in the communication, "Through Sheer Exhaustion".) To forestall such actions in the future, a proposal was submitted setting forth more clearly the exact organization of the International Committee. Thus no country had complete control; instead it was put in the hands of the small active Executive Subcommittee mentioned above.

An attachment to Communication #5 was an unpublished report of the RSNA Standardization Committee, a copy of which follows:

REPORT OF STANDARDIZATION COMMITTEE
October 13, 1934

At the last regular meeting of the committee held in Chicago, during the meetings of the American Congress of Radiology, a fairly comprehensive report was agreed upon and published a few months later. That report was really the result of some years of effort under Dr. Ernst's chairmanship, and the committee is probably not desirous of making any immediate extensive changes. It will, in fact, form the basis for our discussions preceding the next International Congress of Radiology in Chicago.

The main purpose of the report was to clarify our position at the 4th International Congress, and in this respect it did give us a strong hand to play at Zurich. No other countries had any such complete recommendations--worked out and discussed in detail, although one country tried at the last moment to introduce some of its recommendations with practically no opportunity for discussion.

Many of the more important details of our recommendations were incorporated in the international recommendations, although, of course, the actual wording was usually altered.

Prior to the Zurich meeting, discussions were had with the French, German, Italian, and Swedish standardization committee representatives. As a result, together with them, we worked out an agreeable set of recommendations to be presented to the International Committee. With one exception these were all adopted. It is also worth noting that largely as a result of our committee's endeavors, the international standardization committee was reorganized along lines which will eliminate much of the nationalism which has dominated its last three meetings and materially lessened the benefits of its decisions. It is hoped and expected that the next meeting of the International Committee will be its most thorough and far-reaching.

Our report of last year was published last spring. The report of the International Committee appears in the current number of Radiology. There is no need to read them now. Our work for the next three years will be in the nature of ironing out the rough places in our last report.

From the clinical point of view, one of the committee's desires has been to set up some kind of standard treatment recording chart, wherein may be expressed simply, all of the dosage factors entering the treatment of a patient. This would contain the minimum number of factors required and would contain directions for obtaining each. This should greatly simplify the descriptions of treatments in publications and in talking. (May I ask at the moment whether or not you consider such a step advisable. The committee may be spared a great deal of work.)

Again, in preparation for the 1937 meetings of the ICRU, the SC/RSNA considered the question of the definition of the roentgen for application to high-energy radiations. A committee paper was prepared in October 1936 as follows:

A NOTE ON THE ROENTGEN
October 1936

In clinical work it is customary and convenient to specify the quantity of radiation in roentgens and to rate the output of a tube in roentgens per minute. The roentgen is thus the fundamental unit of dose in x-ray therapy and it is important to know just what it is a measure of.

The accepted definition is based on the conductivity produced per cubic centimeter of air, or in other words, on the number of ions which can be collected from one cubic centimeter of air.

For low voltage radiation the number of ions produced in a given cubic centimeter within the beam does not depend on the cross section of the beam nor on the proximity of other substances, but with high voltage radiation it depends very much on these factors. This is obvious for the secondary electrons do not all remain in the beam and their range in air may be large compared to the width of the beam and hence a large fraction of the ionization may be produced in the air surrounding the beam, and not in the beam itself. If, on the other hand, the beam is wide compared to the range of the secondary electrons then the ionization in any one cubic centimeter within the beam is produced in

part by electrons originating within a given volume and in part by electrons from the surrounding air. In addition, there may be a large contribution due to scattered radiation. Thus, the ionization per cubic centimeter may be many times as great in a large beam as in a narrow beam.

In principle, it is possible to measure the ionization actually produced in one cubic centimeter in any beam and in some respects this would seem to be a most useful procedure, for it would presumably give the most direct information regarding the biological effect to be expected under a given set of circumstances, but unfortunately the measurement is somewhat inconvenient to make.

It has become customary instead to measure the total ionization produced in and around a narrow beam by the secondary electrons originating in a known volume of air. This is a very different thing from the number of ions produced in one cubic centimeter of air. That this is so is readily seen from a consideration of the standard air chamber which is generally approved for measuring the quantity of x radiation.

THE STANDARD AIR CHAMBER

The standard air chamber and all other equivalent devices such as the German Fasskammer are all designed to measure all of the ions produced in and around a narrow beam by the secondary electrons originating within a known volume of air.* A narrow

*Ed. Note: The condition of measurement is now (1980) called "electronic equilibrium."

beam is defined by one or more diaphragms and permitted to pass between the plates of an air-gap condenser. A section of the condenser is insulated from the rest and arranged so that all the ions drawn out of a volume of known length but unknown cross section may be measured. This volume we shall refer to as the collector section. The ions in this volume are produced by secondary electrons originating within the beam partly in front of the collector section and partly within it, but they correspond in range and energy and therefore in ionization exactly to all of the secondary electrons originating in that section of the beam which lies within the collector section. We shall refer to this as the measuring volume.

Thus, the number of ions collected from the collector section is exactly proportional to the true absorption in the measuring volume and therefore the energy absorbed is

$$E = E_1(T + S_1)\, n\, V$$

where E_1 is the energy in the beam, T is the photoelectric absorption per electron, S_1 is the true scattering absorption coefficient (a better name for which would be conversion coefficient), n is the number of electrons per cubic centimeter, and V is that volume of the beam which is within the collector section, i.e., the measuring volume.

This energy is said to be truly absorbed but it must be noted that the resulting ionization is not confined to the measuring volume but occurs in an unknown volume within the measuring section.

In addition to the ionization just described, there is a certain amount of ionization produced in the collector section by radiation scattered from the beam all along its path. This contribution is generally small but may not be altogether negligible, for although the scattered radiation is a hundred to a thousand times less absorbable than the secondary electrons, there may be as much as five times as much energy scattered per cubic centimeter as there is truly absorbed, or rather converted, and scattered radiation can reach the collector sections from a much greater length of the beam. This is especially important if the collector section is much larger than necessary.

In defense of this generally accepted procedure one might advance the argument that the standard air chamber measures roughly the ionization which would be present in one cubic centimeter of air in a very wide beam. This, however, is not necessarily true for in a wide beam the ionization may be considerably greater than this due to scattered radiation. Unfortunately, the definition does not state whether scattered radiation shall or shall not be included in the measurement.

The measurement does not depend greatly on the size and shape of apparatus nor, within wide limits, on the cross section of the beam used, hence it gives information regarding the characteristics of the X-ray equipment used rather than regarding the effect of the radiation in any particular case. For very high voltage radiation, the standard chamber must unfortunately be so large as to be quite impractical and it seems likely that it will become necessary to resort to other means for making the measurement such as the thick-walled thimble chambers, or similar devices. The only other alternative would be to base the measurement on some other quantity as for instance the ionization actually produced within one cubic centimeter regardless of whether the secondary electrons are fully utilized or not; but on the whole it would seem preferable not to make a change as radical as this and it seems to me advisable to remove the ambiguity in the present definition.

A definition which I believe would conform to what we are actually accustomed to measure would read somewhat as follows:

"The international unit is that quantity of radiation which, when the secondary electrons from one cubic centimeter of air are fully utilized and* scattered radiation

*Ed. Note: Today, 1980, there would be added here, "ionization produced by electrons generated by." In 1936 this sophistication of understanding had not yet developed.

is avoided, produces in air at zero degree C and 760 mm mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured."

L.S.T.

The growing concern for the definition of the roentgen, as adopted in 1928, that would be suitable for the measurement of gamma rays and high-energy radiations was the subject of extensive discussion prior to the 1937 meeting of the International Commission on Radiological Units in Chicago. Proposed changes by the United States members of the Commission were discussed and agreed to.

In addition to its regular membership, the Standardization Committee had asked Dr. C. C. Lauritsen of the California Institute of Technology to attend its meeting in September 1937. Lauritsen, one of the first to introduce the million-volt accelerator, had made some proposals in the thirties regarding free-air ionization chamber measurements for million-volt radiation. Though these turned out later to be invalid, his subsequent considerations of the problems were appropriate. Following the meeting in September 1937, there was correspondence between him and members of the committee which was pertinent to the development of the philosophy of radiation measurement. Part of that correspondence follows:

(Lauritsen to Taylor) September 27, 1937

The radiologists certainly are entitled to a unit of dose and I think that it is significant that they have misconstrued the roentgen in such a way as to be able to use it and the best that we can hope for now is a definition which makes it suitable for their purpose. It would be most unfortunate to introduce a unit which would be different in magnitude or which would make present instruments obsolete. The only solution would therefore seem to be to have two units, one a unit of intensity of radiation, the other a unit of dose. I should like to suggest that the unit of intensity be called the duane and that the unit of dose be called the roentgen. I am enclosing a note on this subject which I hope you will read carefully and I should like to have your comments and suggestions as soon as possible. I believe that you will agree with me on these units in a general way, but it is very likely that you would prefer a somewhat different wording. I should like very much to know your precise opinion on this and also what would be your suggestion as to the most effective way of reaching general agreement on this or some similar solution. It seems to me that perhaps the best method would be for you to take this matter up with the members of the American Committee individually as well as with others in this country who are interested in the subject, then if agreement has been reached in this country the American Committee could get in touch with the individual members of the Foreign Committees. But as I said before I should like to have your suggestions as soon as

possible for I feel that it is of the utmost importance to get a solution to this problem before things become even more complicated than they are at present.

(Taylor to Lauritsen) October 7, 1937

I find your letter of September 27th most interesting and agreeable. I am fully in accord with your general idea of having two x-radiation units and moreover, I think most other physicists are also. This very point was discussed at some length in our final subcommittee meeting in Chicago (Behnken, Hopwood, Pugno-Vanoni, Solomon (M.D.), and myself). We all agreed in principle, but felt that at the present time it had not been sufficiently discussed and its introduction would make too great confusion. I am quite desirous of pulling for such a change and feel that our efforts have a reasonable chance of success if we just thrash out our difficulties among ourselves first. The present international proposals embody a last minute effort to unscramble the present confusion.

There are two points that we should consider: (1) The physicists have always wanted a unit of "intensity" and not "quantity." This was also brought up at the meetings. (2) Several people have independently proposed the establishment of a complete set of x-ray units. The latest was Newell's attempt. Six years ago Pugno-Vanoni made similar proposals. Kaye, Behnken, and I tried to work out a system four years ago, but with no great success. The question arises then, why not go all the way and set up a complete new system of units. Such a system would necessarily embody your proposals in the form you give, or something similar. How would such a plan strike you? Newell's proposals are quite sound and have been thoroughly gone over by an expert on such matters.

Taking your suggestions, I question the use of the term duane. I believe Friedrich is really the father of such measurements, even though Duane did much toward getting open air measurements into vogue. The first definitions of the "roentgen" (free air type) were made by the Germans. Actually Villard and Szillard made the first proposals in 1908 and the present air standard and unit is essentially as Villard proposed it at that time. Consequently, I doubt that the Europeans would take very kindly to such a name as the duane. We can find a good Greek name for it.

I agree with you that the open air standard is the best and will probably remain the best primary standard. I should oppose any departure from this, on the basis of our present knowledge. I wonder, if, as long as we use a unit based on the open air chamber, we might make it an "intensity" unit. It is much more convenient for physicists than the "quantity" unit.

The dose unit is alright essentially as you give it. However, in both cases, I should prefer the mass unit rather than the volume unit. The mass concept is surely coming in any case, and it does simplify the thinking on, as well as the definition of, the unit. I shall give the whole matter more thought but should like your reactions to my comments thus far.

(Lauritsen to Taylor) October 18, 1937

Many thanks for your letter of October 7th. It seems to me that the main thing to be decided now is whether we shall have two units, one for measuring radiation and one for measuring dose. If it is decided to have two units I think the first should be based on the open air chamber but I do not care particularly whether it is called the duane or something else, as long as it is not called the roentgen. I can not believe that it makes any difference to the physicists whether it is a unit of intensity or of quantity but I think it will cause further confusion if one unit is a unit of intensity and the other unit of quantity.

It is simple enough to express the output from a tube in duanes per minute at 100 cm distance and the output from a source of radium in duanes per hour at 1 cm, or any other convenient time and distance.

Total dose should be designated in roentgens at a given point, such as roentgens on the skin or roentgens at 10 cm depth.

Regarding the question of whether the units should be based on mass or volume, I have no strong preference but it should, of course, not be changed in order of magnitude. Therefore, since it is based on 1 cm^3 it seems to me better to leave it that way rather than to put in the weird looking mass equivalent. If we were making a new system I should not object to basing it on the ionization per gram but that is

impossible now. You might solve the problem by saying, "per cm^3 of air weighing 0.001293 grams."

The matter was also pursued by Dr. Laurence, a member of SC/RSNA.

(Laurence to Taylor) October 25, 1937

I am making the following comments on the unit proposed by Dr. Lauritsen in compliance with your request.

I am inclined to think that units of this kind will be credited with merits they do not possess. They are intended for the measurement of the relative number of ions formed in the tissue. (I leave discussion of this until later.) It has been mentioned that the number of ions formed per unit volume is not proportional to the number of roentgens near the boundary of the material (e.g., the skin surface) and near the boundary of the beam (this is unimportant clinically for narrow beams are not used), and it has been stressed that it is a measurement of an effect of the radiation, rather than the radiation itself. Tell the radiologist this and he will think "This is fine. We have a measurement of the effects at last. It is only necessary to state the relative number of ions per c.c. formed and we have fully described the treatment without specifying the number of roentgens, the quality, the volume exposed, or the nature of the material over the skin." That assumption is quite erroneous. At best, we have reduced the number of significant variables by one only, and we are not very sure that we have done that. (For evidence, see Dr. Failla's measurements with such a unit.)

Now consider whether we have simplified the problem for the radiologist. He is familiar with the present roentgen. He knows that the intensity is greater at the skin surface than in free air, that if he places his thimble chamber on the skin his measurement gives a value between the free air dose and the actual skin dose (in roentgens) and that if he takes care that the chamber and skin is covered with sufficient thickness of material of organic composition (and the focal-skin distance is not too short) his error will be only a few percent. We tell him that if he always covers the skin with certain thicknesses (depending on quality) of certain materials, he need not worry about the boundary effect resulting from the long range of the secondary corpuscles. We tell him also that if he cannot use the prescribed covering material he will have to use a greater or less dose to get the desired effect, and we supply him with data based on measurements with Dr. Failla's chamber that will enable him to make the correction. All this he can easily understand. We believe that the correction data based on the extrapolation chamber will permit closer correlation in clinical experience but we do not claim that we have accurately solved the problem, as we would imply we committed ourselves to the invention of a new unit based on extrapolation measurements.

Most radiologists do not understand clearly the meaning of the roentgen, but gradually by rules of thumb we are teaching them to avoid the more serious sources of error. It is now proposed to undo what has been done, and start all over again with an even more difficult unit--one that the physicists even have difficulty in interpreting.

As regards names; it would be a mistake to rechristen a unit that is defined now in all text books, and give its former name to something else. Let us give the new name to the new unit.

I have assumed above that measurement in terms of Dr. Lauritsen's unit is a measure of the relative number of ions produced in the tissue (or more precisely, in the material of the phantom which is similar to the tissue). This assumption is only justified (by suitable accuracy) with extra-hard x-rays. The difference in the ratio of measured ionization current with the number of ions produced in the phantom material very close to the cavity is small for different qualities of extra-hard rays, and for most purposes can be ignored. The ratio varies considerably (order of up to 25% probably) in the 100 to 200 kV range. (Note also that in the 100 to 200 kV region the wall of the chamber must be the phantom material--even the chiffon wall of Dr. Failla's chamber is too thick to neglect if its atomic composition differs significantly from that of overlying material.)

To summarize, (1) the Lauritsen unit is a satisfactory basis of measurement of ionization production with extra-hard radiation. (2) The distinction between the number of roentgens and the number of ions is important with extra-hard x-rays. (3) The proposed unit has not the significance intended when used with softer radiation. (4) Measurement in this unit is difficult with softer radiation. (5) The choice between

roentgens and the proposed unit is probably not important clinically with softer radiation owing to the shallowness of the boundary region.

The foregoing adverse criticism is given to call attention to the difficulties. My attitude is that we, the physicists, should make sure that the radiologist needs a new unit, and that we have chosen the most suitable one and are not likely to abandon it for an entirely different one later, before we tell them about it. The concept of ionization density, or even relative ionization density, is of course, useful to the physicist and the biophysicist. Kaye, at Chicago, suggested the most satisfactory unit for the former I have heard yet; e.s.u./c.c. The unit of the latter is "one"; relative values are pure numbers.

Except for these discussions there was temporarily no further action on the question of basic radiation quantities and units. By late 1938, it was clear that Europe would be in a turmoil with little chance of a meeting of the Congress, the ICRU, or the ICRP in the foreseeable future. Also, the combined Standardization Committee of the RSNA and ARRS was preoccupied with other problems--the registry of physicists, clinical dosimetry, and an assortment of other problems. Not until 1947 were the questions of radiation quantities and units addressed again in depth, and then under organizational circumstances that caused some confusion. This will be dealt with in other chapters and in the Committee reports scattered throughout this document. For references to these by page number see Appendix A.

Radiation Standards Discussions (1947)

New actions on the subject included several "position papers" designed to provide a starting point for discussions. By this time, 1947, many physicists had had considerable experience in the atomic energy programs and could contribute new concepts and approaches not yet developed in the 1930's. Included in the new radiation armamentarium were particle radiations, radiations from artificial radionuclides, newly devised quantities and units, and an almost limitless array of new clinical applications. One of the early discussion papers, prepared for a meeting in November 1947, follows:

PHYSICAL TERMINOLOGY USED IN MEDICAL RADIOLOGY TO DESCRIBE EXPOSURE TO RADIATION

G. C. Laurence, 3-12-47

Confusion frequently arises from inconsistent use of such terms as "dose", "exposure", "energy absorption", and "roentgen" in describing the exposure of personnel to gamma-rays and other forms of radiation. Part of the difficulty results from the fact that some of these terms have meanings in common usage which differ from their restricted technical significance in radiology. In recent years radiological physicists have attempted to promote more careful use of such terms in an effort to clarify their meaning. During the war, however, scientists in the Atomic Energy Project became concerned with new problems of radiological dosimetry, and through their isolation in secret work, have departed from the usage that prevailed in medical radiology. The invention of a number of new terms and physical units has in some cases also added to the confusion.

The following explanation is intended to call attention to some of the ambiguities that arise. It is hoped that it will contribute to clarity in the use of these terms within the project. The discussion is elementary, since it is concerned only with the meaning of the terms.

Geometrical Considerations

It is convenient to speak of the flow of quanta or particles, the flow of energy, the conversion of energy and the dose, <u>at a point</u>. For this purpose the word "point" means an elementary volume of space which is sufficiently large to avoid any difficulty in interpretation due to the quantized nature of atomic processes, but sufficiently small that the physical properties of the medium and the intensity and other characteristics of the radiation can be regarded as uniform throughout the volume.

A distinction is made between incident or free air irradiation, and tissue irradiation. The former refers to conditions which would exist at the point in question if the exposed person or biological tissue was removed. For practical reasons

permissible exposure limits are usually specified in terms of incident or free-air irradiation. Tissue irradiation refers to conditions actually in the biological tissue.

It is assumed that, in general, the radiation is reaching the point concerned from many directions. Thus the flow of quanta or particles traversing a point is given by

$$N = \int \frac{dn}{dw}\, dw$$

where dn/dw is the number of quanta or particles per unit area per unit solid angle travelling in the directions confined within the cone of solid angle dw, and the integration is taken over all possible directions.

The Energy Absorption in the Tissue

The physiological effects that are caused by irradiation of biological tissues with gamma-rays, X rays, fast alpha, beta or proton particles, result from the absorption of their energy in relatively degraded forms such as ionization atomic excitation, ultra-violet rays, molecular dissociation and other processes involving energy conversions of not more than a few electron volts. It is this degraded energy that leads to most of the chemical and physical changes which disturb the normal physiological processes in the tissue. Hence the quantitative description of the degraded energy absorption is of direct interest in comparison of the physical effects of irradiation with the consequent biological effects.

The absorption of the degraded energy can be described in terms of the unit, erg per gram. The magnitude thus described can be called briefly "the energy absorption" as long as it is clear from the context that the reference is to the low grade energy conversion only, and not to high grade energy conversion processes by which the energy is carried a significant distance away to give low grade absorption elsewhere in the tissue. In other words, "energy absorption" used in this way means the quantity of energy per gram actually absorbed locally at the point in question, where "point" is used in the sense described in the introduction.

Sometimes the word "dose" has been used in referring to the local energy absorption. This has been a frequent cause of confusion because the word "dose" is used in radiology with a very different technical meaning which will be explained later. The term "energy absorption" is a natural and convenient name for the concept we are discussing, and it is consistent with the use of the unit, erg per gram. The word "dose" for this purpose should be avoided.

There is no satisfactory known method of measuring the energy absorption in biological tissue. It is usually calculated from information about the fast ionizing particles (secondary beta-rays produced by gamma-rays, or recoil atoms produced by neutrons or particles emitted by interstitially distributed radioactive matter) that are directly responsible for the production of the low grade energy.

It is believed that the same energy absorption produced in the same tissue of fast beta particles, and by heavier particles such as recoil atoms, will not produce the same physiological effect. The difference may be due to the denser concentration of molecular changes along the path of the heavier particle. In specifying the energy absorption, therefore, it is important to state whether it is caused by beta-particles or heavy particles. For example, the energy absorption due to a neutron exposure might be described as "1000 ergs/gm. due to beta-particles and 3000 ergs/gm. due to heavy particles". In such a case it would scarcely be satisfactory from the biological standpoint to describe it as "4000 ergs/gm".

The Ionizing Particle Dose

Neutrons, gamma rays, and X rays do not produce the degraded energy absorption in the tissue directly. They produce beta particles and (in the case of neutrons) protons and recoil atoms. It is these fast ionizing particles that produce directly the highly degraded energy which causes the biological effects.

For calculating the energy absorption it is convenient to use ionizing particle dose. The ionizing particle dose is measured in a standard substance, namely dry air, in multiples of the roentgen, by adapting this unit which was originally defined for x

rays and gamma rays for use with ionizing particles. The definition of the gamma-ray and x-ray unit as revised and adopted by the 5th International Congress of Radiology at Chicago in 1937 is as follows:

> "The roentgen shall be that quantity of x or gamma radiation such that the associated corpuscular emission per 0.001293 gm. of air produces, in air, ions carrying 1 e.s.u. of quantity of electricity of either sign."

The unit has not been redefined for use with ionizing particles by an International Congress, but the definition implied by current usage might be expressed as follows:

> "One roentgen is that quantity of high energy corpuscular radiation such that it produces in air, ions carrying 1 e.s.u. of quantity of electricity of either sign per 0.001293 gm. of air."*

*Ed. Note: Assumes electronic equilibrium, 1980.

The dose of fast ionizing particles would be measured directly in terms of this unit by use of a thin-walled ionization chamber of suitable design. The dimensions of the chamber should be so small that the flow of particles is approximately uniform throughout its volume, and it should be filled with dry air at atmospheric pressure. The walls of this chamber should be so thin that they do not appreciably alter, by absorption or scattering, the flow of the fast particles at the point. The presence of insulators and other parts of the apparatus should not interfere with the distribution of the particles. A special form of such a chamber is the extrapolation chamber developed by Failla [1].

As an ideal chamber for the measurement of the fast particle dose at a point in a material it is possible to imagine a cavity filled with air in the material, so small that its presence does not disturb the distribution of the particles, and provided with means for measuring the ionization produced within. Bearing this in mind, we might appropriately interpret the term "ionizing particle dose" as meaning quantity of the particle stream measured in terms representing the ability of the particles to produce ionization in air contained within a specified tiny cavity at the point in question. This dose would be expressed in roentgens, on the basis of one roentgen per e.s.u. of charge of either sign collected under saturation conditions per 0.001293 gm. of air.

The energy absorption resulting from a dose of 1 r of beta-radiation or other fast particles is approximately 83 ergs/gm. in air, about 88 ergs/gm. in biological tissue, and appreciably different in materials of high atomic number and in hydrogen. This conversion ratio can be used in estimating the energy absorption from the measured dose. It may be noted that on the strict definition given above, the dose of fast ionizing particles at any point is an attribute of the particles themselves, and not an attribute of the energy absorption in the material occupying that point. "Energy absorption" is applicable to the effect produced by the particles, and "dose" is applicable to the particles themselves. The same beta particles traversing a point in space are characterized by the same dose no matter what material is present at that point, but the energy absorption depends on the material. It must be noted however that the dose, and indeed the particle flux are affected by the material surrounding the specified point.

In speaking of the dose at a point surrounded only by the atmospheric air of the room, we call it the <u>free air dose</u>. The free air dose is important because it is amenable to direct measurement and, therefore, convenient for specifying the safe limits of beta-particle exposure.

Similarly the expression "<u>tissue dose</u>" is used to describe the dose which may be measured in a phantom or calculated at a point in tissue when correction for absorption and scattering is applied to the free air dose. For example, measurements might show a dose in one second of 10 r at a particular point in front of a cathode ray tube. If now a patient's body is placed in the beam so that the point in question is 2 mm. below the surface of the skin, the tissue dose might be only half as big as the free air dose at that point, namely 5 r.

The Dose of Gamma Rays or X rays (Throughout the text "gamma rays" will be used to mean either gamma rays or x rays).

The "dose of gamma rays" means, in the technical sense used in radio-therapy, a quantity of γ radiation measured in roentgens. For this reason it is an inherent attribute of the gamma rays; it is the quantity measured in terms representing their ability to ionize air--an ability which they still possess in the absence of air, or in the presence of another substance, and remains latent if the rays are carrying (sic) a vacuum. The dose of gamma-rays is not a measure of the absorption of the gamma-rays or other effects that depend on the nature of the material present.

The definition of the roentgen as already quoted on page 4 is:

> "The roentgen shall be that quantity of x or gamma-radiation such that the associated corpuscular emission per 0.001293 gm. of air produces, in air, ions carrying 1 e.s.u. of quantity of electricity of either sign."

To understand this definition it is helpful to imagine the gamma rays from an external source traversing a very large room containing only air, and to consider an elementary volume located in the room at a distance from the nearest wall greater than the range of the secondary beta particles. Within this elementary volume the gamma rays produce, by photo-electric effect and Compton effect and pair production, secondary beta particles that travel out into the air surrounding the elementary volume and produce ionization. If all the negative ions (or the positive ions) produced everywhere both inside and outside the elementary volume by the secondary beta particles produced within the elementary volume are collected, their total charge measured and divided by the mass of air in the elementary volume gives the dose of gamma rays at the elementary volume expressed in roentgens.

There is a reciprocity between the elementary volume and the surrounding air. The number of ions produced in the elementary volume by secondary beta particles originating in the surrounding air is equal to the number of ions produced in the surrounding air by beta-particles that originate in the elementary volume [2,3]. This fact offers an alternative method of determining the gamma ray dose at the elementary volume, namely measuring the total charge of either sign produced within it and divided by the mass of air which it contains.

We can now imagine the air surrounding the elementary volume replaced by a solid shell of equivalent material. By equivalent we mean that it would contribute the same secondary beta particles to the enclosed elementary volume of air. Graphite, bakelite, and most thermoplastic organic materials are suitable materials for this air equivalent shell, if the gamma rays are fairly hard. The thickness of the shell, of course, must be such that no secondary beta particles originating outside it can penetrate to the enclosed volume of air. This shell with its enclosed air is called an "ionization chamber with air equivalent walls" or simply an "air walled chamber"--a practical device for the measurement of gamma ray dose.

Biological tissue itself is of suitable composition to form the material of air equivalent chamber walls, and therefore [4] a cavity in the tissue filled with air would constitute an ideal ionization chamber for the measurement of gamma-ray dose in a biological tissue, except close to its boundaries where error would result from the effects of secondary beta-particles originating outside the tissue.

It is useful at this point to compare the method of measurement of the tissue dose of gamma rays and the dose of the particles which they produce. Both can be measured in small chambers. The beta-particle dose is measured in a chamber with walls so thin that absorption of the beta-particles in penetrating the walls can be neglected. The gamma-ray dose is measured in a chamber with walls so thick that the secondary beta particles cannot penetrate them. In biological tissue the dose of the gamma rays and the dose of the beta particles produced by these gamma rays are equal except within a distance of the tissue boundaries less than the range of the beta particles.

The free air dose of gamma rays is the dose which would occur if the biological tissue were removed. The tissue dose of gamma rays is the actual dose obtained when the tissue is present. These differ by the absorption and scattering of the gamma rays in the tissue in reaching the point. It is convenient to use free air dose in specifying safe exposure limits, since this is capable of easy direct measurement. In radiation therapy, on the other hand, the tissue dose is specified because a better correlation with physiological effects can thus be obtained.

The energy absorption in tissue resulting from exposure to gamma rays is proportional to the tissue dose except near the boundaries. The ratio of dose to energy absorption is approximately 88 ergs/gm/roentgen.

The departure from strict proportionality near the skin surface is commonly disregarded in radiotherapy because it is small owing to the presence of the base of the treatment cone immediately above the skin surface. This base is usually made of bakelite or other material which is similar in atomic composition to tissue and thus serves to displace the effective tissue boundary above the skin surface.

The following terms are in common use in radiotherapy.

"Dosage rate" at any point is the dose which occurs at that point in a short interval of time divided by that interval of time in which it occurs. It is expressed in such units as roentgen per second, roentgen per minute, etc. (Permissible dosage limits should not be expressed as dosage rates but as a maximum dose in a specified time interval, e.g., ".3 r in one week", not "0.3 r per week". The former expression sets no limit on the dosage rate which may occur for a very short interval during the specified time.)

The "dose meter" is an instrument calibrated to read in roentgens and used for the measurement of dose.

The dose rate meter is an instrument calibrated to read in roentgens/day (hour, min. or sec.) as the case may be and used for the measurement of dosage rate.

"Depth dose" is a tissue dose X cm below the skin surface.

"Dosimetry", as used in radiotherapy, is the practice and principles of measuring the gamma-ray dose.

Positive Beta Particles

When a radioactive isotope that emits positive beta-particles is distributed in the body of an animal the tissues are traversed by the primary positive beta particles, the secondary gamma rays resulting from the absorption of the positive beta particles, and the tertiary negative beta particles produced by the gamma rays. We can speak of the dose of beta particles, adding together the negative beta-particle dose and the positive beta-particle dose. We can also speak of the dose of secondary gamma rays.

The dose of secondary gamma rays is of minor interest except as it occurs as an intermediate step in calculating the dose of tertiary beta-particles. In comparison of the physiological and physical effects we are concerned only with the dose of the two kinds of beta particles. Alternatively, we can compare the physiological effects with the energy absorption due to the two kinds of beta particles.

The distributions of the primary and tertiary beta-particle doses are usually quite different. If the radioactive element is selectively absorbed in a small gland the primary beta-particle dose is predominant within it. The tertiary beta-particle dose is more widely, and therefore more weakly, distributed in the neighbouring tissue and its effect within the gland will be negligible in comparison with the positive particle dose.

If the radioactive element is distributed throughout a large bulk of tissue the tertiary beta-particle dose cannot be ignored.

Neutrons

Neutron irradiation is commonly described in terms of neutron flux, i.e., in terms of the number of neutrons per sq. cm. of area perpendicular to their path traversing the point where the flux is specified. This is equivalent to the number of neutrons present in unit volume multiplied by their average velocity. Since the neutrons traverse the point with a wide spread of directions, the flux is the integration of its components in all directions in accordance with equation (I).

No specific meaning has yet been given to "dose of neutrons" by general usage. Unlike in the case of beta particles, the adaptation of the roentgen is not simple and offers little advantage. We can, however, usefully speak of, and use, the doses of fast ionizing particles which are produced by the neutrons.

When thermal neutrons fall on the skin and penetrate to underlying tissue the neutron flux decreases with the depth x approximately according to

$$n \propto e^{-x/L} \qquad (I)$$

if a large area of the skin is so exposed, where L is the diffusion length of the thermal neutrons in the tissue approximately 3.2 cm.

The slow neutron flux at the skin surface, if the exposed area is large, is very nearly equal to what the neutron flux would be in free air at the same point provided there are no solid objects near from which the neutrons would be scattered. This is because the back scattering from the body almost completely replaces the neutrons cut off in the solid angle of 2π by the presence of the body. If other dense material is close to the skin re-scattering occurs between the skin and the material which will increase the neutron flux, particularly if the material has a large ratio of the scattering to the absorption atomic cross section. Hence in monitoring for health protection close to large objects, it is desirable that the measuring instrument be backed by a suitable phantom to provide multiple scattering.

The thermal neutrons produce a dose of protons, by the n,p reaction in nitrogen, which is proportional to the neutron flux. Assuming that 6% by weight of the tissue is nitrogen, the dose of these protons at a point is approximately 5.4×10^{-11} r/ per n/sq.cm. of flux at the point. The energy absorption due to the protons is approximately 4.8×10^{-9} ergs/gm/ per n/sq.cm.

The slow neutron flux also produces gamma rays through n,γ absorption in hydrogen and other elements. Nearly all of it is due to hydrogen, with an energy of 2.2 MeV. per quantum. The gamma rays in turn produce a beta particle dose that is greater and more deeply penetrating than the proton dose. It is difficult to calculate, but it can be measured in a phantom using a small ionization chamber with thin aluminium walls to exclude protons and alpha particles, and with 3/4% $LiNO_3$ added to the phantom solution to correspond to the 6% of nitrogen in biological tissue.

Fast neutrons falling on the human body produce a dose of atomic recoil particles of which about 93% is contributed by protons. The depth of penetration of this dose depends on the energy of neutrons on entering the body. For fission neutrons and neutrons from radium alpha-beryllium sources it decreases by a factor e in very approximately 7 cms. For neutrons of less than 1-MeV. energy the penetration is very shallow.

After slowing down to thermal energies the neutrons produce a dose of protons and a dose of beta-particles as described previously, but with a somewhat greater distribution in depth than the atomic recoil dose produced in high velocities, and with a maximum value at about 1 to 2 cm. below the skin surface. The atomic recoil dose produced at high velocities and the proton dose produced at thermal energies may be added together to give the total dose due to heavy particles. It is desirable to distinguish, however, between the heavy particle dose and the beta-particle dose on account of the difference in biological effect which are believed to result from the energy absorption of the two types of particles.

"Equivalent" Units

The importance of careful distinction between dose and energy absorption may be seen from the difficulties that arise if we regard the roentgen as a unit of energy absorption.

If the roentgen is interpreted as a unit of energy absorbed, it is only applicable to a particular material - air. Those who have attempted to use the roentgen in this way, have therefore abandoned it in describing effects in other materials, and have proposed various "roentgen equivalents."* But by long established practice which would

*One of these is the "REP."

be very difficult to alter, the number of units which is associated with a particular combination of gamma-ray protons in tissue is equal to the number of roentgens associated with the same combination of gamma-ray photons in free air, and that numerical equality has been retained in the relation of the roentgen to the "equivalent roentgen". Having thus fixed the size of the equivalent roentgen we find when we attempt to calculate from the free-air dose the number of "equivalent roentgen" at a point (an elementary volume) in the tissue that the absorption properties of the tissue at that point are not used in the calculation. (Only the properties of the surrounding material are required.) We are thus in the untenable position of professing to evaluate absorption in a material with a complete disregard of its absorption characteristics. When we made the unit "equivalent" to the roentgen in an effort to avoid disturbing the established procedure we actually made it identical with the roentgen--we abandoned its

use for the measurement of absorption and confirmed its use in the measurement of an attribute of the radiation.

Another kind of equivalent unit which has been suggested is identical with that adaptation of the roentgen for the measurement of ionizing-particle dose which we adopted above. Since this adaptation is so commonly used (and it is usually clear from the context whether the gamma-ray or beta-particle dose is intended) the qualifying word "equivalent" is unnecessary.

Biologically "equivalent" units present the difficulty of standardization. The difficulty is avoided if we regard them not as true units, but as brief laboratory colloquialisms to describe the irradiation in terms which take into account the probable sensitivity of the tissue. For example, if we interpret "1 rem of neutrons" as a convenient abbreviation which means "the neutrons that are capable of producing, in the opinion of the speaker, the same biological effects in the tissue as one roentgen or gamma-rays", we are using a well established, precise unit, vis., the roentgen, and we are shifting the lack of precision of the speaker's interpretation of the experimental data on the relative biological effectiveness of neutrons and gamma-rays (where it belongs). Thus we avoid the need of defining a new unit which is difficult to standardize. "Neutrons equivalent to 1 r" might be used as an alternative to "1 rem of neutrons".

Integrated Dose

Mayneord has developed the concept of the integrated dose of ionizing particles. This magnitude, D, results from the integration of the ionizing particle dose, r, throughout the mass of tissue irradiated thus

$$D = \int r \, dm \text{ - - - - - - - (II)}$$

It has particular application in discussing the irradiation of the blood or other body fluids in circulation or flowing throughout the body. It should be noticed that it is only of significance if the integration is applied to the ionizing particle dose and not if applied to the gamma-ray dose. Corresponding to the integrated particle dose there is an integrated energy absorption, related to it through the factor 88 ergs/gram roentgen, which would be used in direct comparison with the physiological effects.

References

[1] Failla, Radiology, August, 1937
[2] L. H. Gray, PRS, A156, 578, 1936
[3] G. C. Laurence, CJ of R, A15, 67, 1937
[4] H. Carmichael, Chalk River Report MTec-168

Summary of Suggestions

For Research and Discussion of Fundamental Principles,

1) Base the discussion on tissue irradiation--not free-air irradiation.

2) Compare biological effects directly with the energy conversion (in ergs/gm) produced by the ionizing particles, or with the dose (in r) of the ionizing particles. The two are proportional in any particular tissue and choice between them is a matter of convenience.

3) For the purpose of suggestion 2), where necessary convert neutron flux (in n/sq.cm.) and gamma-ray dose (in r) to the ionizing particle dose (in r).

4) Avoid use of the word dose where energy conversion is meant.

For Health Monitoring, and Statement of Permissible Exposure,

1) Make measurements and specify limits in terms of free-air irradiation.

2) Measure and specify limits of gamma irradiation in terms of the dose (in r).

3) Measure and specify the limits of neutron exposure in terms of neutron flux (n/sq. cm.) and the maximum energy of the neutrons (in Mev.).

4) Measure and specify the limits of beta-particles in terms of dose (in r).

5) Calculate and specify the limit of exposure internally to the radiations from radioactive substances in terms of quanta or particles emitted together with a description of their energy per quantum or particle.

G.C.L.

In a further discussion of the same general problem, Laurence (5-13-47) proposed an "experiment." It was an interesting exercise that gave an indication of the efforts at that time to unscramble the measurement situation. He wrote:

> The experiment is an attempt to justify formally and coordinate the prevailing current uses of the roentgen. I feel tempted to apologize for introducing into it such long words as "proclivity" and "depletion". If you can suggest better ones, I shall be grateful. The difficulty is that the word "dose" is used with such a variety of meanings that it is useless for the present purpose. In attempting to distinguish the different and very complex concepts to which it is applied, it is inevitable that one uses rather lengthy phrases. I have used the words "depletion" and "proclivity" here because they do not occur frequently in everyday speaking and writing, but their common usage meanings are suggestive of the restricted technical significance with which I use them. I should be most interested to hear your comments, as well as those of Dr. Fano.
>
> The energy of a high energy ionizing corpuscle crossing a small region of material is depleted in small energy conversions (of the order of a few electron volts) to produce ionization, atomic excitation, ultra-violet light, electrolytic dissociation, etc., which are absorbed locally in the region. The energy of the high energy ionizing corpuscle may also undergo large losses to produce secondary high energy corpuscles and X-rays which are not completely absorbed locally in the region. The word "depletion", however, is reserved here for the small energy losses which are absorbed locally in the small region. The depletion that occurs during irradiation may be described quantitatively in ergs per gram, or possibly in other units (see below).
>
> The depletion depends on an inherent attribute of the irradiation of high energy ionizing corpuscles, i.e., its susceptibility or proclivity to depletion, and also on an inherent attribute of the material in the region, i.e., its depletion constant for the particular kind and energy of high energy ionizing corpuscles.
>
> The proclivity of the irradiation of high energy ionizing corpuscles is described for practical purposes in terms of an effect (namely, ionization in air) which is closely proportional to it, by using the following unit.
>
> One roentgen of proclivity is that proclivity of the irradiation of high energy corpuscles which is capable of producing in dry air ions of either sign having a total charge of 0.00123 e.s.u. per gram of air.
>
> The proclivity may be measured, ideally or actually, in a small air cavity (as in the use of the extrapolation chamber).
>
> The depletion constant of a material is the ratio of the depletion in the material to the depletion that would occur in the same mass of air under irradiation by the same high energy energizing corpuscles (the depletion constant of biological tissue, for example, is approximately 1.07).
>
> The depletion is the product of the proclivity, the depletion constant, and a constant factor for conversion to the units employed (this conversion factor is unity if the depletion and proclivity are both expressed in roentgens).
>
> One roentgen of depletion is the depletion which occurs in air when irradiated with high energy ionizing corpuscles having a proclivity of one roentgen.
>
> Historically the roentgen has been used as a unit in describing irradiation by x rays and gamma rays. One roentgen of irradiation by x rays is that irradiation which is capable of producing in a small volume of free dry air bounded by air equivalent walls thicker than the ranges of the secondary corpuscles, an irradiation of high energy ionizing corpuscles having a proclivity of one roentgen. In the case, for example, of hard x rays traversing a region of tissue which is bounded by a thickness of similar tissue greater than the ranges of the secondary corpuscles, an x-ray irradiation of one roentgen produces an irradiation of secondary high energy ionizing corpuscles having a proclivity of approximately 1.00 roentgens, and this irradiation of secondary corpuscles undergoes a depletion of approximately 1.07 roentgens.
>
> G.C.L.

Because of the urgency of sorting out the growing confusion in radiation quantities and units, a roundtable discussion of the problem was planned for November 30, 1947, in Boston by Dr. R. R. Newell. A number of statements for the meeting were as follows:

Early History of X-Ray Dosimetry

L. S. Taylor

I will take just a few moments for a very brief review of the early history of radiation dosage measurement which started 25 years ago with the main problem of establishing a system of x-ray dosage but which has been extended in recent years to problems of dosage for many other types of radiation.

It is important for us today to appreciate the fact that the original requirements for a unit and method of x-ray dosage was for what was then thought to be, or perhaps, I should say hoped to be, a relatively simple and straightforward problem. In the early 20's, x rays were recognized as a useful therapeutic agent. It was realized that as an x-ray beam passed through the body it was partially absorbed and hence, it was thought desirable to measure a property of this radiation which was related to its absorption in tissue. At that time it was also recognized that the process of ionization of gases formed a convenient method for measuring x-rays. Consequently, it was considered that if the ionization was measured in a gas whose effective atomic properties were closely parallel to those of tissue, that such an ionization measurement would then bear a relationship to the ionization produced in tissue. Air was chosen as an ionization mechanism because it most closely met this fundamental requirement. The first specific proposal for employing air ionization measurements as a means of x-ray dosage measurement were made in 1918 by Szillard. From then up to 1928, a considerable number of investigators studied the application of air ionization measurements to x-ray dosage measurement and control.

Some of the earliest studies in this connection led to the development of thimble ionization chambers made of animal horn with the idea that such chambers could be inserted into the body or used in phantoms for exploring the distribution of radiation administered in broad beams to the body. This earliest work involving the construction of such thimble chambers was beset with many difficulties and it appeared to be virtually impossible to make two of them alike in an absolute sense. It was not until the work of Fricke and Glasser that we had a reasonably clear understanding of the mechanism involved in thimble ionization chambers but even after then it was realized that there were numerous serious problems in the way of using such chambers of standards. In the meantime, a proposal had been made that x-ray dosage be expressed in terms of the ionization produced in a defined volume of air. The big difficulty with the thimble chamber was that the walls of the chamber both added and detracted from the ionization produced within it and the absolute magnitudes of these effects were not easily definable or reproducible. As a consequence, the units of x-ray dosage were defined in terms of the ionization produced in a definite volume of air unrestricted by any wall material which would influence the ionization within the specified volume.

The physical achievement of such a measurement was accomplished by means of what we now think of as open-air ionization chambers and earliest models were of both the cylindrical and parallel plate type in which the electrodes were sufficiently far removed from the measuring volume so as to preclude the possibility of their affecting the ionization in the volume. The fact that such apparatus could be constructed was demonstrated for the voltages then in use, which did not exceed 200 KV peak. The first definition of x-ray dosage was tentatively accepted on an international basis in 1928 by the Second International Congress of Radiology. Essentially, the unit of dosage was defined in terms of the ionization produced in a cubic centimeter of air under normal conditions, with the avoidance of wall effects, and was called the roentgen. It is rather interesting and not entirely illogical that this early definition was to a considerable extent built around the apparatus for its measurement. However, as later events proved, this has turned into a very awkward situation because as our exciting energies increased, we were placed in a position of having to continually modify the definition of the roentgen in order to cope with the new properties of the higher energy radiations.

Because of this situation, minor modifications in the definition of the roentgen were made in 1931, '34 and '37--it being hoped at the last date that we had reached the ultimate in energies which were then in the neighborhood of a million volts. However, we are already operating at energies in the million-volt regions where entirely new processes come into play. Here we encounter severely limiting difficulties even in the simple measurement of ionization in an unrestricted body of air, not to mention the fact that the process may not even be representative of the basic situation which we wish to measure and study.

The definition of a roentgen over these formative years has, as I mentioned, been changed in wording. It has not, however, been changed so as to affect its magnitude in the moderate energy region. Essentially, the definition is based on the measurement of a tertiary phenomenon which takes place in the ionization chamber. We measure the ions which are produced by the secondary corpuscular radiations which in turn are developed through the absorption of an extremely small fraction of energy absorbed from the x-ray beam. This fraction varies rapidly with the exciting energy of the beam so that for very low energies or very lightly filtered radiations, we get disproportionately large ionization readings. This might be all right provided such readings in air closely paralleled the effects in tissue, but as we now know, this relationship is by no means simple. As far as the strict measurement of a roentgen is concerned, we can accomplish this in the moderate energy region without any serious difficulty. For, let us say 200 KV, a parallel plate ionization chamber which will contain a great bulk of the secondary radiations can be constructed of reasonable dimensions. The two critical dimensions must be chosen so as utilize the full range of the secondary corpuscular radiation and provide that the measuring volume be removed sufficiently far from the limiting diaphragm so that we have radiation equilibrium in the region of the measuring volume. For 200 KV this means an ionization chamber having a plate separation of 12 to 15 cm and with a distance of about 20 cm between the measuring volume and limiting diaphragms. For 50 KV, these dimensions will be reduced to 5 and 7 cm respectively. On the other hand, for a million volts, these dimensions become of the order of 250 and 500 cm. As energies increase beyond a million volts, we have the process of pair production setting in. By its nature this process precludes the possibility of establishing radiation equilibrium within any free-air ionization chamber of reasonable size.

In the meantime we now have the betatron with energies in the multi-million-volt range. We are compelled to study these radiations in comparison with lower energy radiations and to evaluate their relative effectiveness. Unless we can accomplish this in terms of a unit common to high and low energies alike, we are in the unpleasant situation of measuring a distance in feet and meters without knowing the relationship between the two.

Hence, we are compelled to investigate new methods for measuring x-ray dosage relative to the properties which I mentioned briefly above. Gray in particular, and other workers have demonstrated the soundness of the concept of utilizing the ionization produced in a cavity as a means for measuring the energy left by the radiation in a volume of material equivalent in shape and size of the cavity. The experimental attainment of this is not simple but the difficulties can no doubt be overcome. As far as the basic definition of a unit is concerned, it appears highly logical from physical considerations, to express this in terms of energy absorption and it is on that subject that Dr. Fano will report to you in a few minutes. I might mention here that work along these lines has been carried out by a number of investigators whose studies for the most part point in the same direction. Being much concerned with this problem at the Bureau of Standards, we have attempted to gather together these various studies with the idea of suitably coordinating them in the hope that a general agreement can be reached.

I have not mentioned what I consider to be the urgent necessity for having a unit which will measure in common terms x rays, gamma rays, neutrons, protons and electrons, since for biological purposes, we are compelled to draw comparisons between the effects of these radiations. This will no doubt come out in our subsequent discussions.

L.S.T.

Notes on Language of Engineers and Radiologists

R. R. Newell

These terms, built to parallel terms in illumination engineering prove now to have more possible use than when suggested.

I have used rhegmas in my clinical treatment records for 8 years. It makes it always easy to know whether one is talking about the beam of primary irradiation, or the sum total effective in the tissue irradiated (sum of effects of primary, backscatter and exit dose).

Rhegma has the same dimensions and I think the same size as Parker's rep (roentgen equivalent physical). Being defined in terms of volume ionization, it is to be extended to other ionizing irradiations without alteration.

Plem now appears useful as the unit of radioactive source, since we need to measure the quantity of certain radioactive substances in terms of their gamma activity, i.e. as radioactive sources. In therapy we are interested in irradiation (quantity of radiation

arriving). In most isotope measurement we are interested in the source, independent of the distance at which the physical measurement is performed.

In photography we're interested in exposure, i.e. the radiation arriving. In highway lighting we buy lumens (total luminous flux). In a searchlight we buy candle power, being interested in the intensity of the source. It is true we are interested in the intensity in only one direction, whereas in isotopes we think of radiation unselected as to direction.

There is no named unit for a source in terms of radiant flux (watts) put out. For a source of luminous flux, the candle is the unit. For a source of roentgen (or gamma) flux, the plem is the unit. The other terms are useful to help follow the thought.

1 plem gives 1 kludon per steradian
(1 candle gives 1 lumen per steradian)
which is 1 kludon per cm^2 at 1 cm.
(1 lumen per cm^2 at 1 cm.)
which is 1 rhothion at 1 cm
(1 phot at 1 cm)
which is 1 r per sec. at 1 cm
(the light unit corresponding to the roentgen would be a phot-second)
which would be 3600 r per hour at 1 cm
1 plem = 0.36 r per hour at 1 meter

Maybe the plem could be defined as the intrinsic (x-ray or gamma-ray) activity of the source. The measurements if uncorrected for self absorption and external absorption, etc. would then be stated in apparent plems.

R.R.N.

In further preparation for the meeting, Newell sent the following communication to the expected participants:

November 18, 1947

I promised you a summary. You have got instead a request to read the extended remarks of Dr. Robley Evans in Nucleonics October 47 and Dr. Lauriston Taylor's circular of 10 July 47. I am asking these men to lead off by high-lighting their portions of the subject of units so as to refresh your memories at the beginning of our meeting. Dr. Failla will do similarly for what he plans to have mimeographed in extenso. Dr. Curtiss has sent me some notes which I have embodied in the following brief:

The roentgen is neither a unit of X-ray quantity (energy flux) nor of X-ray treatment (energy absorption), yet it serves in a way for either. Taylor gives in detail how it does so serve. Are you contented with the roentgen in essence?

When the energy is high enough so that the range of recoil electrons ceases to be very short compared with range of scattered X rays, the definition of the roentgen becomes equivocal. Do you think it needs redefinition, specifically to avoid effects of scattered X rays?

When energy gets very high, pair production and secondary x-ray emission become important and, at many million volts, reverse the rule that secondaries are more absorbable than the primary rays. Equilibrium between primary and its secondaries may become impossible to attain. The corpuscular emission may have a range of 10 or 20 meters in air. Taylor says "measurement (in this range) that would correspond even approximately to the definition of the roentgen would yield little information on the primary radiation". Have you a suggestion for a unit that would be preferable in this range?

What is your feeling about Taylor's alternative proposals:

a) Change the roentgen to make it a measure of ionization within the irradiated material?

b) Change the unit of dose to make it measure the specific absorption of energy, without specifying a particular effect (ionization)?

He points out the dichotomy: If unit remains based on a specific effect, then the "dose" remains a characteristic of the radiation and equal irradiations (exposures) of different materials gives same "dose". But if change to energy absorption, then equal irradiations of different materials give different doses.

He also points out the inflexibility of a unit based on a specific effect, requiring, as we observe, frequent revision. But admits the difficulty of assessing the accuracy of a unit of specific energy absorption.

Other ionizing radiations are not X rays and can't properly be measured in roentgens. How would you extend dosimetry to include alpha, beta, proton, neutron rays?

Do you think ergs absorbed per cc would offer an advantageous blanket correlation of them all? or

Would you set up separate units for the several kinds?

Afternoon:

The historical definition of the curie is the amount of radon in equilibrium with 1 gm of radium, or the amount of any member of the radium family which disintegrates at this rate. This is the definition approved by International Radium Standards Commission.

There seems some logic in extending the latter part of the definition to a radioactive isotope of any element. Difficulties are: a) Lack of precise knowledge of radium disintegration rate; b) Lack of knowledge of disintegration schemes of many radioactive isotopes, making it impossible to know what measurements of their activity mean in terms of actual disintegrations. How do you feel about the curie's being used outside the radium family?

Quite illogical is the use of the curie to mean a unit gamma-ray source. You do agree it's indefensible?

Moreover comparing one gamma-ray source with another, the ratio is dependent on the wavelength sensitivity of the particular measuring instrument. This difficulty persists whatever unit we use for gamma-ray sources. Unless we use a unit based on radiant energy (ergs per cm^2 per sec, or watts per cm^2).

How do you evaluate the substitution of the rutherford for the curie as a unit of radioactivity (10^6 disintegrations per sec.)? It has been well chosen as to size (well fitted to the quantities in present experimental use). It would not be subject to revision, its size being arbitrary and not based on measurement. It would suffer same difficulty as the curie for isotopes of unknown disintegration scheme. What strategem can be found to escape this difficulty?

Dr. Curtiss writes that the National Bureau of Standards will use the rutherford in its laboratories.

He also says that for measuring gamma-ray sources, the Bureau will use roentgens per hour at one meter (rhm). This is useful for gamma emitters whose disintegration scheme is incompletely known, or whose disintegration rates are hard to measure. Do you find this more acceptable than a unit based on the gamma-ray activity of a standard (radium or other)? Does the hybrid nature of the unit bother you? Inasmuch as it's a measure of a radioactive source, does it seem illogical to name it on the basis of irradiation? We measure light sources in candles, not in lumens per cm^2 at 1 meter. One candle gives one lumen per steradian. At one foot this amounts to one lumen per square foot (one foot candle) of illumination. At 1 meter it is one lumen per m^2 (one lux).

I expect you vigorously to promulgate your own ideas. I expect to voice my own, but make no promises that I shall hold to them.

The meeting will take up at 11 am Sunday. We will recess for ten minutes to clear the cobwebs at 12. At 1 we will stop for lunch. This will be served on the job and paid for by the College.

I'm having no stenographic notes. I'd like to get from each of you a memorandum of the opinion you have arrived at by the end of the meeting. You can state your present opinion from the floor. I don't want it in writing, lest you be embalmed in it.

References: Evans, Nucleonics, October p. 32
Taylor, circular of 10 July 1947 (revised 11, 24, 1947)
Newell, Radiology 32: 270, 1939
Lind, Science 103: 761, 1946

R.R.N.

Also in preparation for the meeting of November 30, Dr. Laurence from the National Research Council of Canada forwarded a copy of a Canadian memorandum on the units which he would offer for discussion at the Boston meeting. He forwarded a copy to Taylor for discussion in advance of the meeting. His memorandum follows:

Nov. 24, 1947

Memorandum to: Dr. Lewis
Dr. Cipriani
Dr. Carmichael
from: G. C. Laurence

I expect to attend a discussion next week in Boston on the units which are used in radiology. I am summarizing my present views on this subject below for your information, in case you care to comment before I leave.

ROENTGEN (a)

The use of this unit, as its definition was revised at the Chicago Congress, should be retained in describing irradiation by soft and medium hard x rays. In this use it applies to the x-ray irradiation present in vacuum, air or other material, and does not apply to the energy absorbed in the material traversed (see REP below).

ROENTGEN (b)

The roentgen has been commonly used in describing the exposure of the material to high energy electrons produced either externally or by the conversion of the energy of x rays or γ rays traversing the material, as for example in the use of the extrapolation chamber. For this purpose it may be defined in close analogy with the definition for x rays as follows:

One roentgen is that irradiation by fast ionizing particles which is capable of producing ionization of either signs having a total charge of one electrostatic unit per 0.00122 gms of dry air, contained in a small cavity (in the limit of vanishing cavity volume) in the material traversed by the particles.

This use is already well established in practice and proves very useful.

In the use of soft x rays on soft tissues, the irradiation expressed in roentgens (described in (a) above), and the irradiation by the secondary electrons (as here described in (b)) are numerically equal and no serious confusion will arise from the use of the name 'roentgen' for both purposes.

In the case of very hard x rays, and γ rays, the roentgen (a) has not been satisfactorily defined (see memorandum by Taylor and Fano), but the roentgen (b) applied to the secondary radiation may be used instead for medical purposes.

REP

There is evident need for a unit to describe the energy transferred locally to a material by the primary radiation in the form of heat, ionisation, atomic and molecular excitation, chemical energy and electrolytic dissociation. This need may be met by the rep if it is defined as a unit of energy per unit mass and equal to a specified number of ergs per gram. The magnitude of the unit would thus be defined with an accuracy limited only by the accuracy in the definition of the erg and the gram. The conversion factor would be defined absolutely by agreement. (It may however be difficult to reach such an agreement. The number 83 is rather firmly established in practice but it seems to have no other merit, since it does not correspond accurately to the energy conversion in soft tissue produced by an irradiation of one roentgen of soft x rays. The use of a round number, such as 100, would have advantages arithmetically. My own preference is to dispense with the rep entirely and be content with ergs per gram.)

The use of the expression "roentgen equivalent physical" should be discouraged because the unit is in no sense whatever equivalent to the roentgen. The term rep should be regarded as the full name of the unit and not as an abbreviation for the other expression.

Some may feel that the rep should not be defined in terms of ergs per gram, but in terms of roentgens per gram representing the energy conversion produced by an irradiation of one roentgen of soft x rays. One objection to this is that it would limit its use to soft and medium hard x rays since the roentgen has not been defined for other kinds of high energy primary radiation.

NEUTRONS PER SQ.CM. PER SEC., QUANTA PER SQ.CM. PER SEC., ELECTRONS PER SQ.CM. PER SEC., etc.

These terms are now in common use in describing the treatment of biological material in the case of high energy radiation, for which the roentgen and the rep have not been satisfactorily defined. This practice should be retained at least for the present.

REM

The rem cannot be regarded as a physical unit because it is incapable of definition in terms of more fundamental physical units or measurement procedures. As it is currently used, it is really an abbreviation for a statement by the author of a paper that in his judgement the particular biological effect which he is considering is the same as would have been produced in the material by so many roentgens of soft x rays. It is much more informative to the reader to make such statements in precise terms rather than to speak vaguely in terms of rems. For this reason, the rem should not receive the support of any responsible committee on units at the present time. It may be reconsidered, if in the future it has been established by usage on a more accurate basis.

DOSE

The term dose is commonly used in referring to all the physical magnitudes which are measured in terms of the various units discussed above. However unsatisfactory this practice may appear to many, it is too firmly established to alter. Where it is desirable to use distinctive terms, the following are suggested.

Irradiation

It is suggested that this term be applied to those quantities which are expressed in roentgens including both the (a) and (b) usage discussed above.

Energy Conversion

It is suggested that this term be applied to those quantities which are expressed in reps as defined above.

Flux

It is suggested that this term be applied in referring to quantities expressed in terms of the number of neutrons, quanta, electrons, protons etc. per sq.cm. per sec. as discussed above.

Energy Flux

It sometimes happens, in attempting to calculate the number of reps or attempting to predict the biological effects, that it is desirable as an intermediary step to evaluate the total energy of the particles or quanta traversing unit area per unit time (as in the case of fast neutrons). Convenient units for this purpose are electron volts per sq.cm. per sec.

G.C.L.

After the meeting, Dr. Newell sent Dr. Condon, Director of the National Bureau of Standards, the following brief memo covering the discussions at the November 30 conference. Presumably, this was because Condon had joined with Curtiss in promoting the introduction of the term "Rutherford"--an abortive effort.

Memorandum for Dr. Condon:

We are sorry not to have had the benefit of your discussion at Sunday's meeting before the Commission on Units of the American College of Radiology. The list of interested persons attending is appended.

In conformity with my firm intention, no action was taken at this meeting and no action can be taken by the College before its next meeting in February. The day was given over to exchange of ideas among those attending. Some of us have talked over the matter of units further among ourselves and quite informally, particularly Dr. Failla, Laurence, Marinelli, Taylor, Parker and Corrigan.

The general feeling is that the roentgen ought to be left untouched. The difficulties of its extension into the megavolt region had better be solved by talking about specific energy absorption. But it would be unwise to redefine the roentgen to become itself a unit of specific energy absorption, even though most radiotherapists might have difficulty seeing that this change makes any practical difference in their work.

It seems wisest at present to use the units we have, and give the specific energy absorption in ergs per gram for instance, rather than attempt to name a new unit. There is pretty widespread doubt if any unit of specific energy absorption ought to be tied to the roentgen, even if we should later decide to name one; i.e., reps and rhegma, which are tied to the roentgen, may be off the most desirable road.

There is also widespread agreement that the curie should not be redefined. If very many workers with isotopes have unlawfully extended its use outside the radium family, that cannot now be undone, and although some confusion is widely evident, nevertheless this is less than the confusion that would result from making such a change in its definition as to try now to bring past usage into official acceptance.

Some of us see a positive disadvantage in the use of the curie outside the radium family. Very many doctors have used radium and radon and think they know what a millicurie is. It is the amount of radon that you use in place of radium, with correction for its rapid decay. There will be many of them who think they know what a millicurie of P-32 is, and they'll be wrong, and dangerously wrong. The extension of the curie makes some sense in the basis of measurements, but there is no correlation whatever in biologic effect.

It would seem desirable to give quantities of radioactive nuclide in terms of disintegrations per second and call it just that. Some of us thought it might be all right for Oak Ridge to send out the samples measured in number of atoms. But the important point is to use a measure that will be completely divorced from pre-conceived therapeutic notions on the part of the users. We can't stop the use of the curie just like that, but we can insist on using always the elementary and unequivocal expression, disintegrations per second, ourselves, and so guard against any possibility of being misunderstood.

If, later, a named unit of activity (disintegrations per second) should be adopted to make the saying and writing easier, that ought consciously to be put off until two things are assured, namely: the educational value of stating the disintegrations per second in so many words has become less acutely needed, and the danger has passed that the old-time workers will simply memorize the conversion factor for their old curie, to the new unit, thus retaining to a degree the present misleading qualities of the curie as a unit and refusing to reeducate themselves by calling a disintegration a disintegration.

We think therefore that the adoption of the rutherford or any other named unit of activity ought to be avoided for an indefinite while.

In line with our feeling that no new unit should be tied to the roentgen, but that rather we should talk more about energy in the standard physical units, we have thought that it would be wisest to name a source in terms of its energy emission. If one measures samples of gamma emitters by their gamma-ray activity, one does not in practice link the gammas one measures to the therapeutic effect to be obtained via one's clinical experience in radium therapy--and if one tries to do so it leads to dangerous blunders. What one does is compare one's unknown sample with a standard of the same material. This will often be indirectly, via a permanent radium standard. The readings will just be scale readings, of course, and need never be translated to roentgens.

The size of the sample can be expressed in number of atoms, number of disintegrations per second, or in gamma emission. The latter could be in watts if one wishes to use fundamental units, or in ergs per second. We see a good many disadvantages to rhm as a unit of source activity.

R.R.N.

Renewed International Discussions (1948-1949)

In 1946, Taylor began to renew contacts with his European colleagues who had survived the war years. Among these were Walter Binks and W. V. Mayneord, in England, both of whom had been very active in the area of radiation measurements and radiation quantities and units. Although they had not been directly involved in the ICRU or ICRP up to that time, both were key figures in the British radiation measurements field. Taylor's objective was to revise and regroup the two International Commissions on which he held positions at the time, as Secretary of the ICRU and Acting Secretary of the ICRP. In this connection, Binks and Mayneord visited the United States in the late 1940's. For details of the re-establishment of the commissions, see Taylor, 1979, 7-205 to 7-210.

In May 1948, Mayneord sent Taylor advance copies of a study made by the British Committee for Radiological Units entitled "Memorandum on a Measurement of Ionising Radiations for Medical and Biological Purposes," which is reproduced below (BRU/13, May 1948). Having followed the U.S. activities along these lines, the British were very anxious to obtain the U.S. reactions to their proposals. Also, Dr. G. Failla had visited with the British Committee, which further strengthened the liaison between the committees in the two countries. In addition, Binks and Taylor, who had assumed responsibility for the radiation programs at the National Physical Laboratory in Teddington and the NBS, respectively, were actively exchanging information from the viewpoint of the two national laboratories. (At this point the Physikalisch-Technische Reichsanstalt in the Russian Zone of Berlin was defunct. A new organization and laboratory was in the process of being established in Braunschweig, West Germany, to be known as the Physikalisch-Technische Bundesanstalt. It was still 2 or 3 years before its radiation programs were effectively reorganized.)

Unfortunately, the British Units Committee's request for prompt U.S. reactions to BRU/13 came at the very time the Standardization Committees were being reorganized and shifted to the American College of Radiology. Thus, the real effort on studying the British proposals fell to the National Bureau of Standards and to members of the old RSNA Standardization Committee, although they were not yet officially organized as a Committee of the ACR.

Nevertheless, the mechanics of the study were handled by the American College of Radiology Commission on Radiological Units, Standards, and Protection (CRUSP), under the Chairmanship of Dr. R. R. Newell. This was fortunate for he was the one radiologist in the United States who had the best understanding of the problems of radiation measurements and physical quantities and units. At that time, members of CRUSP had to be Fellows of the College and radiologists; physicists were not allowed to be members. Because of this situation, Newell, by informal arrangement, left the whole matter to the old RSNA Standardization Committee under Taylor's chairmanship. In spite of the confusing situation, everyone worked together in harmony, and very useful results came out of the working combination, particularly that between the National Bureau of Standards and the radiological organizations.

The British report (BRU/13) proved to be a well prepared and very useful document. It was in many respects in close agreement with the positions already established in the United States. Following is a copy of the British paper:

BRITISH COMMITTEE FOR RADIOLOGICAL UNITS
BRU/13 : MAY, 1948

Memorandum on the Measurement of Ionising Radiations for Medical and Biological Purposes

Technical developments during the last few years have made necessary the measurement of a wide variety of ionising radiations for medical and biological work. It is important for the scientific development of radiation therapy, as well as for fundamental radiobiology and the protection of workers against the hazards associated with the use of ionising radiations, that if possible some common system of measurement be developed which will enable the clearest and simplest correlation of biological and physical phenomena to be obtained.

Over a period of many years the röntgen has established itself as a unit of the greatest value for the measurement of X-ray and gamma-ray dose. It is now generally agreed, however, that the röntgen is adequate only within a limited range of X-ray and gamma-ray quality, and does not meet the wider need. In approaching the problem of redefining the röntgen, or alternatively defining a new unit of dose, the British Committee for Radiological Units has considered it essential to aim at a uniform system of dosimetry for the whole range of ionising radiations, equally applicable to sources of radiation external to or incorporated within the medium in which it is desired to specify the dose. The present memorandum is the considered opinion of the British Committee for Radiological Units on the alternative solutions to the problem, and states a preference upon which comment is invited.

In July 1947, a document was prepared by members of the staff of the National Bureau of Standards, Washington, recapitulating reasons why the röntgen as at present defined is inadequate for the measurement of very high voltage X radiation. Since this document was widely circulated and analyses clearly the concept of dose as applied to ionising radiations generally, we shall assume familiarity with its contents, and state only summarily what appear to us to be the principal points now to be considered.

I. We regard it as established that outside the restricted range of X and gamma radiation used hitherto in therapy it is not possible to devise a unit which is a measure both of the primary radiation incident at a given point, and at the same time of the physical quantity underlying the changes which take place in the medium at the point under consideration, as a result of the exposure to radiation. Since a choice of the quantity to be measured is necessary, the British Committee has no hesitation in deciding in favour of the second alternative. It is thereby implied that if an ionisation method be employed for the measurement of dose the conditions of measurement must be such that the observed ionisation characterises the corpuscular radiation traversing the medium at the point under consideration. That is, the measurements are considered to be made in a cavity type ionisation chamber having walls of negligible thickness or in an extrapolation type ionisation chamber.

In addition to the restricted X and gamma-ray phenomena, an entirely new set of problems has arisen from interest in the biological effects of a wide range of new particles, producing their effects some-

times by new mechanisms. For example, high energy neutrons set in motion recoil light nuclei, particularly protons, which provide, in tissues, the main mechanism of energy absorption. High energy protons themselves release electrons and also, particularly at very high energies, cause many nuclear disintegrations of complex type ; high energy alpha particles also produce intense ionisation and complex nuclear disintegrations.

Neutron dosimetry raises problems other than those hitherto encountered in the measurement of X or gamma rays in that the biological effects depend greatly on the presence of specific elements, particularly hydrogen and nitrogen. For instance, the ionisation current in a chamber may be fairly accurately proportional to the percentage of hydrogen in its walls when exposed to fast neutrons. The close correlation and even approximate equality of energy absorption in air and soft tissues (corresponding to their approximately equal atomic numbers) so far as X and gamma rays are concerned no longer holds, and if it were intended to measure neutron flux, a more appropriate medium in which to measure is water or some substance having a chemical composition similar to that of soft tissues rather than air.

It would appear that the only hope of correlating the biological and physical effects of these various types of radiation would arise from a measurement of the energy absorbed in the vicinity of the point of interest in the given medium. In general, the measurement would still be made by observing the ionisation in a small cavity within the medium of interest and, if desired, endeavouring to deduce from this ionisation the energy loss in the appropriate element of volume by the ionising particles crossing it.

II. From the standpoint of biophysical interpretation, it will generally be desirable to know the total energy in all forms dissipated by the ionising particles per unit mass of the medium at the point under consideration. This raises the question as to whether a new unit of dose is required at all, since the quantity of greatest interest can be expressed in ergs./gm., thus employing only the fundamental units of physics.

The use of an absolute energy unit is by no means new and was indeed suggested in the earliest days of X-ray measurement. Important experimental work on the subject was carried out many years ago, and while preferring as a practical measure the ionisation method of measurement, many workers have realised that the röntgen was in essence to be regarded as a halfway house to an absolute energy measurement by a somewhat indirect route.

The advantages of an absolute energy system have been many times discussed, and lie in the correlation made possible between physical, chemical and biochemical phenomena underlying biological and clinical effects. The use of an absolute energy system enables theoretical investigations to be made at a more fundamental level.

It is significant that in attempted dosimetry of artificial radioactive substances the procedure adopted is almost always to calculate the energy absorption in ergs./gm. of tissue from the beta- or gamma-ray sources employed and then to attempt a correlation on the basis of the known energy absorption when one röntgen is

delivered to one gramme of air or water. The approximate soundness of this idea is already established empirically in one or two instances (for example, P32 and Na24) in which such calculations predict at least the order of magnitude of the biological changes produced by these substances when widely diffused in the body in comparison with whole body X or gamma irradiation.

In the study of neutrons and alpha particles a similar approach is successfully employed, though here, on account of the effects of specific ionisation, less direct correlation has hitherto been possible.

It is clear that even the measurement of energy absorption in absolute energy units will not solve in detail the important problem of correlation with biological effect. It is well established that specific ionisation along the track of ionising particles is of significance and a given amount of energy absorbed, say from high energy electrons, will not necessarily produce the same effects qualitatively or quantitatively as the same amount of energy absorbed from alpha particles. Indeed, it now seems clear that the biological complexity is such that no physical system of measurement can hope to give clear-cut simple biological correlations with dose in all circumstances. Such general correlation is in any case impossible in view of the influence of the time factor, necessarily and rightly excluded from the fundamental method of measurement.

It is tempting, from the standpoint of pure physics, to adopt immediately as unit of energy absorption, ergs./gm. of material. No new unit is required, and the theoretical foundations are fully laid. Numerical conversions into other units likely to be used in calculations (such as Mev./gm.) are carried out by well-known numbers, and there can be little doubt that from a purely physical point of view this is the most acceptable theoretical solution. Ergs. per gramme is, indeed, the fundamental unit of dose and has been well recognised as such for many years. The question is therefore essentially a practical one of actual measurement and convenience in therapeutic and experimental practice.

In view, however, of the established position of the röntgen, the place which it occupies in biophysical thought, and the fact that for a long time to come biophysicists, as well as radiotherapists, are likely to find it convenient to measure therapeutic X rays and gamma rays in röntgens, it seems desirable to the British Committee to define a new unit which shall be at the same time :

(1) Related through known or measurable physical constants to the energy dissipated by the ionising particles in the actual medium under consideration.

(2) So defined that for all ionising radiations an exposure of unit dose implies approximately the same absorption of energy per unit mass of tissue as exposure to one röntgen of X or gamma radiation.

III. There are already in the literature in addition to ergs./gramme two units of dose and one unit of absorbed energy. The units of dose are (a) the American " röntgen-equivalent-physical " (rep) and (b) " the Energy Unit." The unit of absorbed energy is the " gramme-röntgen." All these units have been defined with the above considerations in mind and are related as follows :

(a) *The röntgen-equivalent-physical and gramme röntgen.* The energy absorbed per unit mass of tissue exposed to a dose of one " rep " is equal to the energy absorbed per unit mass of air exposed to 1 röntgen (approximately 84 ergs./gm.). The gramme-röntgen is defined as the real energy conversion when 1 röntgen is delivered to 1 gramme of air.
1 rep is thus exactly synonymous with 1 gramme-röntgen/gm. of tissue.

(b) *The Energy Unit.* The energy absorbed per unit mass of tissue exposed to a dose of one " energy unit " is equal to the energy absorbed per unit mass of water exposed to 1 röntgen (approximately 93 ergs./gm.).

It follows from these definitions that water or soft tissue exposed to 1 röntgen of electromagnetic radiation, not appreciably photo-electrically absorbed in tissue, receives a dose of 1.1 gramme-röntgen/gm. or 1.1 rep, but a dose of 1.0 energy units.

In view both of the large body of biological and clinical information already correlated with X and gamma-ray doses expressed in röntgens, and of the likelihood that commercial equipment calibrated in röntgens will continue to be used within the range of X-ray and gamma-ray qualities for which it is suitable, it would thus appear preferable to adopt 93 ergs./gm. rather than 84 ergs./gm. as the unit of dose, so that the X-ray and gamma-ray energy absorbed per unit mass of an aqueous solution or of soft tissue exposed to a given number of röntgens shall be exactly equal to that resulting from exposure to the same number of units of any other kind of ionising radiation.

On the other hand, the advantages of choosing 84 ergs./gm. as the fundamental unit would be that the gramme-röntgen has already been used widely in practice and enables comparison of therapeutic techniques to be made over a wide range of conditions. Also the gramme-röntgen per gramme is, as explained, the exact equivalent of the American " rep," so that no changes would have to be made in numbers expressed in " reps." At very high voltages, of the order of 50 Mev., air and soft tissues are alike as regards absorbing power and the difference in energy absorption in air and water will probably disappear. Moreover, air is likely in practice to be the intermediate measuring medium and strict proportionality between ionisation and gas pressure will only be maintained if gas and walls of an ionisation chamber have the same atomic number.

In balancing these advantages and disadvantages, the Committee is of the opinion that the simpler numerical relationships resulting from the adoption of the energy unit (93 ergs./gm.) justify its selection.

IV. While interpretation of biophysical phenomena requires a knowledge of energy absorbed in ergs./gm., calorimetric measurements of dose are not in general practicable, and the measurement of the ionisation produced in air, or less commonly in a gas mixture having the atomic composition of the irradiated tissue, is likely to remain the basis of practical radiation dosimetry. It is therefore now necessary to decide whether it is preferable to define dose in terms of the *measured quantity*—gas ionisation—or the *significant*

quantity—ergs./gm.—in the exposed medium. The British Committee incline to the view that it is wiser to choose a measured quantity as the unit, and offer the following definition for discussion. The symbol J is temporarily used for the unit of dose inasmuch as it is commonly used for ionisation, and not likely to be confused with the symbol for any other physical quantity.

(1) The J is the internationally agreed unit of dose for all ionising radiations.

(2) One J has been received at any point in a medium, when the ionisation which would have been observed in an infinitesimal cavity containing the point is 1.58×10^{12} ion pairs per gramme of air enclosed in the cavity.

(3) The relation between the J, which is applicable to all ionising radiations, and the röntgen, which is usefully applicable to a limited range of X or gamma-ray qualities only, is as follows :
Air which has been exposed to 1 röntgen of electromagnetic radiation not photoelectrically absorbed will have received 1.02 J units, while water or soft tissues will have received 1.0 J units.

Note

1.58×10^{12} ion pairs per gramme of air corresponds to the absorption of 93 ergs./gm.

The accompanying table illustrates by reference to a variety of dosimetric problems the relation which would exist between the observed ionisation, the dose and the energy absorbed in the medium in ergs. per gramme, if the definition given above were adopted.

TABLE OF EXAMPLES

A. *Calculation for the case in which the dose is measured in röntgens.*
The energy absorption resulting from exposure to 1 röntgen is :
The energy absorbed per unit mass of air = 84 ergs./gm.

The energy absorbed per unit mass of water $= 84 \times \frac{n_{water}}{n_{air}} =$ 93 ergs./gm.

The energy absorbed per unit mass of any medium $=$

$$93 \frac{(\sigma_a + \tau + \pi_a)}{\sigma_a} \times \frac{n_{medium}}{n_{water}}$$

Where π_a is the real absorption coefficient due to pair formation. All coefficients are per electron.

B. *Dose measured in J units by means of an infinitely thin wall chamber.*
1 J = $1 \cdot 58.10^{12}$ ions/gm. of air in the cavity.
Energy absorbed per gm. of medium.

$$E_m = [\rho_m]_{air}^{medium} W_{air} \times 1 \cdot 58.10^{12} \text{ eV/gm.}$$

$$= [\rho_m]_{air}^{medium} W_{air} \times 1 \cdot 58.10^{12} \times 1 \cdot 60.10^{-12} \text{ ergs./gm.}$$

$$= [\rho_m]_{air}^{medium} W_{air} \times 2 \cdot 53 \text{ ergs./gm.}$$

where $[\rho_m]_{air}^{medium}$ = mass stopping power of the medium relative to air
and W_{air} = average energy in eV lost by the ionising particles per ion pair formed in air.

Now $[\rho_m]^{\text{medium}}_{\text{air}} = [\rho_e]^{\text{medium}}_{\text{air}} \times \frac{n_{\text{medium}}}{n_{\text{air}}}$

where n=number of electrons per gm.

$[\rho_e]^{\text{medium}}_{\text{air}}$ =stopping power per electron of medium relative to air

Hence $E_m = [\rho_e]^{\text{medium}}_{\text{air}} \times W_{\text{air}} \times \frac{n_{\text{medium}}}{n_{\text{air}}} \times 2.53$ ergs./gm.

C. *Medium voltage X rays and gamma rays.*

In the particular case of *water*

$$\rho_e = 1.02 \qquad \frac{n_{\text{medium}}}{n_{\text{air}}} = 1.11$$

$\therefore$ $E_m = 1.02 \times 32.5 \times 1.11 \times 2.53 = 93$ *ergs./gm.*

as intended by the definition of the unit.

In the particular case of *soft tissue*

(Composition by weight H=10, C=12, N=4, O=73, mineral=1)

$$\rho_e = 1.013 \qquad \frac{n_{\text{medium}}}{n_{\text{air}}} = 1.10$$

Hence $E_m = 1.013 \times 32.5 \times 1.10 \times 2.53 = 92$ *ergs./gm.*

D. *Megavoltage X-rays.*

Calculations exactly as for medium voltage X rays and gamma rays. A dose of 1 J implies an energy absorption of 93 ergs./gm. up to such voltages that the ionisation due to pair production in oxygen is comparable with that due to Compton recoil electrons, or such voltages that an appreciable part of the total ionisation is due to photodisintegration particles.

E. *Neutrons.*

(a) Dose measured by thin wall chamber containing air

$$E_m = {}_R[\rho_e]^{\text{medium}}_{\text{air}} \times {}_RW_{\text{air}} \times \frac{n_{\text{medium}}}{n_{\text{air}}} \times 2.53 \text{ ergs./gm.}$$

where now ${}_R[\rho_e]$ and ${}_R[W_{air}]$ refer to recoil atoms instead of electrons.

(b) Dose measured in thin wall chamber containing a gas having the same elementary composition as the medium

$E_m = {}_RW_{gas} \times 2.53$ ergs./gm. per $1 \cdot 58.10^{12}$ ions/gm. formed in the gas.

Since the J unit is defined in terms of the ionisation that would be produced per gm. of *air* the value of E_m must be derivable from the dose by the same formula as in (a) above.

$$\therefore \text{ Dose in J units} = \frac{J_m}{1 \cdot 58.10^{12}} \times \frac{{}_RW_{gas}}{{}_RW_{air}} \times \frac{n_{air}}{n_{medium}} \times \frac{1}{{}_R[\rho_e]^{\text{medium}}_{\text{air}}}$$

where J_m is now the ionisation per unit mass of the gas.

F. *Distributed radioactivity.*

(a) If measured by ionisation methods using thin walled chamber containing air, the calculation of the energy absorbed per gm. of medium is exactly as for X and gamma rays.

(b) If computed from the activity per gm. of the medium and the mean energy emitted per disintegration

One microcurie destroyed per gm. of tissue delivers

$$E_m = 0.0854 \, \bar{E} \, T_{\frac{1}{2}} \text{ ergs./gm.}$$

where $\bar{E}$ is the mean energy emission per disintegration $T_{\frac{1}{2}}$ is the half-life in seconds.

Hence the dose in J units is

$$\text{Dose} = \frac{0.0854 \, \bar{E} \, T_{\frac{1}{2}}}{[\rho_e]^{\text{medium}}_{\text{air}} \times W_{\text{air}} \times \frac{n_{\text{medium}}}{n_{\text{air}}} \times 2.53}$$

which for soft tissue reduces to $\frac{0.0854 \, \bar{E} \, T_{\frac{1}{2}}}{92}$

Hence Dose $= 9 \cdot 28.10^{-4} \, \bar{E} \, T_{\frac{1}{2}}$
if $T_{\frac{1}{2}}$ is measured in seconds

or Dose $= 3.34 \, \bar{E} \, T_{\frac{1}{2}}$
if $T_{\frac{1}{2}}$ is measured in hours.

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Also, in the early spring of 1948, the RSNA Standardization Committee prepared a similar document which it planned to have published as an extended Letter to the Editor of Radiology. The statement appears below:*

*Bear in mind that our knowledge and understanding of radiation quantities and units developed rapidly after the mid-1950's. However, it is of interest to record some of the intermediate steps in the development of our current concepts. This is a useful example.

The Roentgen and Other Physical Units of Radiation Therapy
June 1948

Sir,

The use of very hard x rays and other forms of radiation for the treatment of disease has presented a difficult problem of finding satisfactory physical units for purposes of dosimetry. The roentgen has served very well in use with medium hard x rays; that is, with filtered x rays produced at between 100 and 1000 (?) kilovolts. It is less satisfactory for use with harder x radiation, gamma radiation and other forms of radiation. This has led to various suggestions, either to alter the roentgen so that it has wider application, or to devise new units for use with these other forms of radiation.

The problem was the subject of a round table discussion arranged by the American College of Radiology, and of many informal conversations during the meeting of the Radiological Society of North America in Boston. It is of great importance to the clinical radiologists who might use these units in their day to day work. Some have expressed concern about the possibility of changes in the roentgen which might make present techniques, instruments and depth dose tables obsolete. We felt that it was desirable, therefore, to allay needless misgivings and at the same time indicate the limitations in the use of the roentgen and the nature of the problem of measurement of very hard radiation.

It may be said at once that nearly everyone who took part in the recent discussions felt that the present method in the measurement of medium hard x rays should be disturbed as little as possible. Many radiologists are accustomed to making measurements with dosimeters on the skin surface and determine the depth dose by the use of tables which have previously been prepared by measurement in a phantom. They will naturally wish to continue in this manner using the same instruments and the same depth dose tables for x rays produced at less than 1000 (500?) kilovolts.

In attempting to use the same procedure with very hard x rays, such as those emitted from a 30 MeV betatron, fundamental difficulties are encountered.

In the first place, it is not possible to measure the x radiation in such a way as to comply with the definition of roentgen. The thickness of the wall of the dosimeter must exceed the ranges of the fast electrons produced in it by the incident x rays. This means that the dimensions of the ionization chamber must be very large, in fact larger than any tumor which is likely to be treated. The instrument, therefore, is far too bulky for practical use. There is, furthermore, an even more serious difficulty of principle. This is that the necessary thickness of the walls is so great as to cause a substantial attenuation of the x rays themselves. Thus the ionization produced in the chamber is not so much related to the x rays flowing through it as the x rays flowing into the external wall surface. In the limit of extremely hard x rays, at 100 Mev and beyond, the shower production phenomenon sets in and no equilibrium between the primary x rays and their ionizing secondaries is ever achieved.

Secondly, even though the problem of measuring the very hard x rays was solved by resorting to a unit which is a modified roentgen, the number of such modified roentgens would not be proportional to the quantity of energy absorbed locally in the tissue, and it would not be very helpful, therefore, in predicting the biological effects. This difficulty was not serious in the case of medium hard x rays because the local energy transfer was closely proportional (except very near tissue boundaries) to the roentgen measurement of the x rays, and it was possible to correlate the biological effects directly with the number of roentgen of x-rays.

A third consideration is that the biological effects at distances of less than a few centimeters below the skin surface are caused chiefly by fast electrons from the

filters, target, and other parts of the betatron. Measurement of the x rays alone, therefore, will not determine the effects of the treatment.

These three difficulties can be avoided to a considerable extent by disregarding the roentgen measure of the x rays, and making a different kind of measurement. For this purpose, a thin-walled ionization chamber is used instead of the thick-walled instrument which is required for the measurement of the x rays. The chamber may be small compared to the dimensions of the tissue and can readily be placed between the treatment cone and the skin surface in the usual manner or immersed in a phantom. The ionization measured in it is closely proportional to the energy transferred to the immediately adjacent tissue or phantom material, including that which is contributed by fast electrons from the window and filters of the betatron, or from any other surrounding object.

It will be seen from the foregoing that we are concerned with three physical magnitudes, namely:

1. The ability of the x rays to ionize air in a thick walled chamber or, which is the same, in a free air chamber i.e. "the roentgen measure of the x rays".

2. The ability of the fast electrons (both those produced by the x rays in the tissue and those reaching the tissue from the betatron or other external sources) to ionize air in a thin walled chamber, which we shall refer to for brevity as "the ionization measure of the fast electrons".

3. The energy left in the tissue by the fast electrons in passing through it, which we shall call "the local energy transfer".

The remarkable thing is that it has not been necessary thus far to emphasize the distinction between these quantities. This is because of the special circumstance that they are proportional when we are dealing with medium hard x-rays in the tissue (at a place not too close to the skin surface or the tissue boundaries). It is easy therefore, to correlate the roentgen measurement of the medium hard x rays with the biological effects under these conditions.

In the case of very hard x rays (and even with medium hard x rays when very close to the tissue boundary) the "roentgen measure" of the x rays, even if it were significant would not be proportional to "the ionization measure of the fast electrons". It would, therefore, <u>not</u> be proportional to the "local energy transfer" and could not be correlated directly with the biological effects.

On the other hand, "the ionization measure of the fast electrons" <u>is</u> closely proportional to the "local energy transfer," <u>anywhere</u> in the tissue and with <u>any</u> quality of primary x rays harder than 100 kilovolts, and can therefore be used for correlation with the biological effects.

Is it necessary for the clinical radiologist to be familiar with all of these three concepts and use them in the discussion of his work? Many who use only medium hard x rays have not felt this need, but for those who use very hard x rays and are faced with a still unsolved dosimetry problem a clearer understanding of dosage measurement seems essential to avoid errors. This requires a familiarity with all three of the above concepts.

Since the "roentgen measure of x rays" loses its significance in the higher voltage range, it has been suggested that we dispense entirely with it in this range and rely on the other measurements. However, some measure of x rays is still required for the specification of x-ray equipment and the description of protective shielding. Some such measure will probably be developed and standardized in a not too remote future but it is not likely to be closely related to the roentgen measure. The attempt will probably be made to measure the important characteristics of an x-ray beam such as the flow of x-ray photons or the energy flux carried by them without trying to measure all the ionization produced by their secondary electrons. At any rate an adequate standardized measurement of a multi-million-volt x-ray beam by any accessible method is an outstanding problem for early action.

The third of the concepts listed above, namely the "local energy transfer" is the most pertinent one for the quantitative characterization of the biological value of a treatment. It is already predominantly used in the individual thinking of workers in the field. It is also applicable to the dosimetry of all ionizing radiations other than x rays and gamma rays such as, for example, neutrons. For these reasons it seems likely that it will form the basis for a specification of tolerance levels. For the same reasons a majority among us and among the persons we have consulted feel that the concept of "local energy transfer" should come to be considered as the central one in the future radiological work with hard x rays and other ionizing radiations. They are

also inclined to advocate that this trend should be sanctioned by the next International Congress of Radiology, perhaps by the adoption of a standard unit of energy left in tissue by ionizing radiations passing through it.

Before proceeding to discuss in some detail the possible choices for a unit of this type, it should be emphasized that the second of the concepts introduced above, namely "the ionization measurement of the fast electrons", has very considerable importance. As already pointed out, this quality is simply directly related to the "local energy transfer" by x rays of any quality. At the same time it is more directly accessible to direct experimental determination by existing techniques than the local energy transfer. Finally, it is more closely related to the already established concept of "roentgen measure of x rays" so that it could be more easily assimilated by a larger number of workers.

The "ionization measure of the fast electrons" can always be expressed in terms of electrostatic units of charge per gram of air. Many feel that it is desirable to have a more convenient practical unit to describe this quantity. A practical unit with a distinctive name would have the advantage of brevity. If the size of the unit corresponded to one electrostatic unit per gram of air, it would have a further advantage of convenience in that the "roentgen measure of the x rays" and the measure of the "ionization measure of the fast electrons" would be numerically equal when used in therapy involving medium hard x rays. In seeking a name for such a unit for the "ionization of the fast electrons" it has been suggested that the use of the word "roentgen" should be extended for this purpose. Those who oppose this suggestion point out that a new and distinctive name for the unit would avoid confusion with the "roentgen" measure of x rays", and would avoid the necessity of stating each time it is used whether it refers to the x rays or the fast electrons.

Those who have suggested that the "roentgen measure of x rays" should be dispensed with in radiology may, perhaps, favor using the word roentgen for the unit of the "ionization measure of the fast electrons". (If we are not among "those" and do not agree with them, why not delete this?)

The "local energy transfer" can always be expressed in ergs per gram of material exposed to radiation. Here also a practical unit would have the advantage of brevity in discussion. If the size of the unit were approximately equal to the quantity of energy transferred to the tissue by the passage of one roentgen of medium hard x rays, the radiologist would have an immediate appreciation of the approximate magnitude of the biological effects of the exposure. "Ergs per gram" is less familar to him. The "local energy transfer" as we have described it, is a very different physical quantity from the other two which are based on ionization measurements. Its basic units are ergs per gram instead of e.s.u. per gram. In calculating the one from the other, we must take into account the properties of the tissue and of the radiation. We must be careful therefore not to confuse this quantity with the other two and for this reason it would seem desirable that the name for its practical unit should be different. The word "rep" might be suggested for this purpose. The word has already gained considerable currency in representing approximately the energy transferred to the tissue by the passage of one roentgen of medium hard x rays. For the purpose considered here, it would be necessary to define it as equal to a specified number of ergs per gram. If this number is chosen as 83.8, it would conform to the most common usage at the present time. It should be noticed that however the unit of "local energy transfer" is defined it will always be necessary in calculating this quantity, either from the "roentgen measure of the x rays" or from the "ionization measure of fast electrons", to multiply by a constant that is characteristic of the kind of tissue. For example, if the rep is defined in the manner just discussed, the number of reps produced in muscle by 100 roentgen of medium hard x rays is approximately 1.1 times 100.

There is a natural desire to use the same units in neutron dosimetry as in x-ray dosimetry. The units which we have been discussing for the "local energy transfer" would have the same precise physical meaning when the tissue is exposed to neutrons as when it is exposed to x rays, but the quantity would, in general, be difficult to calculate for neutron exposure. It should be remembered also that the correlation that exists between the "local energy transfer" and the biological effect in the case of medium hard and hard x-ray treatment, does not hold unchanged in the case of neutrons. The biological effect corresponding to a given number of ergs per gram of tissue depends on the velocity of the neutrons passing through the tissue because the effectiveness of the energy transferred to the tissue by the fast electrons and by the heavier ionizing particles is different. Moreover, it is uncertain to what extent the "local energy

transfer" should include the late effects resulting from the production of radioactive isotopes in the tissue. Corresponding to the unit of "ionization measure of the fast electrons" for x rays, it is possible to invent a unit for the "ionizing measure of the fast electrons and other charged particles" for use with neutrons. However, no one has yet demonstrated a satisfactory method for measuring the ionization in an air cavity produced by both electrons and heavy particles. The possibilities of other units based on ionization measurements are being investigated at the present time, but it is too early to judge their value. It is generally agreed that the whole problem of neutron dosimetry is so difficult that it is undesirable at this time to propose any formal action in approving special units for neutron dosimetry.

In the view of a majority of the writers, however, these considerations do not detract from the desirability of focussing the attention on the concept of "local energy transfer" which is equally applicable to a broader range of radiation and whose importance is not contingent upon the special technique of ionization measurement. This argument seems to apply particularly to possible international agreements whose repeated modification has proved bothersome and confusing. The "ionization measure of the fast electrons" may well continue to play an important, even though auxiliary, part in practical measurements and informal discussions. The desire that international definitions and units be permanent and unequivocal also makes one inclined to express the fundamental measurements directly in ordinary physical units such as "ergs per gram" rather than in specially defined units of different magnitude. The desire for brevity and adherence to already widespread practices should not becloud the advantage of breaking away clearly from terms which may have been used in different and confusing ways.

K. K. Corrigan, G. Failla, G. C. Laurence, H. M. Parker, L. S. Taylor

When Mayneord visited the United States in July 1948, and spent substantial time at the National Bureau of Standards, he had two prime missions. The first was the re-constitution of the International Congress of Radiology to be held in London in 1950, which involved the reorganization of the ICRU and ICRP (Taylor, 1979, 7-205). His second mission was further discussions of the British and American proposals for quantities and units. These conversations mainly involved Laurence, Failla, and Taylor.

Mayneord's visit was followed by a communication to Taylor, Condon, and others in various countries. Identified as BRU/15 and dated August 20, 1948, this memorandum reminded the recipients that, in accordance with the past procedures of the ICRU, new proposals had to be in the hands of the Commission members at least 6 months before the meeting of the Congress. There were only two survivors of the original Commission Dr. Rolph Sievert from Sweden and Taylor from the United States. Mayneord's communication of August 20, 1948 follows:

(Mayneord to Taylor) BRU/15

This memorandum attempts to set out some of the problems in the measurement of ionising radiations for medical and biological purposes and suggests a possible solution. It must be emphasised that the text is intended merely as a basis for discussion, though the actual recommendations are in a form which the Committee thought might be finally appropriate.

The British Committee is anxious that these matters be discussed and, if possible, conclusions reached well in advance of the proposed International Congress of Radiology to be held in London during the summer of 1950. We would, therefore, be grateful if you would let us know your views before 1st January 1949. If no reply is received it will be presumed that you are in agreement. Should you be a member of a National Committee for Radiological Units would you be good enough to lay the memorandum before your Committee and let us know as soon as possible its reactions.

It is laid down in the Regulations governing the selection and work of the International Committee for Radiological Units (Radiology, Vol. 29, pp. 634-636, November 1937) that a preliminary report shall be published and circularised by the Executive Sub-Committee of the International Committee for Radiological Units to all its members at least six months before the meeting of the Congress. As soon as comments have been received it is hoped that it will be possible to prepare an interim report. You will therefore appreciate the need for prompt action.

Dr. Condon turned all matters over to Taylor who, in turn, discussed the problems with the remnants of the RNSA Standardization Committee and the newly organized CRUSP. Shortly

thereafter, Taylor prepared the following short memorandum concerning Mayneord's visit and circulated it among various individuals expected to take part in future discussions:

Memorandum concerning Dr. Mayneord's visit in Washington in July 1948 and the discussion on the British Memorandum on radiological units:

Advance copies of the recent British Memo BRU/13, May 1948 entitled "Memorandum on the Measurement of Ionising Radiations for Medical and Biological Purposes" were circulated in this country shortly in advance of Dr. Mayneord's visit. Opinion at the X-ray Section of the National Bureau of Standards was in agreement with the first three sections of this memorandum, where the problem is analyzed very tersely, but not with the brief Section IV, where the proposals of the British Committee are formulated. It was noted, in fact, that the earlier sections seem to lead the reader to a different conclusion than the one presented in Section IV. Informal contacts with Dr. Failla and Marinelli indicated their concurrence with the N.B.S. view.

Discussion with Dr. Mayneord indicated that there is hardly any doubt in his own mind that the eventual dose unit should be based on the "significant quantity" (ergs/gm). On the other hand expediency considerations dictated the preference expressed in Section IV to define dose in terms of the measured quantity--gas ionization. It was feared, it seems, that no sufficient body of opinion is prepared yet to swing all the way from the present roentgen unit to an energy unit. He agreed, however, with the concern expressed by American workers lest a new choice of dose unit should again prove to be a temporary one. From this standpoint the adoption of the significant rather than of the measured unit presents an overwhelming advantage.

It appeared then at once that these different considerations could be reconciled by a recommendation that both types of unit be adopted simultaneously. A "significant" quantity such as the "energy unit" or the "rep", should be clearly stated to be the fundamental one. At the same time the use of a gas ionization unit, such as that suggested by the British Committee, should be recommended as a temporary expedient to bridge the gap between the widespread present practice and the new fundamental unit.

No action was taken at the time of Dr. Mayneord's visit concerning this possible solution. It seems of importance now to ascertain whether any substantial body of opinion is prepared to accept a solution of this type. If so, such a solution could be properly formulated and offered in reply to Dr. Mayneord's cover letter of 20th August 1948 (BRU/15), as an amendment of Section IV of the present British Memorandum.

On November 22, Taylor wrote to Mayneord, transmitting copies of a preliminary report which responded to the British communication BRU/15. In his cover letter, he stated that the question of units was undergoing considerable debate in the United States and that he could expect to receive some résumé of the discussion within his deadline. He was uncertain, however, as to the effectiveness of the U.S. Units Committee and pointed out that a number of the members were meeting to discuss the question independently. This group would include Failla, Laurence, Fano, Wyckoff, and himself. Regardless of whether or not formal committee action could be taken, the ideas of that group would be forwarded.

In his letter Taylor also wrote:

"I had a very interesting visit with Dr. Paterson during which we discussed some organizational plans for the Congress. He will take this up with you as soon as he returns to England. Based on earlier discussions with you and with Dr. Christie, he is inclined to simply scrap the old ICRU and ICRP set-up and organize new ones on the basis more along the lines you and I have discussed. After he has seen you, he will get in touch with Dr. Christie to formalize the arrangement."

(Dr. Paterson was to be the President of the International Congress in London in 1950 and Dr. Christie was the President of the Congress held in Chicago in 1937.) Following is the initial reply to Mayneord's letter of August 20, 1948:

Tentative reply to Dr. Mayneord's letter of August 1948 (BRU/15) requesting comments on the British Committee proposals (BRU/13)

November 1948

At the time of your visit here last summer there appeared to be general agreement in this country with the content of the first three sections of BRU/13. No contrary

opinion on this point has reached us in the meanwhile.

Disagreement developed, however, with regard to the substance of Section 4 where there was introduced a definite proposal in favor of a new "measured unit", namely the J. As you know, we have been increasingly concerned over the consequences of past decisions centering the dosimetry upon a "measured unit". This has required a repeated redefinition of the units in the attempt to follow the expanding energy range and to include new types of radiation with the consequent necessity for the modifications of measuring techniques. To avoid the recurrence of such difficulties we are inclined to recommend that the International Congress of Radiology recognize once and for all that the principles of radiation dosimetry must be based upon a fundamental or "significant unit". We believe that the primary term of reference for the quantitative characterization of a radiation treatment should be the "energy dose", that is, the energy absorbed by each portion of material exposed to radiation. This energy dose may be expressed in standard energy units, ergs/g which would allow no doubt as to the interpretation of any statement concerning dose. There might be some temporary advantage in using the "rep" or the "energy unit", provided they be defined as a fixed number of ergs/g.

While stressing the importance of the recognition of a "significant unit" as the fundamental one, we fully share your concern over the facts that direct measurements of the energy dose "are not in general practicable" and that "the measurement of the ionization produced in air" is likely to remain the basis of practical radiation dosimetry". We also appreciate that a drastic change in the basis of dosimetry would be disturbing to the radiologist unless the transition were somehow smoothed out so as not to require a sudden change in his thinking and techniques. As it was tentatively agreed at the time of your visit here, one might then utilize as a secondary unit a "measured unit" based on the number of ion pairs per gram of air in a small cavity. This would have the advantage of providing a secondary measurement which in the range of soft x rays and soft tissue would be numerically equal to a measurement in roentgens. This would be the case of your J.

Since it is generally agreed that the present roentgen should be preserved as a unit of moderately hard x rays, we are then faced with having to deal with three different quantities and the corresponding units. This is not so serious and has in fact some advantages since, whether we like it or not, there are actually three quantities to be considered. The situation might be eased by the introduction of a clearcut statement that the roentgen is a unit of x rays, that the J is a unit of ionizing corpuscular radiation, and that the energy dose is a unit of energy.

To summarize we offer the following statements to replace Section 4 of BRU/13:

(1) The quantity of physical action undergone by a portion of material exposed to radiation shall be characterized as the "energy dose" and expressed as the amount of energy absorbed per unit mass. This quantity alone does not purport to indicate the effect of the energy absorption (such as e.g., the biological damage) as such effect unavoidably depends on other, qualitative, characteristics of the radiation.

(2) The quantity of corpuscular ionizing radiation flowing at a point in a material shall be characterized by the number of ions per gram of air produced in an infinitesimal cavity at the point in question. This quantity shall be measured in XXX and one XXX will represent the production of YYY ion pairs per gram of air. A measurement in XXX shall be acceptable as a term of reference instead of a measurement of energy dose, provided the correlation between the two measurements for a particular material and corpuscular radiation is clearly defined. The measurement in XXX will then be of advantage so long as its accuracy is better than our knowledge of the numerical value of the correlation between the two measurements.

(3) The quantity of X or gamma rays flowing at any point shall be measured in roentgens according to the definition of this unit given by the 1937 Congress. The range of application of measurements in roentgens will be limited by the practical feasibility of meeting the requirements of the standard definition. A measurement in roentgens shall be acceptable as a term of reference instead of a measurement of energy dose, provided the correlation between the measurement in roentgens at one point and the measurement of energy dose at the surrounding points is clearly defined. The measurement in roentgens will then be of advantage so long as its accuracy is better than our knowledge of the numerical value of the correlation between the two measurements.

Meanwhile, a crash effort was undertaken to crystallize the thinking in the United States on this subject. Because, at the time, Taylor was heavily engaged in major organization problems both at the AEC and NBS, Dr. Ugo Fano and Dr. Wyckoff, both staff members of the X-Ray Section, spearheaded the effort and brought the program to a satisfactory stage by the end of December.

Prior to that time, Fano and Wyckoff had assembled all available Committee members at NBS on November 23rd and an agreement was reached on a draft statement which was circulated to the full membership for review and comment prior to December 20th. A revised formal statement and excerpts of specific comments of two members were transmitted to Mayneord as follows:

To Mayneord, December 20, 1948
Subject: Radiological Units

This is in reply to your letter of August 20th, requesting comments on the British Committee proposals on radiological units (BRU/13). Many interested persons on this side of the ocean have been thinking on your proposals in recent months and holding informal conversations and correspondence. In particular, Drs. Corrigan from Detroit, Failla and Rossi from New York and Laurence from Chalk River came to Washington on November 23rd for a whole day of discussions on the subject. This letter intends to present the views agreed upon in that session and has also been submitted for concurrence and comments to other workers.

At our session there crystallized and came to definite expression a somewhat new slant on matters of units. We have been faced with the difficulty of reaching a broad agreement on new units and assigning them proper names. We also recognize the possibility for confusion that can result from the introduction--even though only tentative--of a large number of new units. At the same time we find that fundamental units based on the standard physical systems are substantially adequate for our purposes and that their use constitutes sound practice. The use of fundamental units should furthermore be of great help in avoiding any confusion on the significance of measurements and units. We thus find ourselves leaning very heavily away from our earlier tendency to recommend the introduction of one or more new units or to recommend a re-definition of the roentgen. Rather, we feel that the aim of a committee on units should now be to define with precision the significance of the different quantities which are of actual importance in radiation work and the relationships between these quantities. We are then inclined to recommend that the International Congress do not propose any new "derived" unit and also discourage the use of such units in published books and reports. These feelings of ours have considerably influenced the following more specific discussion even though its main points had been rather clear to us for some time.

It was already apparent at the time of your visit here last summer, and it has been generally confirmed since that time, that the presentation of the problem in the first three sections of BRU/13 has been received very favorably and that your efforts in preparing it have been greatly appreciated.

On the other hand, it was also apparent that the substance of section 4 of BRU/13, where there was introduced a definite proposal in favor of a "measured unit", namely the J, was not meeting with general agreement. This disagreement does not seem very deep, however, since the earlier sections of BRU/13 imply that the British Committee themselves would prefer adoption of a "fundamental unit" were it not for certain difficulties of a practical nature. It looks as though the disagreement is a matter of different feeling as to the importance of overcoming practical obstacles.

As you know, most of us have been increasingly concerned over the consequences of past decisions centering the dosimetry upon a "measured unit". This has required a repeated re-definition of the unit in the attempt to follow the expanding energy range and to include new types of radiation with the consequent necessity for the modification of measuring techniques. It is true that the J is so defined as not to be too closely related with any particular measuring technique. Still, its usefulness presupposes that the amount of ionization observable in a small air cavity is, and will remain, quite generally the basis for the estimation of energy absorption which is our main goal. The enclosed excerpts, from informal memos prepared by Dr. Failla and Dr. Rossi, indicate that this assumption does not quite obtain, even at the present time, especially with regard to neutron measurements.

To avoid the recurrence of such difficulties we are strongly inclined to recommend that the International Congress of Radiology recognize once and for all that the principles of radiation dosimetry must be based upon a fundamental or "significant unit". We believe that the primary term of reference for the quantitative characterization of a radiation treatment should be the "energy dose", that is, the energy absorbed by each portion of material exposed to radiation. (It is recognized, of course, that the specification of the energy absorbed is not generally sufficient by itself for predicting biological effects.) This energy dose may be expressed in standard energy units, ergs/g which would allow no doubt as to the interpretation of any statement concerning dose. We understand that the "rep" or the "energy unit" may be favored by other workers and this would not meet substantial difficulty provided they be defined as a fixed number of ergs/g, but, as indicated above, we are not inclined to favor this use.

While stressing the importance of the recognition of a "significant unit" as the fundamental one, we fully share your concern over the facts that direct measurements of the energy dose "are not in general practicable" and that the measurement of the ionization produced in a gas "is likely to remain the basis of practical radiation dosimetry". We also appreciate that a drastic change in the basis of dosimetry would be disturbing to the radiologist unless the transition were somehow smoothed out so as not to require a sudden change in his thinking and techniques. At the time of your visit here, we agreed in favoring the suggestion of a secondary unit as a "measured unit" based on the number of ion pairs per gram of air in a small cavity. This would have the advantage of providing a secondary measurement which--in the range of soft x-rays and soft tissue--would be numerically equal to a measurement in roentgens. In the recent discussion here it was stressed that the same advantage may be achieved in the main by a statement setting forth the relationship between the measurement of ionization in a small cavity and the energy dose. This should be supplemented by an effort to circulate detailed information regarding the numerical factors which relate the energy dose with the amount of ionization observed in a cavity under specified conditions.

In the end, since it is generally agreed that the present roentgen should be preserved as a unit of moderately hard x rays, we find ourselves faced with having to deal with at least three different physical concepts and perhaps with as many units. This is not so serious and has in fact some advantages since, whether we like it or not, there are actually three quantities to be considered. The way of easing this difficulty is to explain the situation as clearly and concisely as possible.

To summarize, then, we offer for discussion the following statement to replace Sect. 4 of BRU/13.

"It is recommended that

(1) The use of the roentgen as a unit of x and gamma-ray quantity should be continued within those limits of radiation quality where it is practicable to meet the present definition.

(2) For the correlation of the radiation dose with the biological or other effects which it produces, the dose shall be expressed in terms of the quantity of energy absorbed per unit mass of the material irradiated, at the place concerned.

(3) The dose thus defined shall be called the "energy dose".

(4) The erg per gram should be the unit of energy dose.

(5) In practical measurements of radiation exposure by the use of ionization chambers the ionization should be expressed in electrostatic units of charge per gram of gas in the chamber.

(6) Such measurement should be made with instruments and under conditions such that the ionization in the gas is produced by substantially the same flow of corpuscular radiation as exists in the material under consideration.

(7) Since the calculation of the energy dose from measurements of ionization requires a knowledge of parameters and variables characterising the radiation and the irradiated material, the International Committee on Units shall promote the compilation and the distribution of the best available data useful for this purpose."

Concurred in by: K. Corrigan, G. Failla, U. Fano, G. C. Laurence, H. H. Rossi, L. S. Taylor, and H. O. Wyckoff.

Enclosure 1. Excerpts from Memo by Dr. Failla

The implication of the infinitesimal cavity requirement has already been mentioned. When the range of the ionizing particles in atmospheric air is long, the requirement can be met by using reasonably small chamber volumes and, if necessary, extrapolating to zero volume. (The walls, of course, must be tissue equivalent.) In the case of fast neutrons in the energy region of practical importance, the range of the heavy recoils (principally O, C, and N) in atmospheric air is extremely short (sometimes less than 1 mm) so that, either the linear dimensions of the chamber volume are made smaller than this, or the air pressure must be correspondingly lower than atmospheric pressure. In the case of most of the practical measurements of neutron dose, the ionization current is entirely too small under these conditions. It should be borne in mind in this connection that very often the chamber volume must be kept within certain limits of size dictated by geometric requirements of the dosage problems under investigation. Accordingly, in general it is well nigh impossible to make measurements of neutron doses in J's by using air as the ionized gas. The alternative is to use ionization chambers in which both the walls and the gas are tissue equivalent. To derive the numerical value of the dose in terms of the J when the gas is not air, requires a knowledge of the relative value of the average energy lost by a particle per ion pair produced in the tissue equivalent gas in the air, and the relative value of the stopping powers of the two gases for the ionizing particle under consideration.

In the present state of the art it is generally assumed that the average energy lost by an electron per ion pair produced in air is 32 to 33 eV. for all electron energies of practical importance. (Considerable uncertainty exists as to the value of this quantity at very low electron energies). For alpha particles a value of 35 eV. is commonly accepted. Little is known about the average energy lost by a heavy recoil (O, N, C) in producing an ion pair in air, but a value of 35 eV. is generally used. The corresponding values for a tissue equivalent gas are known with less accuracy. However, the experimental determination of this quantity is relatively simple. In the case of alpha particles from a thick uranium source, the value is 2 to 10% lower than that for air, depending on the particular gas mixture used to simulate the composition of tissue. In the case of fast neutrons one may take the value for alpha rays and determine the energy absorbed per gram of tissue with sufficient accuracy for most practical purposes (when tissue equivalent walls and gas are used in the ionization chamber).

Using air in a chamber with tissue equivalent walls one has first the difficulty of complying with the infinitesimal cavity requirement (as already discussed) and then the complication caused by lack of reliable data about relative stopping powers of H, O, C, and N recoils in the tissue equivalent material. This is particularly serious since stopping powers vary considerably with the energy of the ionizing particle. Experimental determinations of stopping powers are more difficult than experimental determinations of energy lost per ion pair. Furthermore, even if exact values were available for all particles and energies, one would have to know in addition the average values to use for the energy ranges obtaining in a given neutron beam.

Enclosure 2. Excerpts from Memo by Dr. H. H. Rossi

The objections to the "J" are that determinations in terms of this unit will, in certain cases, be difficult or impossible (within reasonable limits of error), and that it fulfills the objectives set forth in 1) and 2) (of Section II BRU/13) only in a very restricted manner.

An "infinitesimal cavity", as mentioned in the definition and an "infinitely thin wall chamber", as considered in the table of examples, are structures which have to fulfill two basic requirements: A) They must present so little mass that they absorb the charged particles to a negligible extent and B) The product of their mass per square centimeter and their cross section for interaction with the primary radiation must be such that the contribution to the measured radiation by secondaries generated inside these entities is also negligible.* On the other hand, there are practical limitations

*The additional requirement that the sensitive volume have such a size that the radiation intensity be constant throughout, is in practice, usually unimportant.

of an opposing nature. The air cavity must be large enough to furnish a reasonable current at practical radiation intensities and the wall of the ion chamber rigid enough to insure constant volume. In cases where the medium is a solid, and especially if the solid is conducting, the wall difficulties may be largely or completely avoided, but the problems attending the choice of the air cavity are inherent in the "J".

It has been pointed out in detail* that in the case of measurement of the tissue

*"Notes concerning a redefinition of the roentgen unit of radiations," by H. H. Rossi.

dose of fast neutrons around 4 Mev, an error of about 7% is virtually unavoidable, because an air cavity of practical dimensions will, as far as secondaries other than protons are concerned, register the contribution of the oxygen and nitrogen in the air rather than the oxygen, carbon and nitrogen recoils originating in the wall. This error may be expected to increase somewhat with increasing neutron energy, because the relative importance of these contributions tends to increase. On the other hand, there is virtually no information on the range and relative distribution of secondaries at neutron energies intermediate between slow and fast (between 100 eV and 100 KeV). This energy seems to be predominant at certain partially shielded installations (such as outside the enclosure of high energy cyclotrons) and, since measurements in these locations are done at the tolerance level, large cavities are required where, very likely, small ones should be used. Similar considerations will apply for x rays of such low energy that the photoelectrons liberated in tissue have very short ranges.

The objective 1) is that the dose measured in J units be related through known or measurable physical constants with the energy imparted to the medium. The quantities required will be the relative stopping power of medium and air and the W for the charged particles in question. Since at least the former of these is definitely a function of the particle energy, a precise determination requires knowledge of the energy spectrum of the primary radiation and of the interaction cross-section. Even if this information were always available--and in the case of neutrons it usually is not--a computation of the average stopping power would be a tedious job at least. Approximations may, of course, be made, but the limits of error inherent in them are considerable. Throughout the memorandum the relative mass stopping power of water and air are considered to be 1.13. Actually, the value is believed to vary from 1.23 at 5 KeV to about 1.10 at 50 MeV*. As a result, tissue receiving 1 J from a distributed H^3 source will receive about

*Heitler, "The Quantum Theory of Radiation".

10% more energy than in the case in which the 1 J is due to P^{32}.

In the case of neutrons, the variation in stopping power for protons is about the same as the variations for electrons quoted above*, but there are far greater

*Gray, L. H., "The Ionization Method of Measuring Neutron Energy." Proc. Camb. Soc. 40, 72 (1943).

variations in the stopping powers for the heavier recoils, and a calculation of the effective stopping powers is fraught with many uncertainties.

The fact that objective 2)--equal energies absorbed by tissue for equal J units of different radiations--is not strictly met, may be not too important. Nevertheless it should be pointed out that the product of W_{air} and the relative stopping power of tissue and air is about 20% higher for alpha particles of a few MeV than for electrons of the same energy. Therefore, 1 J of alpha rays will represent the absorption of about 111 ergs/gram.

At present the best way of avoiding the difficulties attendant to measurements of tissue dose in the case of neutrons and very soft x rays, seems to be in a scheme in which the gas inside the ionization chamber is also made tissue equivalent. This scheme is, however, much more appropriate for a determination of the energy absorbed in ergs/gram rather than a measurement in terms of J, because such an evaluation would be the exact reverse of the process which has just been described, i.e., it would necessitate a calculation of air ionization in terms of energy absorbed, and suffer from all the uncertainties connected with stopping powers.

It is realized that the measurement of energy absorbed, in the manner outlined, is by no means ideal, primarily because there remain uncertainties in the value of W to be

employed. Nevertheless, it would seem that at present a measurement of energy absorbed per gram of tissue can be performed with greater accuracy than a measurement of ionization in a vanishing air cavity. An exception may be found at high intensity installations where chambers at extremely low air pressures might be successfully operated.

H.H.R.

On December 29, Taylor wrote to Mayneord explaining some of the difficulties that had been encountered in taking formal action on his letter. He pointed out that, in addition to the "Committee Report," a report had been prepared which he believed would receive the concurrence of the leading radiological physicists in the United States. A final draft was being circulated for approval.

While not quite meeting Mayneord's deadline of January 1st, Taylor indicated that the gist of the recommendations would be the adoption of an energy unit based on energy per unit mass and defined without regard to any particular measurement techniques. A practical unit would be defined as follows: "XXX is the dose received from ionizing radiation at a point in tissue when the energy absorbed is 93 ergs per gram at that point." He further stated:

"We have deliberately omitted naming this unit although the 'rep' and 'rhegma' were proposed. Most of the physicists, and for that matter, most of the medical people, prefer the use of the word 'rep' without regard to its previous usage. This was recognized as a possible source of confusion, but it was felt that it was convenient, short, and began with an 'r', thereby relating it phonetically to the roentgen. Rhegma was pushed mainly by Newell and did not find a great deal of favor. The only strong feeling on the subject is that the unit ought to be something short and begin with the letter 'r' for reasons noted above."

While these actions were in progress, the newly organized Commission on Radiation Units, Standards and Protection (CRUSP) was becoming active and held its initial meeting on December 8th. The attendance hinged upon those attending the RSNA annual meeting. Of the 10 attendees, only three, Newell, Failla, and Taylor, had been involved in the earlier discussions. Following is a copy of the minutes of that meeting which, with the formal report, were presented to the ACR.

Meeting of the Commission on Units, Standards and Protection
Wednesday, December 8th, 1948, 2:30 P.M.
Minutes

Dr. Newell read Dr. Mayneord's letter and proposed to gather opinion from Consultants to the Commission on which the Commission can draft an answer to Dr. Mayneord. He stated that he proposed to get a subcommittee to draft this reply, which would then be acted on by the whole Commission and (with endorsement by the Executive Committee of the College) forwarded to Dr. Mayneord.

Dr. Newell stated that he thought it inadvisable that his prejudices too much affect this second meeting of the Consultants on units and advised a temporary chairman for this afternoon's discussion. Dr. Parker was nominated and elected and took the chair.

Dr. Taylor presented a draft gotten out by himself with some others in the East. After considering this it was agreed:

a) The roentgen should be retained in its present definition for those qualities for which it is adapted.

b) Dose should be recorded in terms of the significant quantity, namely specific energy absorption, rather than the measured quantity (exposure). This follows the British argument, but contravenes their recommendation.

c) The unit of dose would be ergs per gram.

d) It may be desirable to adopt a practical unit, namely that dose which is given when specific absorption of radiant energy is 93 ergs per gram. In a footnote two possible names for this are suggested, namely "rep" and "rhegma".

Dr. Newell having returned to the chair, Dr. Taylor was appointed to draft the answer to Dr. Mayneord covering these points and to write arguments supporting this position. He will consult Dr. George Laurence and other interested physicists. Before sending this letter he will give the several members of the Commission opportunity to veto it.

On motion of Dr. Failla, the chair appointed Dr. Taylor a subcommittee of one to conduct correspondence concerning the questions of units to be brought before the International Congress of Radiology in 1950. He is to keep close council with the Chairman in this.

The matter of shoe fitting fluoroscopes was considered again, the College having been approached on the subject by the National Shoe Retailers Association. The Chairman had quoted to them the American Standard Z 54.1, 1946 which permits only 2 r to a foot for such shoe fitting. It was decided that the Chairman should prepare an editorial on the subject to be offered to the Editor of the Journal of the American Medical Association.

R. R. Newell, M.D., Chairman

On February 11, 1949, Taylor transmitted to Mayneord what was hoped to be the United States' last word on radiation quantities and units for consideration at the 1950 International Congress of Radiology. Following is his letter, and the formal recommendations:

(Taylor to Mayneord) February 11, 1949

This is in further reference to my letter of 29 December. Enclosed are 24 copies of the recommendations on Radiological Units which were agreed upon by the Commission on Radiological Units, Standards and Protection of the American College of Radiology at its meeting in December, 1948.

This commission consists of the following persons:

S. Cantril	R. R. Newell
H. Friedell	R. Taft
G. Henny	L. S. Taylor

The following persons have cooperated with the commission and have signified their agreement to the enclosed report.

W. S. Bale	L. D. Marinelli
C. B. Braestrup	K. Z. Morgan
K. L. Corrigan	H. M. Parker
G. Failla	Edith H. Quimby
U. Fano	H. H. Rossi
G. H. Henderson	M. M. D. Williams
G. C. Laurence	H. O. Wyckoff

In addition to this Committee, many other individuals have been considering your proposals contained in BRU-13 and considerable informal conversation and correspondence on the subject have taken place. In particular, two informal meetings to discuss the problem have been held. One was a conference including K. E. Corrigan, G. Failla, H. H. Rossi, U. Fano, H. O. Wyckoff, G. C. Laurence, and L. S. Taylor, held in Washington on November 23. A further meeting was held with H. M. Parker, K. Z. Morgan, G. Failla, M. M. D. Williams, and L. S. Taylor in San Francisco on December 6. These were followed by recommendations made by these groups to the American College of Radiology for final consideration and adoption.

At these sessions there crystallized and came to definite expression a somewhat new slant on matters of units. We have been faced with the difficulty of reaching a broad agreement on new units and assigning them proper names. We also recognize the possibility for confusion that can result from the introduction--even though only tentative--of a large number of new units. On the other hand we find that fundamental units based on the standard physical systems are substantially adequate for our purposes and that the extension of their use in radiology would constitute sound practice. The use of fundamental units should furthermore be of great help in avoiding any confusion on the significance of measurements and units. We thus find ourselves leaning very heavily away from our earlier tendency to recommend the introduction of one or more new units or to recommend a re-definition of the roentgen. Rather, we feel that the aim of a committee on units should now be to define with precision the significance of the different quantities which are of actual importance in radiation work and the relationships between these quantities. We would then be inclined to recommend that the International Congress do not propose any new "derived" unit and also discourage the use of such units in published books and reports. However we are willing to compromise on this point to the extent of accepting a "practical unit", as indicated in the enclosed material. These feelings of ours have considerably influenced the following more

specific discussion even though its main points have been rather clear to us for some time.

It was already apparent at the time of your visit here last summer, and it has been generally confirmed since that time, that the presentation of the problem in the first three sections of BRU-13 has been received very favorably and that your efforts in preparing it have been greatly appreciated.

On the other hand, it was also apparent that the substance of section 4 of BRU-13, where there was introduced a definite proposal in favor of a "measured unit", namely the J, was not meeting with general agreement. This disagreement does not seem very deep, however, since the earlier sections of BRU-13 imply that the British Committee themselves would prefer adoption of a fundamental unit" were it not for certain difficulties of a practical nature. It looks as though the disagreement is a matter of different feeling as to the importance of overcoming practical obstacles.

As you know, most of us have been increasingly concerned over the consequences of past decisions centering the dosimetry upon a "measured unit". This has required a repeated re-definition of the unit in the attempt to follow the expanding energy range and to include new types of radiation with the consequent necessity for the modification of measuring techniques. It is true that the J is so defined as not to be too closely related with any particular measuring technique. Still, its usefulness presupposes that the amount of ionization observable in a small air cavity is, and will remain, quite generally the basis for the estimation of energy absorption which is our main goal.

I hope the delay in getting this material to you has not caused you any considerable inconvenience. It was virtually impossible to get it to you sooner because of our inability to get together the Committee from the American College of Radiology.

We will be very happy to receive your reaction to our proposals and will, of course, be prepared to give the matter any additional study which may be necessary. Official correspondence relating to the attached report may be sent to me and I will see that it is properly brought to the attention of the American College of Radiology and other interested groups on this side.

Enclosure,

REPORT ON RADIOLOGICAL UNITS FOR INTERNATIONAL ADOPTION BY THE COMMISSION ON RADIOLOGICAL UNITS, STANDARDS AND PROTECTION OF THE AMERICAN COLLEGE OF RADIOLOGY AND BY OTHER INTERESTED INDIVIDUALS

I Recommendations

To avoid recurrent difficulties associated with the use of dosage units based on specific measuring systems we are strongly inclined to recommend that the International Congress of Radiology recognize once and for all that the principles of radiation dosimetry must be based upon a fundamental or "significant unit". Several fundamental or significant units of radiation dose have been presented in one form or another. Of these, we believe that the "energy dose"--that is, the energy absorbed by each portion of material exposed to radiation--should be preferred for primary quantitative characterization of a radiation treatment. (It is recognized, of course, that the specification of the energy absorbed is not generally sufficient by itself for predicting biological effects.) This energy dose may be expressed in c.g.s. units, viz. in ergs/g, which would allow no doubt as to the interpretation of any statement concerning dose.

In addition to the basic energy absorption unit (erg/g) we appreciate the value to the sciences of radiology and radiobiology of a practical unit to facilitate the transition from dosimetry in roentgens, where presently applicable, to dosimetry in energy units. This unit should be a derived unit, defined as a certain number of ergs/g.

For this purpose we recommend that one practical unit[1] be defined as the energy dose

1/ While it was agreed that the choice of a name for this unit be left open, two proposals were made to the Committee. These were the "rep" and the "rhegma", both having some priority as to usage but both having the slight disadvantage of having been defined somewhat differently than defined here.

received from ionizing radiation at a point in a material when the energy absorbed is 93 ergs/g at that point.[2] This is approximately equal to the "energy unit" considered in

2/ See the remark further below concerning the numerical value to be adopted.

Section 3 of BRU-13. The difference lies in the fact that the BRU figure was not specified as a fixed number of ergs but was based on energy absorption of 1 gram of water exposed to 1 roentgen of x rays.

While stressing the importance of establishing a "significant unit" as the fundamental one, we fully share the concern of the British Committee for Radiological Units over the facts that direct measurements of the energy dose "are not in general practicable" and that the measurements of the ionization produced in a gas "is likely to remain the basis of practical radiation dosimetry". It is also appreciated that a drastic change in the basis of dosimetry would be disturbing to the radiologist unless the transition were smoothed out to avoid a sudden change in his thinking and techniques. At the time of Dr. Mayneord's visit here, informal opinion favored the adoption of a "measured unit" based on the number of ion pairs per gram of air in a small cavity in addition to the "significant unit". This would have the advantage of providing a secondary measurement which, in the ranges of soft x rays and soft tissue, would be numerically equal to a measurement in roentgens. In more recent discussions here it was stressed that the same advantage may be achieved in the main by a statement setting forth the relationship between the measurement of ionization in a small cavity and the energy dose. It was agreed that detailed information should be circulated regarding the numerical factors which relate the energy dose with the amount of ionization observed in a cavity under specified conditions.

Finally, since it is generally agreed that the present roentgen should be preserved as a unit of moderately hard x rays, we now find ourselves faced with having to deal with at least three different physical concepts (the energy dose, the ionization in a small cavity and the ionization in free air). This is not so serious and has in fact some advantages since, whether we like it or not, there are actually three quantities to be considered. The way of easing this difficulty is to explain the situation as clearly and concisely as possible.

To summarize, then, we offer for discussion the following statement to replace Section 4 of BRU-13. It is recommended that:

(1) The use of the roentgen as a unit of x and gamma ray quantity should be continued within those limits of radiation quality where it is practicable to meet the present definition.

(2) For the correlation of the radiation dose with the biological or other effects which it produces.

(A) The dose shall be expressed in terms of the quantity of energy absorbed per unit mass (ergs/gram) of the material irradiated, at the place concerned.

(B) For this purpose a practical unit (of name yet unspecified) may be used which is defined as equal to 93 ergs/g (see footnote 2).

(3) Since the calculation of the energy dose from measurements of ionization requires a knowledge of parameters and variables characterizing the radiation and the irradiated material, the International Committee on Units shall promote the compilation and the distribution of the best available data useful for this purpose.

(4) When the energy dose is to be obtained from measurements of ionization and with the purpose of facilitating the calculation of the energy dose, the ionization measurements should be made with instruments and under conditions such that the ionization in the gas is produced by substantially the same flow of corpuscular radiation as exists in the material under consideration. The ionization should be expressed in electrostatic units of charge of either sign per gram of gas in the chamber.

II Discussion

The British Committee for Radiological Units is to be congratulated for the excellent presentation of the problem in the memorandum of May 1948 BRU-13. The discussion in this memorandum clearly indicates that, for purposes of biological dosage, it is eminently desirable to express the dose of ionizing radiation in terms of the energy absorbed per unit mass of tissue in the region of interest. The implication is

clear that the British Committee would prefer adoption of a unit based on energy absorbed, were it not for certain difficulties in the practical use of such a unit.

If it is assumed--as stated in the British memorandum--that the "interpretation of biophysical phenomena requires a knowledge of energy absorbed in ergs per gram," the practical difficulties remain, no matter what unit is adopted. In the ultimate analysis it is necessary to express the dose in terms of energy absorbed when the correlation of dose and biological effect is made. This being the case, it is the opinion of the American Committee[3] that a unit of dose based on energy absorbed is preferable. Such a

3/ Commission on Radiological Units, Standards and Protection, American College of Radiology.

unit might be called the energy roentgen[4] r_e and defined as follows: The dose of

4/ This is not being proposed as a name but is used here for convenience in discussion.

ionizing radiation at a given point in a medium is one "energy roentgen" when the energy absorbed by the medium is 93 ergs per gram at the point in question.* This is a derived

*Ed. Note: This assumes electronic equilibrium, 1980.

unit precisely defined in the c.g.s. system of units, and is independent of the method of measurement and the state of the art. It is now necessary to examine the difficulties encountered in the practical uses of this unit, in the present state of the art, and to compare them with the difficulties involved in the use of the "J". It is obvious that ionization measurements are the most practical at the moment. The conditions under which such measurements are to be made are essentially the same in the two cases. The stipulation of an "infinitesimal cavity" in the definition of the J implies that the ionization in the air of the cavity must represent the interaction of the ionizing radiation with the medium rather than the interaction with air. The same requirement must be fulfilled when the unit of dose is the r_e. Therefore, to express tissue doses in either unit the walls of the ionization chamber must be tissue equivalent. If air is used as the chamber gas, the dose in J's is determined directly. To derive the dose in r_e's one must know in addition, the average energy lost by an ionizing particle per ion pair formed in air and the relative stopping power of the tissue with respect to air for the ionizing particle in question. If more than one kind of ionizing particle and different energy ranges are involved, appropriate summations must be made, or overall average values of these two quantities must be used. The accuracy with which the dose can be expressed in r_e's will depend on the accuracy of the available values of those two quantities. In this connection the worst case encountered in practice is that of fast neutrons. This is also the case in which measurements in J's are fraught with serious difficulties.

The implication of the infinitesimal cavity requirement has already been mentioned. When the range of the ionizing particles in atmospheric air is long, the requirement can be met by using reasonably small chamber volumes and if necessary extrapolating to zero volume. (The walls, of course, must be tissue equivalent.) In the case of fast neutrons in the energy region of practical importance, the range of the heavy recoils (principally O, C, and N) in atmospheric air is extremely short (sometimes less than 1 mm) so that, either the linear dimensions of the chamber volume are made smaller than this, or the air pressure must be correspondingly lower than atmospheric pressure. In the case of most of the practical measurements of neutron dose, the ionization current is too small under these conditions. It should be borne in mind in this connection that very often the chamber volume must be kept within certain limits of size dictated by geometric requirements of the dosage problem under investigation. Accordingly, in general it is well nigh impossible to make measurements of neutron doses in J's by using air as the ionized gas. The alternative is to use ionization chambers in which both the walls and the gas are tissue equivalent. To derive the numerical value of the dose in

terms of the J when the gas is not air, requires a knowledge of the relative value of the average energy lost by a particle per ion pair produced in the tissue equivalent gas and in air, and the relative value of the stopping powers of the two gases for the ionizing particle under consideration. On the other hand, to express the dose in terms of the r_e, it is necessary to know only the average energy lost by a particle per ion pair produced in the tissue equivalent gas (in addition to the ionization current per gram of the gas).

In the present state of the art it is generally assumed that the average energy lost by an electron per ion pair produced in air is 32 to 33 eV. for all electron energies of practical importance. (Considerable uncertainty exists as to the value of this quantity of very low electron energies.) For alpha particles a value of 35 eV. is commonly accepted. Little is known about the average energy lost by a heavy recoil (O, N, C) in producing an ion pair in air, but a value of 35 eV. is generally used. The corresponding values for a tissue equivalent gas are known with less accuracy. However the experimental determination of this quantity is relatively simple. In the case of alpha particles from a thick uranium source, the value is 2 to 10% lower than that for air, depending on the particular gas mixture used to simulate the composition of tissue. In the case of fast neutrons one may take the value for alpha rays and determine the energy absorbed per gram of tissue with sufficient accuracy for most practical purposes (when tissue equivalent walls and gas are used in the ionization chamber).

Using air in a chamber with tissue equivalent walls, one has first the difficulty of complying with the infinitesimal cavity requirement (as already discussed) and then the complication caused by lack of reliable data about relative stopping powers of H, O, C, and N recoils in the tissue equivalent material. This is particularly serious since stopping powers vary considerably with the energy of the ionizing particle. Experimental determinations of stopping powers are more difficult than experimental determinations of energy lost per ion pair. Furthermore, even if exact values were available for all particles and energies, one would have to know in addition the average values to use for the energy ranges obtaining in a given neutron beam.

Having emphasized the importance--in fact the practical necessity--of using ionization chambers with tissue equivalent walls and gas, it is well to point out explicitly that this would be done only when necessary. In other words, the definition of the r_e does not specify the method of measurement and one is at liberty to choose the simplest method that will give the result with the desired accuracy. In many cases air-filled ionization chambers can be used and tissue equivalence of wall may be obtained, reproducing in the wall material the atomic composition of tissue.

Addendum A

L. D. Marinelli has emphasized a practical example of conditions where, even at the present time, the significant unit--i.e. the energy absorbed--is calculated directly rather than through ionization measurements. This is the case when radioactive materials are distributed through a tissue or through a whole body.

Addendum B

K. Z. Morgan has pointed out that the amount of energy absorbed by water which is exposed to 1 roentgen of x rays is somewhat larger than 93 ergs/g. The correct value, which takes into account the difference of stopping power per electron between H and air is more nearly equal to 96 ergs/g. The value of 93 erg/g suggested in the recommendations for the practical unit may well be subject to discussion pending official action. A value of 95 or perhaps 100 ergs/g might be found more desirable.

Newell's final word on the question raised by the American and British Committees was contained in an "unofficial" note to Mayneord on February 23rd, the pertinent part of which follows:

"I am sure most United States radiologists who think effectively about the meaning of the roentgen think of it as a unit of exposure when they are measuring a beam of x rays, but think of it as a dose (ionization or energy absorption per unit volume) when talking about its effect on patients. Am I correctly informed that most British radiologists do, too? This is so deeply set in the minds of most of us, and we are so unconscious of the switch from one dimensional concept to the other, that it took George Laurence some

effort to make me see clearly that the roentgen is a measure of exposure (let the tissue do with it what it will!).

"If irradiation be designated in terms of specific energy absorption, the radiologist and biologist will take that as a satisfactory basis for study of correlation of dose and effect. He will be aware of the disturbing influence of columnar ion density, but will not try to recalculate his dose to allow for it, because he knows that this effect varies with the biologic substrate. But if the irradiation is designated in terms of exposure, he will try always to calculate from that the specific energy absorption. He will not try to correlate the effects observed with the exposure directly (unless it is a purely practical clinical problem, like treatment of eczema). What I am getting at is:

"Sure, the measured quantity can be stated with more certainty, for in many instances it is hard to be at all sure of the specific energy absorption in a liquid or solid. But the difficulty remains just the same. It is not cured by deciding to define the unit in terms of the measured quantity. It is merely shifted from the shoulders of the physicist who knows something about it onto the shoulders of the radiologist or biologist, who may be much less understanding of the problem. I would rather have the clinician or the investigator put his foolish trust in the calculations of the trained physicist than in the calculations of his own untrained self."

cc: Lauriston Taylor
George Laurence

During the spring of 1949, Dr. L. H. Gray of England, who is credited with the practical philosophy on cavity ionization, spent some time in the United States, including a few days at NBS. This led to further consideration of some of the questions raised in Taylor's letter of February 11th to Mayneord. As a result of this, a new memorandum was prepared by Fano expressing his views and those of Gray, Wyckoff, and Taylor.

The memorandum was forwarded to Mayneord and later to members of the newly re-established ICRU. It follows:

Memorandum on Radiological Units
X-Ray Section National Bureau of Standards, May 26, 1949
U. Fano, L. H. Gray, L. S. Taylor

This is a follow-up to the "Report on Radiological Units for International Adoption by the Commission on Radiological Units Standards and Protection of the American College of Radiology" which was circulated under a cover letter from L. S. Taylor to W. V. Mayneord dated February 11, 1949. That report will be referred to as the "American Report."

Dr. L. H. Gray of the British Medical Research Council visited the United States in April and May, 1949. He held extensive discussions on the problem of units with the primary purpose of ironing out the remaining differences between the recommendations of the British Report BRU 13* and those of the "American Report". (See attached copy)

*British Commission for Radiological Units. Memorandum on the Measurement of Ionizing Radiations for Medical and Biological Purposes, BRU/13, May 1948 (see p. 81).

The specific goal was to arrive at a modified draft of recommendations that might be approved by both groups. Since these groups alone (together with their cooperating workers) have expressed an active interest in the radiological units problem on the international plane, it is felt that a draft of recommendations acceptable to them would constitute an adequate proposal for initial consideration by the next radiological Congress.

This memorandum intends to present for consideration the draft of recommendations tentatively agreed to with Gray, together with some explanatory remarks and notes. The notes may be regarded as an addendum to the recommendations and aim at explaining the reason for the choice of specific values for the units.

The text of the recommendations is divided into basic parts and "tentative" parts. The basic parts are essentially a statement of principles and coincide almost exactly

with the recommendations of the "American Report". The "tentative" parts aim at the implementation of those principles and certain specific suggestions as to the names and values of the units to be considered. It is felt that these parts may very well be left open for reconsideration until a very late date. Many workers may want to approve the basic parts now without prejudice of their acceptance of each detail of the tentative parts.

Gray agreed fully with the basic principle of the "American Report" to adopt a unit of "energy dose" as the fundamental unit of radiology. At the same time he stressed the case for the standardization of a practical unit in terms of which ionization measurements should be reported.

This case rests on the fact that ionization measurements are frequently more accurate--in the present state of the art--than the knowledge of the constants required for reducing them to energy units. If each individual worker felt bound to report his results only in energy units, the accuracy of the final results would be weighed down by the inaccuracy in the knowledge of the constants, and a part of the information actually gathered in the experimental observations would be lost. Alternately the individual worker would probably resort to some method of his choice to report the raw results of his ionization measurements prior to conversion to energy units. Gray's point is that the International Congress should try to codify a procedure for reporting the results of ionization measurements (prior to their conversion to energy units) in order to prevent the wild confusing growth of different practices aiming at the same goal.

The history of the "American Report" shows that a recommendation in line with Gray's point was very nearly included in the report. The main resistance to the acceptance of such a recommendation stemmed from fear of the confusion arising from the use of three instead of two units and fear that this acceptance would prevent the energy unit from attaining its inherently predominant status.

It seems to us that these obstacles are not sufficiently serious to offset the advantages to be obtained by codifying the methods by which ionization measurements should be reported. The draft recommendations presented here include such a codification. The predominant status of the energy unit is clearly recognized in the enclosed recommendations. Nevertheless it seems likely that this recognition is not adequately stressed and might usefully be enhanced in the eyes of the radiological public by a subtler choice of words, of symbols, or of some other propaganda device. Suggestions in this direction are particularly welcome.

The specific numerical values of the units introduced in the enclosed recommendations were chosen in such a way that water or soft tissue exposed to 1 roentgen of medium-hard x rays also receives very approximately one unit of energy dose and induces one unit of ionization in the gas contained in an infinitesimal cavity.

This choice was made with the understanding that a majority of radiological physicists favor the adoption of special practical units designed to match the x-ray roentgen at least approximately. We are prepared to go along with this procedure. Nevertheless, we wish to restate our opinion--previously expressed in other informal memoranda--that it may prove wiser in the long run to express all results in standard physical units such as ergs/g or e.s.u./g as the case might be. According to this view the "tentative" statements and the accompanying notes might simply be deleted from the following recommendations.

RECOMMENDATIONS

(1) The use of the roentgen as a unit of x- and gamma-ray quantity should be continued within those limits of radiation quality where it is practicable to meet the present definition.

(Tentative)--This unit might possibly be specifically designated as the x-ray roentgen (symbol: "rex").

(2) For the correlation of the dose of any ionizing radiation with the biological or other effects which it produces, the fundamental principle shall be to express the dose in terms of the quantity of energy absorbed per unit mass (ergs/gram) of the material irradiated, at the place concerned.

(Tentative)--The unit to be used for this purpose shall be the "energy roentgen" (symbol: "ren") which is defined as equal to 93 ergs/g. (Note A).

(3) Recognizing the practical importance of dose determinations based on the measurement of ionization in gases, and in order to permit the accurate reporting of observations obtained by ionization measurements, made under conditions where the reduction to ergs per gram is hindered by inadequate knowledge of the necessary constants, the following procedure is suggested: The ionization measurements should be made under such conditions* that the ionization in the gas is produced by substantially

*Referred to as "infinitesimal cavity conditions."

the same flow of corpuscular radiation as exists in the material under consideration.

When this procedure is followed, the result of this measurement shall be expressed in terms of the quantity of charge of either sign separated per unit mass of gas in the chamber.

(Tentative)--The unit to be used for this purpose shall be the "ionization roentgen" (alternately: "roentgen ion") (symbol: "rion") which is defined as 773 e.s.u. (i.e. 1.61×10^{12} ion pairs) per gram of air in an infinitesimal cavity enclosed in the material irradiated. (Note B). If the ionization is measured in a gas other than air, the name of the gas shall be stated together with the name of the unit.

(4) Since the calculation of the energy dose from measurements of ionization requires a knowledge of parameters and variables characterizing the radiation and the irradiated material, the International Committee on Units shall promote the compilation and the distribution of the best available data useful for this purpose.

Note A. According to the present definition of the roentgen, air exposed to 1 roentgen of x rays absorbs approximately 32.5 electron volts for each of the $1/(4.80 \times 10^{-10})$ ion pairs that make up 1 e.s.u., per 0.001293 grams of air, that is:

$(32.5/4.80 \times 10^{-10} \times 0.001293)$ eV/g.

Since $1/(4.80 \times 10^{-10})$ eV amounts to 1/300 erg, this quantity equals $(32.5/300 \times 0.001293)$ erg/g = 84 erg/g.

Water exposed to the same radiation absorbs more energy per gram in a ratio equal to the relative number of electrons per gram or per unit of molecular weight (assuming that the energy absorption is due entirely to Compton effect). This ratio is very approximately, (10/18)/(1/2) = 10/9,

(10 electrons per 18 units of molecular weight in H_2O, 14 per 28 units in N_2 etc.)

Therefore the energy absorbed by water is: (10/9) 84 erg/g = 93 erg/g.

Note B. Free air exposed to 1 roentgen of x ray, undergoes the formation of 1 e.s.u. of ionic charge per 0.001293 grams, i.e., using the values of Note A of

$1/(4.80 \times 10^{-10} \times 0.001293) = 1.61 \times 10^{12}$ ion pairs /g.

Water exposed to the same radiation absorbs 10/9 as much energy as air (see Note A). Air in a cavity within water receives less energy per unit mass than the surrounding water in a ratio equal to the relative mass stopping power whose value is taken here as equal to 1/1.13. Therefore the charge of the ions produced by 1 roentgen, under the conditions of Note A, per gram of air in a cavity surrounded by water is:

$(1/1.13)(10/9)(1/0.001293) = 761$ e.s.u./g

which amounts to: $(1/1.13)(10/9) \times 1.61 \times 10^{12} = 1.58 \times 10^{12}$ ions pairs/g.

Notice that free air exposed to 1 x-ray roentgen receives--according to the definition suggested here--

$1/(1/1.13)(10/9) = 1.13 \times 9/10 = 1.02$ rion.

At a later time Gray prepared the following for presentation to the British Units Committee:

NOTES ON THE "MEMORANDUM ON RADIOLOGICAL UNITS"
PREPARED AT THE NATIONAL BUREAU OF STANDARDS
ON MAY 26th, 1949

During April and May, I had opportunities to discuss the Memorandum which had been prepared up to that time by the American and British Radiological Units Committees in some detail with Failla, Mrs. Quimby, Lauriston Taylor, Fano, Robley Evans, Loevinger and Feitelberg. Before visiting the National Bureau of Standards, I had already formed the opinion that, while everyone urged the adoption of ergs/gm as the fundamental radiological unit, all who had given the matter serious consideration were aware that situations might arise in which valuable information would be either lost or reported in an inadequate or misleading manner if this was the only recognized way of specifying dose. Moreover, it seemed to be generally agreed that such situations would best be met by the use of the J unit and the related procedures outlined in BRU/13. For my part, I said that as far as I was aware, the British Committee fully shared the American view that the energy absorbed per gr. of the irradiated medium should form the basis of the fundamental unit of radiology, and would wholeheartedly support a recommendation to this effect at the forthcoming Congress.

It seemed, therefore, as far as I could judge from these discussions, that the outstanding problem which troubled both the American and British Committees was how to give the recommendation regarding the fundamental unit such prominence relative to the definition of the J unit that the reader was impressed not only through the substance but also through the form of what he read that his results would only assume their maximum usefulness when expressed in fundamental units. When I was at the Bureau of Standards, therefore, Fano, Taylor and I spent some time attempting to draft a statement embodying this point of view. This draft was presented and discussed at a meeting of the New York Hospital Physicists Association, which met on May 2nd, under the Chairmanship of Dr. Feitelberg, at the Mount Sinai Hospital, and was attended among others by Failla, Mrs. Quimby and Loevinger. Fano came up from Washington to attend this meeting.

I undertook to convey to the British Units Committee the sense of that meeting. The enclosed memorandum by Fano does this so admirably that I have nothing to add.

L. H. Gray
9th June, 1949

Attachment:

Errors in the Determination of Energy Absorption from X-Rays
May 1949

A question arose in the course of a local discussion concerning the estimated accuracy with which one can determine the energy absorbed in tissue or other materials on the basis of ionization measurements. It appeared that none of us had a ready answer (except that the accuracy is probably not much worse than 5% or better than 1%) and that it would be desirable to review and evaluate the pertinent data. To provide an answer to this question is a necessary step toward implementing "point 4" of the tentative recommendations on radiological units, as set forth in our memo of May 26, 1949. It is also necessary to set the specifications for other conceivable methods (say, calorimetric or chemical methods) for the determination of energy absorption that might be suggested.

This memo is written for the following purposes:

(a) To call the problem to the attention of interested persons.

(b) To set forth a breakdown of the problem, as it is understood by us at the present time, into certain more specific questions.

(c) To ask for comment and suggestions.

Our tentative view is that a satisfactory review and evaluation of the available evidence would require some concerted effort by a number of persons particularly acquainted with the various aspects of the problem. It would be desirable to arrange to carry out and conclude such an effort within, say, the next six months. The ground-work might be done by consultation by mail, beginning with this memo. Simultaneously we might start collecting and examining the pertinent literature here in Washington. Then, in the fall, one might try to arrange for a critical group discussion at a suitable location. It would seem that the determination of energy absorption by ionization must be done by the "infinitesimal cavity" method and application of the Bragg-Gray

principle. The main uncertainties involved seem to be the following:

(1) Experimental accuracy practically attainable in realizing "infinitesimal cavity" conditions by means of extrapolation techniques.

(2) Accuracy of data on the relative stopping power of air tissue and other important materials.

(3) Accuracy of data on the average energy absorption in air, per ion pair produced.

The dependence of the answer of these questions upon the quality of the primary radiation may also require special discussion.

U. Fano, X-Ray Section, NBS

Reacting to this memo, G. Failla later pointed out that they had worked on the problem for some years in experiments designed to provide more accurate information. He noted that there was some disturbing evidence that the stopping power of a substance might be different in the gaseous and the solid or liquid phase. If this proved to be true, the Bragg-Gray relation would become untenable. Therefore they were developing a method for determining this point experimentally.

Gray's more detailed response of July 7th is given below. During this same period, Fano and Taylor published their paper on "Dosage Units for High Energy Radiation" (Ref. 113).

Thank you for your letter of June 24th and the enclosed memorandum on "Errors in the Determination of Energy Absorption from X-Rays". I think it is excellent that you are taking the initiative in this matter and I should be very happy to help in any way I can. I think the difficulties will not be too great in those cases where the gaps in our knowledge can be filled in by the application of existing formulae and only computational effort is needed. I could not personally offer much help along these lines but I was discussing this and closely related problems at Harwell quite recently and I think Cockcroft would probably be prepared to allocate some effort at Harwell. No doubt help of the same kind on an even greater scale could be made available from the A.E.C. I suspect, however, that we shall find that more experimental work is absolutely essential. I have in mind particularly the recent observations of Appleyard, which appear to confirm the earlier measurements of Michel and Phillip regarding the stopping power of water for alpha particles. Appleyard's value is some 13% greater than the figure that would be computed from stopping power of hydrogen and oxygen in the gaseous form.

As far as I know, no existing theory predicts a discrepancy of this magnitude and those theoreticians with whom I have discussed the problem have not had any suggestions to offer. Since water is the anomalous substance, the anomaly cannot possibly be side-tracked. Appleyard is going over to the States shortly to work at Yale for a year so you will have opportunities to discuss this particular problem with him, but I suspect that it is only one of a number of problems which we are up against, which really demand further experimental work.

Although possibly not strictly relevant to the dosage problem, I may mention for your information that J.M.C. Scott, in the Theoretical Physics Division at Harwell, recently computed ab initio at my request the energy dissipation by electrons in water as a function of electron energy using the Bethe formula and the figure $\bar{E}$ = 69 eV proposed by Lea for the effective ionization potential of water which enters into the stopping power formula. It has been disturbing to find that Scott's figures do not tally with Table 10 of Lea's book and the reason for the discrepancy has not been discovered. I presume we may expect to hear from you again before very long suggesting the manner in which it is proposed to assemble existing information, subject it to critical examination and apportion sections for further investigation in particular laboratories.

I expect you will have seen the letter I sent to Fano reporting upon the discussion which took place between Mayneord, Owen and myself on the document on units which was prepared at the time when I was in Washington. I think you may expect to hear officially from Mayneord about this in the near future.

This was essentially the end of the discussions on radiation quantities and units. The RSNA Standardization Committee could have helped with this problem but it had been discontinued, while the College Commission, CRUSP, was never really constituted to deal with such matters. By 1950 the ICRU was reconstituted and became the recognized international focal point for matters of radiation quantities and units. Because NBS played a major role in the work of the ICRU, there was no break in the continuity of its relations with medical radiology.

CHAPTER 7. RADIOLOGICAL SOCIETY ACTIONS (1927-1940)

NOTE: While the various developments previously mentioned were taking place, there was continuous activity on the part of the Standardization Committee of the RSNA. Since it is impractical to weave NBS and RSNA actions into a chronological order, the latter will be treated in discrete units covering time periods close to related NBS actions. The first unit will cover the period during which Drs. Ernst and Glasser were Chairmen of the Standardization Committee--1927 through 1933. If the presentations appear to be somewhat scattershot, that was in fact the way many events took place. Nevertheless, these actions were indicative of the period during which they occurred, even though in hindsight, one may wonder why they were ever considered as problems at all. It is hoped that this treatment will give the reader a better understanding of why we were where we were in the 1930's, 1940's, 1950's, and even today.

In 1927, a series of safety recommendations was made by the Radiological Society of North America through its Committee on Safe Apparatus. This report, presented at the December 1927 Annual Meeting of the RSNA, was in the form of a small pamphlet and not published in the journal. However, the report was improperly filed with the papers of the Standarization Committee of the RSNA and has only recently come to the surface. Because of its importance at the time, and what should have been its subsequent influence upon the programs of the Bureau of Standards, the complete report is given below.*

*While representing the United States at the Second International Congress of Radiology in Stockholm, in 1928, for the formation of an International Committee on X-Ray and Radium protection, the author was unable to present a defined U.S. position because of the lack of agreement between the American societies. There existed the recommendations of the American Roentgen Ray Society, as adopted at their 1922 annual meeting, but the RSNA recommendations could not be located.

Committee on Safe Apparatus

1927

E. L. JENKINSON, M.D., Chairman,
St. Luke's Hospital, Chicago, Illinois.

DALTON RICHARDSON, M.D.,
404 Scarborough Building, Austin, Texas.

1928

E. L. JENKINSON, M.D., Chairman,
St. Luke's Hospital, Chicago, Illinois.

OTTO GLASSER, Ph.D.,
Cleveland Clinic, Cleveland, Ohio.

ROBERT R. NEWELL, M.D.,
Stanford Hospital, San Francisco, Cal.

The report of the 1927 Committee on Safe Apparatus was presented and unanimously adopted at the first executive session of the thirteenth annual meeting of the Radiological Society of North America, November 28, 1927, at New Orleans, Louisiana. It was decided at that session that a copy of the report be sent to all the members of the Society and all the hospitals of the United States.

This report is given herewith, with additions and revisions up to April 27, 1928.

REPORT OF COMMITTEE ON SAFE APPARATUS

PROTECTION

Screen Examinations 70 K V P

The X-ray tube should be completely enclosed with protective material equivalent to not less than 2 mm. of lead. The lead glass bowl is not adequate and is not recommended. A lead box is the only safe enclosure. It is advisable to have the X-ray tube in a glass bowl enclosed in a lead lined box.

The design of the diaphragm should be such as to permit it to be completely closed. The simpler rectangular form of diaphragm will, in general, be found preferable to the iris type. The diaphragm should be made of lead at least three mms. thick. In the case of installations which are incapable of generating peak voltage exceeding 70,000, the lead value of the tube enclosure may be reduced to 1.5 mm. of lead and the diaphragm to 2 mm. We believe, however, it is advisable to use the heavier lead in all cases.

The fluorescent screen attached as a permanent fitting to screening stands, etc., should be fitted with lead glass equivalent to not less than 2 mm. of lead. The glass should be of uniform thickness and free from striæ and gas bubbles. In all positions, the lead glass should be large enough to cover the area irradiated when the diaphragm is opened to its widest. For a screen of smaller area, the lead glass should be mounted in a frame of protective material which overlaps the screen and is of adequate width and thickness to afford protection in all positions of the screen.

In the case of portable screens, consideration of the weight militates against the recommendation of a degree of protection greater than one mm. of lead. Great care should be used in doing portable screen examinations. Long exposures are to be discouraged.

In case of a surgical operation under the screen, such as manipulating fractures or the removal of foreign bodies or kidney stones, the operation of the X-rays should be done by some person in the department familiar with the output and safe dosage of the machines. Surgeons are prone to keep the current turned on too long and are very apt to insist on more milliamperage. Much of this is due to their lack of familiarity with the accumulative effects of the rays.

Proper accommodation of the eyes will obviate long exposures and high milliamperage. The surgeon must,

at all times, be impressed with the importance of proper protection of his hands. The *onus* in these cases rests to a large degree on the X-ray Department. Members should be warned frequently.

The field of examination should be kept as small as possible and the hands during the manipulation be outside of the direct field. During screen examinations, it is essential that great care be used when anesthetics are used. We are of the opinion that any anesthetic is dangerous. Ethylene should never be used in the presence of an X-ray machine—when in operation. It has been our plan to administer the anesthetic outside the fluoroscopic room and to be sure to remove the ether can from the room in which the X-rays are being used. Nitrous Oxide, we believe, is dangerous and should not be used during fluoroscopic examinations or manipulations.

It is imperative that all persons working with the X-rays or Radium be informed of its injurious effects upon the body. These effects can be minimized, if not altogether obviated, if the rules hereby laid down are complied with. Inasmuch as the X-rays and Radium do cause permanent changes to the skin, blood and the organs of reproduction, it is essential that those in charge of the Department inform their associates that great care is necessary in order to protect themselves and patients. Carelessness is certainly to be discouraged.

It is advisable to do complete blood counts at intervals of six months to keep a chart of what changes, if any, are taking place.

A technician who is careless with himself or herself is usually careless with patients.

Persons working in the X-ray Department should have ample time off duty, to be spent preferably out of doors in the sun. Long hours are to be discouraged. We are not going to attempt to say how many hours a technician should work each day. This does not apply only to technicians, but also to the many Physicians working with X-ray for long hours each day. Six hours is probably the maximum. The Director of the Department should at frequent intervals test the various apparatus to see whether there are any flaws in his protection. "A stitch in time" applies here.

Fluoroscopic Equipment

It is our opinion that great care should be used in doing Fluoroscopic Examinations. This is especially true if numerous patients are examined each day. With the fluoroscopic equipment now available, both upright

and horizontal, adequate protection can be obtained. Extending from the tube box to the table top or to the front of the upright, there should be a lead cone at least 2 mms. thick. The cone or square should be as close to the top or front as is possible, allowing only enough space for the transit of the tube box. The addition of the cone will minimize the amount of scattered radiation.

Suspended from the screen a lead rubber apron of two thicknesses should be standard equipment. This apron will move up and down with the screen and will stop secondary and scattered radiation from the patient's body. The long bones of the operator are also protected.

The use of the lead rubber suspended along the sides of the fluoroscopic table also adds to the protection of the operator. The screen carrier used on the fluoroscopic table should be attached to the tube box and move with it. With this equipment, the assistant can keep his or her hands well away from the field of active radiation.

Protective gloves should be of lead rubber (or the like) and afford protection for both the back and front of the hands (including fingers and wrists). The protective value should be not less than ½ mm. of lead. Gloves should preferably be lined with leather or other suitable material. (As practical difficulties militate at present against the recommendation of a greater degree of protection, all manipulations during screen examination should be reduced to a minimum.) We advise the use of leather gloves inside the lead rubber gloves.

In those cases where the necessity is felt for even greater protection for the operator, goggles and aprons may advantageously be worn. The glass of the goggles should have a lead value not less than ½ mm.; aprons should have a lead value of not less than 1 mm. It is our practice always to wear a lead rubber apron during fluoroscopic examinations.

A minimum output of radiation should be used with the bulb as far from the screen as is consistent with the efficiency of the work in hand. Screen work should be as expeditious as possible.

Radiographic Examinations

("Over-head" Equipment)

The X-ray bulb should be enclosed as completely as possible with protective material equivalent to not less than 2 mm. of lead. This figure may be reduced to 1.5 mm. in the case of installations which are incapable of generating peak voltages exceeding 70,000.

The operator should stand behind a protective screen of material equivalent to not less than 2 mm. of lead. In general, such screens should not be less than 3 ft. 6 in. wide and 7 ft. high and should extend to within 1 in. of the ground. If a window is provided, its lead equivalent should not be less than 2 mm. Its dimensions need only rarely exceed 9 in. by 6 in.

Cubicle System

Where the cubicle system is already in existence, it is recommended that:

1. The cubicle should be well lighted and ventilated, preferably provided with an exhaust electric fan in an outside wall or ventilation shaft and suitable air inlets. The controls of the X-ray apparatus should be outside the cubicle.
2. The walls of the cubicle should preferably not take the form of partitions, but should extend from floor to ceiling. If partitions are adopted, they should not be less than 9 ft. in height and extend to floor level.
3. The walls (and where necessary, the floor and ceiling) of the cubicle should be of material equivalent to not less than 2 mm. of lead. Windows should be of high quality lead glass of equivalent thickness. They need only rarely exceed 9 in. by 6 in. in dimensions. Care should be taken that protective material overlaps at joints.

X-Rays for Deep (High-Voltage) Therapy

This section refers to sets of apparatus giving peak voltages above 100,000.

1. Small cubicles are not recommended.
2. A large, lofty, well-ventilated and lighted room should be provided, preferably provided with an exhaust electric fan in a suitable air duct.
3. The walls (and where necessary, the floor and ceiling) of the room should provide protection equivalent to not less than 3 mm. of lead. Windows should be of high quality lead glass of equivalent thickness. They need only rarely exceed 9 in. in dimensions. Care should be taken that the protective material overlaps at joints.
4. The X-ray bulb should be enclosed as completely as possible with protective material equivalent to not less than 3 mm. of lead, preferably 4.
5. A separate enclosure should be provided for the operator, situated as far as possible from the X-ray bulb. All controls should be within this enclosure, the walls and windows of which should be of material equivalent to not less than 3 mm. of lead.

In preparation for the forthcoming meetings of the International Committee on X-Ray Units in Stockholm, in July 1928, a special meeting of the RSNA Standardization Committee was held in Washington on March 10, 1928. The principal objective was to reach agreement on the recommendations on x-ray quantities and units that the U.S. delegation would present to the Congress. By this time, Taylor had replaced Hunt as a member of the committee from the Bureau of Standards.

The following final report of the Committee was published in 1928 (RSNA, 1928):

Radiology 10, 318, 1928

REPORT OF THE COMMITTEE ON STANDARDIZATION OF X-RAY MEASUREMENTS OF THE RADIOLOGICAL SOCIETY OF NORTH AMERICA

1. The unit of effective X-ray intensity, *"one R,"* is that intensity of radiation which produces a saturated ionization current of one electrostatic unit per cubic centimeter of a non-restricted volume of air at a temperature of 0° Centigrade and a pressure of 760 mm. of Hg.

2. The method recommended by the Committee for the measurement of X-radiation in terms of the above unit in a non-restricted volume of air is the employment of an air ionization chamber—

(a) with open windows at the sides of incidence and energence;

(b) with guard electrodes sufficiently large to insure the desired electric field;

(c) with suitable spacing of electrodes to include sensibly all of the ionizing effects of the photo-electrons;

(d) an arrangement of diaphragms to properly collimate the beam and prevent undesired secondary radiation effects.

Such an ionization chamber constitutes an "absolute standard."*

3. The measurement of the X-radiation at the place of practical application may be made by a dosimeter which is calibrated in *R*-units by means of an "absolute standard." Such a dosimeter constitutes an "absolute standard."

Preferably a practical dosimeter should have an ionization chamber in which the measured ionization current is proportional to that produced in an "absolute standard" with the various qualities of radiation employed. The constancy of such a dosimeter may be satisfactorily controlled by means of a suitable radio-active substance properly employed.

4. Voltage and filtration alone do not properly define radiation quality. Spectrometric analysis of the X-radiation gives the most complete determination of its quality. For practical purposes the quality may be expressed by the half value layer in a suitable material (copper or aluminum is recommended) or by the effective wave length.

The half value layer for the X-radiation employed is that thickness of the given material which will reduce the reading of the "absolute standard" or "calibrated standard" to one-half of its value.

The effective wave length of the X-radiation employed is the wave length of monochromatic radiation for which the readings of the "absolute standard," or "calibrated standard." would be reduced in the same ratio as that actually observed when a certain thickness of a given material (copper or aluminum) is interposed.

5. The Committee recommends that dosage be expressed in terms of *R*-units and time of application, and that the quality of the radiation be stated in terms of effective wave length or half value layer.

Respectfully submitted,

Committee on Standardization of X-ray Measurements

Edwin C. Ernst, M.D., *Chairman.*

Otto Glasser, Ph.D., *Sub-chairman.*

Robert A. Arens, M.D.
William E. Chamberlain, M.D.
Noah Earnest Dorsey, Ph.D.
William Duane, Ph.D.
Arthur W. Erskine, M.D.
Gioacchino Failla, Ph.D.
E. A. Pohle, M.D.
U. V. Portmann, M.D.
R. William Stenstrom, Ph.D.
Lauriston Taylor.

*Ed. Note: This should probably have been "calibrated standard."

Aside from the rather abbreviated description of the "absolute standard" ionization chamber, the question of radiation quality measurement was left open. This allowed use of the half-value-layer method which had been in use for some years, especially in Europe, or use of the effective wavelength measurement as proposed earlier by Duane (1928). Later studies at the Bureau of Standards showed that the Duane Method was cumbersome and did not add any information beyond that obtained from the half-value-layer method (Ref. 14). The committee also recommended that dosage be expressed in terms of "R Units," a technically improper term but one which persisted in clinical usage for many years. The correct usage should have been "roentgen" and the symbol "R."

In referring to radiation quality, the report stated that spectrometric analysis of the radiation would give the most complete description of its quality. With this there could be no argument, but there was no simple, meaningful way in which the spectrum could be numerically described. While spectrum could be described by the usual wavelength distribution plot, there was no simple way to predict from it the "effect" of modifying the spectrum. (For example, how does one predict from a spectrum the changes in response of an ionization chamber or the radiation penetrability in tissue?) Given two x-ray beams of different quality, and hence different spectra, treatments with one beam could only to a limited degree be meaningfully compared against treatments with the other beam. This later led to a large number of "different" x-ray spectrometers which served only to confuse the situation (DuMond, 1930).

The radiological profession's concern about the problem of x-ray measurement and the development of suitable standards prompted Dr. Arthur Erskine to present the following resolution at the business meeting of the RSNA in December 1927 (Erskine, 1928):

RESOLUTION
(From the Minutes of the Meeting)

"DR. ERSKINE: I want to present the following resolution:

"BE IT RESOLVED that the Radiological Society of North America recommend for the consideration of the Director of the United States Bureau of Standards the following program of X-ray standardization and study of protection, with the necessary appropriations:

"A. The standardization of X-ray energy according to the definition of the X-ray unit as recommended by the United States Bureau of Standards; the standardization to be made with the use of a constant potential generator of at least 200 K.V. with the smallest possible percentage of ripple. (This is the only method accepted by the foreign laboratories.)

"B. The publication and distribution of the information obtained.

"C. The interchecking of the standard unit with other laboratories and hospitals in order that there may be devised an instrument capable of transportation without loss of calibration.

"D. A system under the auspices of the United States Bureau of Standards, of inspection, testing, and recommendations for all laboratories, clinics, and hospitals, according to a fixed code to be established.

"E. A program for any other research or investigation pertaining to the general problem of X-ray dosage.

"F. The employment of sufficient semi-technical assistance to insure that this program be conducted with least delay and greatest efficiency.

"G. The deputation of a representative of the United States Bureau of Standards to the International Congress of Radiology in Stockholm, Sweden, in July, 1928.

"The Radiological Society of North America endorses heartily the outline of program as tentatively suggested by the United States Bureau of Standards.

"The Radiological Society of North America also offers at any time its full cooperation and assistance to the United States Bureau of Standards for the furtherment of its general X-ray program.

"DR. ERSKINE: I move, Mr. President, that this resolution be adopted and a copy sent to the Director of the Bureau of Standards.

"The motion was seconded and unanimously adopted."

Some 50 years later, in an attempt to develop a logical progression in the early development of the concepts of radiation dose measurements, H. O. Wyckoff encountered considerable difficulty in determining the thinking of those participating in the development of our understanding of radiation measurement during that early period. (With hindsight, Wyckoff did develop what appears to be a reasonable logic. However, in the author's opinion, the earlier participants had not really thought through the basic concepts very clearly, and this led to subsequent confusion for the next decade or more (Wyckoff, 1980).)

By 1930, the x-ray standards program at the Bureau was in sufficiently satisfactory order to start routine calibrations of instruments. A formal announcement of this was made later that year as follows (NBS, 1930):

Radiology 14, 416, 1930

X-RAY STANDARDIZATION PROGRAM OF THE BUREAU OF STANDARDS

I

The solution of the problems of X-ray standardization has arrived at a point where the Bureau of Standards is in a position to calibrate ionometers for the public.

The discrepancies between different standardizing laboratories in regard to the magnitude of the r-unit are now small enough to be neglected in connection with medical applications. The basis for this conclusion is the agreement within 2 per cent, recently obtained by a comparison, through the cooperation of Dr. Glasser, of two instruments from the Cleveland Clinic with those used by the Bureau; and the fair agreement found earlier between the Cleveland Clinic instruments and those of the Reichsanstalt of Germany. Further comparisons will, of course, be made between the different national laboratories.

II

An ionometer sent to the Bureau may be calibrated for any condition of voltage and filtration desired, although all such instruments, to be accepted for calibration, must be adequately controlled by such means as radium or uranium oxide. This will minimize the possibility of undetected damage occurring to the ionometers in transit.

The following procedure is necessary to avoid delays and complication. Notify the Bureau as far as possible in advance of the shipment of the instrument, giving at the same time the type of instrument, date of purchase, method of controlling its calibration, type of X-ray machine with which it is used, peak voltage applied to the X-ray tube as actually measured with a sphere gap, and the half value layer in copper or the effective wave length of the radiation, with an explanation of how the measurement was made.

III

If the ionometer has been calibrated previously, a copy of the calibration should be supplied in order to complete the Bureau's records for every instrument it handles.

The cost of this service will depend upon the number of calibrations made for each instrument. A schedule of fees may be had upon request after sending in the information required above, in addition to stating the number of calibrations desired.

BUREAU OF STANDARDS
Department of Commerce,
Washington, D. C.

By 1932, with the Great Depression at its worst, the x-ray program at the Bureau of Standards, like other Bureau programs, had taken severe cuts in both personnel and operating funds. Because the x-ray standards program was just beginning to reach effective operation, the radiological societies were concerned. Dr. Ernst, who had fought so hard to get the standardization program started, now feared it might be discontinued or cut below a critical operating level. With this in mind, he proposed a resolution for consideration at the business meeting of the Radiological Society. Action on this was unanimous and the prescribed letter forwarded as follows:

ERNST RESOLUTION (Dec. 1, 1932)

"In view of the present policy of our Government toward economic financial retrenchments in the various departments in Washington, especially the United States Bureau of Standards but more specifically the Department of X-Ray, which has been active and has been vitally interested in the study of and standardization of X-Ray measurements,

"BE IT RESOLVED, THEREFORE, that we, the members of the Radiological Society of North America, in the interest of humanity and the treatment of diseases, especially cancer, realize and keenly feel that the Department of X-Ray of our Government at Bureau of Standards, should be continued in the future on the same high plane of scientific efficiency as experienced by the radiologists of America in the past.

"This humanitarian request is made solely in the interest of the cancer sufferers and his diseases, since we believe that this standardization, information and service should continue to be centralized in this department of our Government. Much of the progress of the past made by this special department of X-Ray of the Bureau of Standards will require uninterrupted standardization facilities in the future. The future requisites of the X-Ray Department are not unlike the important radium measurements that have been made by the Bureau for many years in the interest of safety and efficiency of radium applications, which latter efforts have proven themselves today to be indispensable in the interest of science and humanity.

"No other agency is in a position to better serve us as physicians who are interested in the diagnosis and treatment of diseases.

"(The Executive Committee of the Radiological Society of North American is hereby given authority to transmit through the Secretary and Treasurer to Dr. L. J. Briggs, acting Director of the Bureau of Standards, and Mr. Roy D. Chapin, Secretary of Commerce, Washington, D.C., a copy of the above Resolution this day passed by the Radiological Society of North American in the Executive Session at Atlantic City, N.J., on December 1, 1932.)

"(Dr. Ernst would suggest that this communication be sent immediately, so that these two letters reach Washington prior to Monday, December 5, 1932, if at all possible.)"

Following the meetings of the RSNA in December 1932, the Board of Directors of the society decided to make a number of changes in the Standardization Committee assignments and named Taylor as Chairman, and Dr. U. V. Portmann, Sub-Chairman. Drs. Ernst and Glasser remained as members of the committee.

A letter of November 27, 1933, from Dr. W. H. McGuffin, President of the RSNA, to Taylor, notified him of the change. The new makeup and membership of the committee were as follows: Lauriston S. Taylor, Chairman; U. V. Portmann, M.D., Sub-Chairman; Otto Glasser, Ph.D., Cleveland, Ohio; Edwin C. Ernst, M.D., St. Louis, Mo.; Robert R. Newell, M.D., San Francisco, Calif.; R. J. Ullmann, M.D., Santa Barbara, Calif.; G. E. Richards, M.D., Toronto, Ontario, Canada; W. Edward Chamberlain, M.D., Philadelphia, Pa.; A. W. Erskine, M.D., Cedar Rapids, Ia.; G. Failla, D.Sc., New York, N.Y.; W. H. Meyer, M.D., New York, N.Y.; K. Wilhelm Stenstrom, Ph.D., Minneapolis, Minn.; and J. L. Weatherwax, M.A., Philadelphia, Pa. Consultants: Arthur H. Compton, Ph.D., D.Sc., L.L.D., Chicago, Ill.; and William Duane, Ph.D., Boston, Mass.

The first report of the new committee was adopted in September 1933 and published the following year (RSNA, 1934); it included the recommendations that the International X-Ray Units Committee agreed to at its meetings in Paris in 1931.

Following is the 1933 report by the Standardization Committee:

[Reprinted from RADIOLOGY, Vol. XXII, No. 3, Pages 289-294, March, 1934.]

REPORT OF COMMITTEE ON STANDARDIZATION OF X-RAY MEASUREMENTS

SUBMITTED FOR THE COMMITTEE BY LAURISTON S. TAYLOR, CHAIRMAN[1]

CONTENTS

I. RECOMMENDATIONS TO BE SUBMITTED TO THE INTERNATIONAL X-RAY UNITS COMMITTEE

1. The International Unit of X-radiation shall be called the "roentgen" and shall be designated by the symbol r (lower case letter).

2. The roentgen is the quantity of x-radiation which, when the secondary electrons are fully utilized and the effects of scattered radiations avoided, produces in 1 c.c. of atmospheric air at 0° C. and 76 cm. mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured under saturation conditions.

3. Irradiation expresses the quantity per unit time; it shall be given in roentgens per minute (r/m) as measured in air.

4. All data given in roentgens shall apply to the incident radiation, excluding scattered radiation.

5. Inasmuch as the determination or calculation of tissue dose, representing the amount of radiation received by the skin or any part of the irradiated tissue is uncertain, all data relating to tissue dose (sum of incident radiation and scattered radiation in the tissue) shall be designated as "tissue dose" if expressed in roentgens.

6. Since satisfactory agreement regarding methods for measuring the roentgen has been established between the several National Standardizing Laboratories, various standard methods may be employed to establish the unit.

7. Secondary ionization chambers shall be calibrated against the standard chamber for the range and purpose for which they will be used. It is desirable that the relation of such chambers to the standard chambers vary as little as possible with wave length.

8. The practical instruments employed for the measurement of x-rays shall be designated as follows:

(*a*) "Irradiometer," an ionization instrument which gives a continuous indication of the irradiation.

(*b*) "Roentgenometer," an ionization instrument which measures the quantity of x-radiation (time integral of irradiation) in a given deflection of the indicator.

9. The constancy of the calibration of an irradiometer or roentgenometer shall be tested by the ionization produced in the measuring chamber by means of gamma radiation under fixed conditions from a definite quantity of radium element. Such measurements shall be suitably corrected for the atmospheric temperature and pressure under which the tests are carried out.

10. Specification of the quantity of x-radiation shall always be accompanied by a specification of the quality of the x-radiation. Quality is the wave length energy distribution in the x-ray spectrum. For most practical purposes the quality of the x-radiation may be satisfactorily specified in terms of the copper or aluminum absorption curve combined with a statement of the initial filtration. In lieu of an absorption curve, the equivalent constant potential applied to the tube terminals to yield the same curve may be stated as a single numerical magnitude. Up to 100 K.V. (constant) aluminum ab-

[1] The preparation of this report was begun under the chairmanship of E. C. Ernst, M.D.

sorption curves and above 100 K.V. (constant) copper absorption curves shall be used to establish the equivalent potential.

11. The report of a roentgen treatment shall include, in addition to the total number of roentgens and the quality, the following factors:

(*a*) Durations of, and intervals between, irradiations;
(*b*) Distance from target to skin;
(*c*) Size of field.

II. DISCUSSION OF THE RECOMMENDATIONS

¶ 1.[2] Regarding the name and symbol for the International Unit of X-radiation, emphasis should be directed to its use in relation to units in other fields. The name should always be spelled with a small r, and the symbol always a small (lower case) r. Use of the term "r-unit" or "roentgen-unit" should be deprecated since such expressions are entirely out of keeping with accepted physical nomenclature. (See note in RADIOLOGY, 1930, XV, 305.)

¶ 2. Some serious objections had arisen regarding the wording of the definition of the roentgen and the definition itself. The question before the Committee was how far to go in recommending changes in an internationally accepted definition, and it was decided to change the definition as little as possible but still bring it into an unambiguous and more usable form. At the same time, the Committee wishes to put itself on record as preferring a definition which has a better physical foundation.

In March, 1928, this Committee recommended a definition for the unit of "Effective X-ray Intensity." It is still felt that such a definition would be superior to the present one, in that it defines an x-ray beam in terms of intensity rather than quantity. By defining the roentgen as an intensity, much of the confusion in expressing dosage would be avoided. Since the quantity definition is now so widely used, the Committee does not urge this change unduly.

The present international definition has, however, a potentially embarrassing omission. It is only by fortunate circumstance that this omission has not already caused difficulty, in that below 150 K.V. (r.m.s.) its effect is almost negligible. The present definition does not mention scattered radiation, and it may thus be assumed that in measurements of the roentgen, the effect of scattered radiation may be neglected. However, one of the most important uses of the roentgen, particularly at higher voltages, is to permit a proper evaluation of x-ray dosage in comparison with that at ordinary voltages. It is necessary, therefore, that over the whole voltage range the roentgen be the measure of a quantity which can be translated into actual energy absorbed by tissue. Since the roentgen is proportional to the energy *absorbed from the beam by air,* it is necessary, therefore, to restrict conditions so that the absorption will be known. At tube voltages below 150 K.V. (r.m.s.) the absorption is largely photo-electric, whereas at the higher voltages, absorption is due principally to the energy removed from the beam by the recoil electrons in the Compton scattering process. (This is discussed in detail by C. C. Lauritsen, Am. Jour. Roentgenol. and Rad. Ther., 1933, XXX, 380–387 and 529–532.)

The question then arises as to whether the definition should or should not include the effect of scattered radiation. If it should, we are confronted with the problem of how to devise an apparatus which will definitely include all of it. Technical difficulties at the higher voltages—above 150 K.V. (r.m.s.)—seem at present to render this impossible. Should we then include only a definite fraction of the scattered radiation in the measurement and, if so, what shall that fraction be and how shall we define and determine it? As this does not appear feasible with our present knowledge the only alternative in making measurements is to avoid all effective secondary radiation as far as possible, and

[2] Paragraph numbers refer to the new recommendations given in Section I.

to reword the definition so as to embody the necessary conditions.

Our recommended definition appears to be capable of realization at high voltages without going to excessively large ionization chambers. Photographs made in a Wilson cloud expansion chamber 30 cm. in diameter (Lauritsen) show that for a narrow 700 K.V. x-ray beam practically all of the ion paths are confined within 10 cm. of the beam, and the bulk of them within the geometrical beam. Furthermore, it is seen that practically no ion tracks *originate* in the space outside the geometrical beam. This shows clearly that (1) the effect of radiation scattered from the chamber walls is negligible, and (2) there is no appreciable ionization due to photo or recoil electrons produced by secondary (degraded) radiation scattered out of the main beam. (The presence of tracks originating outside the beam indicates the entrance of a scattered quantum into that region, with the subsequent collision with an air atom and the production of measurable ions.)

The objection might be raised that, whereas the definition of the roentgen definitely excludes scattered radiation, it is impossible to realize this condition experimentally. It must be pointed out, however, that it appears to be possible experimentally to include not more than 1 per cent of the measured ionization as due to scattered radiation. Furthermore, it is possible to make rough corrections for the effect of the remainder of the scattered radiation and thereby reduce its net effect to less than 1 per cent of the total. Thus, although we cannot quite realize the ideal definition, its value is in no way impaired.

The newly defined definition can be realized by the use of: (1) suitable diaphragms to stop secondary x-rays and secondary electrons from the filters and diaphragm edges from reaching the measuring volume; (2) a narrow and well-defined x-ray beam, and (3) materials of low atomic number for the walls and electrodes of the chamber. If the diameter of the beam is sufficiently small in comparison with the distance in which the beam loses, say, half its energy, then the fraction of the degraded radiation which is absorbed in the measuring volume directly, or scattered back from the surroundings, may be neglected. Since the energy absorbed from the beam is transferred largely to recoil electrons and thence to ions, it may be determined directly from the measured ion current which gives the number of ion pairs. It is obvious that the inclusion of an appreciable amount of scattered radiation would render such a determination very uncertain at best.

It will be noted that in the proposed new definition of the roentgen we have deleted the phrase "and the wall effect of the chamber avoided." This is done for simplicity since "wall effect" merely refers to secondary radiation, and by the new definition "scattered radiations avoided" covers this also.

Confusion in the present international definition is also brought about by the consecutive inclusion of the terms "conductivity," "e.s.u. of charge," and "saturation current," which frequently leads to a misinterpretation of the definition by physicists and engineers who are not thoroughly conversant with the field; hence, the change from "at saturation current" to "under saturation conditions."

The considerations outlined above led the Committee to consider ideal physical definitions of the roentgen which would avoid any possible ambiguity, such as exists with the present definition. One such definition might be of the following form:

"The roentgen is the time integral of that flux density of radiation which, when the secondary electrons are fully utilized and the effects of all scattered radiation avoided, produces in one gram of air one electrostatic unit of charge in ion pairs." The quantity of radiation corresponding to the unit under this particular definition would be about $^1/_{800}$ that of the present roentgen. To make the unit, under this definition, of the same size as the present roentgen would necessitate a change in the definition from 1 e.s.u. to 773.4 e.s.u. or a

change from 1 gram of air to 0.001293 gram. Another possible improvement in the definition might be obtained by substituting for the last phrase in the definition above, "produces in 1 gram of air, 1,621 $\times$ 10^9 ion pairs." This last would be in agreement with the present international definition.

It should also be pointed out in connection with our proposed definition that the term "secondary radiation" may itself need further definition. This is particularly so at x-ray potentials in excess of 1.2×10^6 volts where it may be necessary to consider the effect of neutrons, protons, recoil nuclei, etc. Thus a rewording of the definition along the following lines may be indicated: "The roentgen is the quantity of x-radiation which, when all electrons and positive ions produced by secondary processes and processes of higher degree are measured, and the effects of all scattered radiations involving interaction of material outside of the measuring volume are avoided, produces in one gram of air one electrostatic unit of charge in ion pairs." It is obvious that with the increasing use of ultra-high voltages the present definition of the roentgen should be subjected to a very close scrutiny.

¶ 3. The term "irradiation" is substituted for "intensity" to avoid confusion caused by analogies between x-ray definitions and light definitions. With the roentgen defined as above for the *quantity* of x-radiation, the term "intensity" should not be used to describe the number of roentgens per unit time. To preserve the analogy to the universally accepted light definitions, use of the term "intensity" would require a different definition of the roentgen. The term "irradiation" has been used sufficiently in the sense of the above definition to warrant its general acceptance. The use of this term to express the roentgens per unit time offsets the disadvantage involved by defining the roentgen as a "quantity" of radiation.

To avoid troublesome fractions, irradiation is given in roentgens per minute (r/min.) rather than in roentgens per second, inasmuch as irradiations used clinically are usually greater than 1 if expressed in r/min.

¶ 4. This paragraph has been reworded to avoid the self-contradiction of ¶ 5 of the 1931 International Recommendations (see Section III). Since the roentgen is defined in terms of the ionization in free air, measurements made with a thimble chamber on the skin or at a depth (both producing scattering) are not in terms of true roentgens because of the wall effect. For measurements made on the skin or at a depth, the results must be described in some unit other than the roentgen. This emphasizes the need for a unit of tissue dosage, and such a unit is now under consideration by the Committee.

¶ 5. This is to amplify ¶ 4 above and show how to designate such measurements as are made where tissue scattering is included.

¶ 6. This replaces ¶ 4 of the 1931 International Recommendations, which was more or less without significance. It serves to emphasize the fact that there is generally satisfactory agreement between the x-ray standards of several countries. It should not, however, be obligatory to use different standardization methods.

¶ 7. This is principally a rewording of ¶ 6 of the 1931 International Recommendations to emphasize the need for calibrating secondary ionization chambers for the *exact* purpose for which they will be used.

¶ 8. The term "dosage meter" for the practical clinical measuring instrument has led to a great deal of loose thinking and misconception regarding the significance of roentgen measurements in the radiologic clinic. A dose is a product of the number of roentgens by some factor which takes all scattering into consideration, hence, it is obviously impractical with our present knowledge to devise a simple physical instrument to measure such a quantity. The secondary instruments ordinarily employed measure only in terms of roentgens and, as shown above, are only strictly valid when measuring the beam without

scattering. Measuring instruments fall into two distinct classes—one which measures the irradiation of a beam and the other which measures the quantity of radiation in a given time. To avoid confusion and avoid use of the term "dosage meter," it is desirable to distinguish between these two classes of instruments.

¶ 9. Control of the constancy of a secondary measuring instrument according to the 1931 International Recommendations is not necessarily positive unless the ionization is produced in the chamber itself by the gamma rays. (For example, a gamma ray test applied to the electroscope or an auxiliary chamber would not necessarily detect a broken collector electrode in a thimble chamber.) Control measurements made in accordance with the above will invariably detect faults in the instrument. There is no doubt but that irradiometers and roentgenometers will eventually be in the same class as voltmeters and ammeters, and that the methods for testing their constancy will be abandoned. If such instruments are reliably and ruggedly constructed, then methods for testing their constancy should not be required outside of the calibration of the instrument with a standard air chamber.

¶ 10. (Replacing ¶ 9 of the 1931 International Recommendations.) In the past, radiation qualities have been expressed in a variety of ways, most of which have their favorable aspects. These included half value layers in copper or aluminum, effective wave length (5 varieties), average wave length, and full absorption curve. The half value layer method has been most generally used (at least abroad) but, as in the case of the full absorption curve, requires a number of separate measurements for its determination. The full absorption curve method has been repeatedly shown theoretically and experimentally to give an adequate representation of the composite radiation quality, and from such a curve all other expressions of quality may be directly derived. Thus the future use of such a curve for expressing quality will permit immediate correlation with all quality data presented in the past by whatever method. This is not readily possible with any other methods now in vogue.

A difficulty in the past in the use of a full absorption curve for describing quality was the inability to express it as a single numerical magnitude. However, this may now be avoided by referring as a base to the absorption curves obtained with constant potential. Thus an absorption curve starting with a stated filter may be expressed by the constant voltage necessary to produce it, and hence all qualities of radiations may be reduced to terms of an equivalent constant potential. Such a method is of distinct advantage in comparing x-ray beams produced by generators of widely different wave forms. This is also important at voltages up to 1,000 K.V., such as are now being used in this country clinically. The voltage wave form of such generators vary from a constant one to one with very narrow peaks, and at a given peak voltage the quality and effectiveness may vary between wide limits. By comparing the absorption curves of such radiations with the absorption curve for a constant potential, they may all be reduced to a common base and the voltages may all be expressed as some equivalent voltage.

¶ 11. (Replacing last sentence of ¶ 9 of the 1931 International Recommendations.) The requirement has been added that, in reporting a roentgen treatment, the skin-to-target distance be given. The need for this is obvious because of the influence of the skin-to-target distance upon the percentage depth dose.

III. 1931 RECOMMENDATIONS BY THE INTERNATIONAL COMMITTEE

(1) The International Unit of X-radiation shall be the quantity which, when the secondary electrons are fully utilized and the wall effect of the chamber is avoided, produces in 1 c.c. of atmospheric air at 0° C., and 76 cm. mercury pressure, such a degree of conductivity that one electro-

static unit of charge is measured at saturation current.

(2) The International Unit of X-radiation shall be called the "roentgen" and shall be designated by the letter "r."

(3) The "intensity" of the radiation shall be expressed in r per second.

(4) Various standard methods shall be employed to establish the unit.

(5) All data given in roentgens (r-units) shall be supplied with an index to distinguish between the incident dose which does not include the scattered radiation, and the effective dose which includes the scattered radiation.

(6) For all comparative purposes it is advisable to employ ionization chambers which have been calibrated in terms of a standard chamber for x-radiation of the various qualities employed. It is also advisable to make the wall effects of these chambers as small as possible.

(7) The practical instrument used to measure x-ray output shall be called a dosage meter (dossismesser, dosimetre).

(8) The constancy of the indications of the dosage-meter shall be tested by means of gamma radiation emitted from a definite quantity of radium element, the measurement being carried out always under the same conditions.

(9) Any specification of dosage is incomplete without specifying the quality as well as the quantity of the radiation. For practical purposes it suffices to specify the quality of the x-radiation in terms of the half value layer in copper when this value exceeds 0.1 mm. of copper, or in terms of the half value layer in aluminum for radiation of less penetration; in all cases the value of the maximum voltage applied to the terminals of the tubes shall be stated. The specification of the dosage shall also include in addition to intensity and quality such factors as the intervals between the times of irradiation and size of field.

The International X-ray Unit Committee further recommends that:

(1) The experimental methods of establishing a standard for the determination of the International X-ray Unit shall be entrusted to a sub-committee consisting of the following members of the Unit Committee: M. de Broglie (France), W. Friedrich (Germany), E. A. Owen (Great Britain), R. Sievert (Sweden), I. Solomon (France), E. Pugno Vanoni (Italy), L. S. Taylor (U.S.A.), (Honorary Secretary of the Committee, E. A. Owen). This Committee shall invite the collaboration of the various existing national bureaus for standard measurements and also those about to be instituted.

(2) This Committee shall consider: (*a*) methods of controlling the constancy of dosage meters; (*b*) the correlation of x-ray and gamma-ray dosage; (*c*) the establishment of a gamma-ray unit of intensity.

(3) The progress of the work done by the sub-committee shall be reported once a year to the members of the International X-ray Unit Committee.

(4) Each country shall be requested immediately to elect its two representatives on the International X-ray Unit Committee; until new representatives are elected the present members shall serve.

(5) The International Committee shall henceforth be called "The International Committee for Radiological Units."

Approved by the Committee in Chicago, Sept. 27, 1933.

In connection with paragraph 9 of these recommendations for the ICRU 1934 meeting, Glasser pointed out,

> "There is no doubt that the methods for testing constancy of an irradiometer or a roentgenometer will eventually be abandoned and these instruments will be in the same class as a voltmeter or an ammeter. If irradiometers and roentgenometers are constructed reliably, then methods for testing constancy are not necessary outside of the calibration of the instrument with the standard air chamber."

While there had been enormous improvements in the reliability of clinical dosimeters (as they were now called), they were still sufficiently delicate, both as to construction and current measurements, and required occasional checks to insure that the calibration had not shifted.

Although the roentgen had been in use since 1928, there was still confusion in the minds of some of the more experienced radiologists. For example, at the business meeting of the American Roentgen Ray Society in September 1933, Dr. Ross Golden of New York City proposed that a special committee be appointed to consider the question of whether roentgens should be expressed in terms of the measurements made in free air or with backscattering from the patient.

Dr. Golden spoke as follows,

> "It has quite properly become the custom to record roentgen dosage in terms of international roentgens. Unfortunately, there are two methods invoked for measuring international roentgens. One without backscattering and one with backscattering.
>
> "When one says he gives so many roentgens, it is impossible to tell what he means unless you know whether he did or did not use backscattering. It seems to me fitting that this Society should lend its authority and influence towards standardization of the use of the term "roentgen" as a measure of dosage as published in papers, so when we read "roentgen" we may know what the writer means.
>
> "I therefore move that a committee of three be appointed, including one physicist, to decide whether it shall be considered by the Society, correct to record roentgens without backscattering, or with backscattering."

Dr. Pancoast in discussing this said,

> "I think the subject has been pretty well covered by the Safety Committee. It would be a wise procedure to refer this to the Safety Committee. That will all come under the National Safety Committee in the end. Mr. Lauriston Taylor is Chairman of that Committee and is in a better position to handle it."

The Safety Committee to which Dr. Pancoast was referring was the Advisory Committee on X-Ray and Radium Protection, the predecessor to the National Committee on Radiation Protection. Actually this was not a question for that committee, but rather for a committee such as the Standardization Committee of the RSNA.

Further discussion included a comment by Dr. Grier.

> "The roentgen unit is a certain definite thing, and how you measure it is something else. A man may want to make it in air and may want to measure it on the patient's body. For this Society to lay down rules and tell a man how to measure roentgens is hardly a proper subject."

In pointing out that the appointment of a new committee seemed unnecessary, Dr. Golden responded,

> "Unfortunately, that does not hold in the literature I have been permitted to read. I have read articles in which I was unable to tell whether the writer referred to roentgens with backscattering, or roentgens without. Therefore, I did not know what dosage he was talking about. There is no intention to tell any man how he shall measure his dosage, but I happen to know in one institution, dosage is recorded in roentgens measured on top of a hot water bag. That doesn't agree with the kind of roentgens I

happen to know about. A roentgen is a roentgen, but we all have to talk about the same method of measurement or we don't understand each other."

(This kind of question was raised frequently for a number of years. Later, in response to the request from the two radiological societies, the Standardization Committee made many abortive attempts to prepare a standard treatment chart and protocol to be used in the clinical applications of radiation. More on this later.)

During the 1930's, there was a gradual turnover of SC/RSNA membership as less active members were retired and new members were added. Although the list below is not chronological, it represents all changes after 1934. G. C. Laurence, Ph.D., National Research Council of Canada (12/34); E. A. Pohle, University of Wisconsin (12/35); J. D. Gendreau, Montreal, Quebec (12/35); Edith Quimby, M.D., Memorial Hospital, N.Y. (12/35). Deleted: Compton, Duane (12/35). Added April 1940: Robert B. Taft, M.D., Charleston, S.C.; Richard Dresser, M.D., Boston; George C. Henny, M.S., Philadelphia; Kenneth E. Corrigan, Ph.D., Detroit. Deleted April 1940: Ullman, Richards, Meyer, Failla.

On December 17, 1940, Dr. Portmann, Sub-Chairman of the Standardization Committee, proposed some additional changes in the committee membership, especially relating to radiologists. He stated as follows:

"I think the following list would correlate our work satisfactorily.

"Physicians:
E. C. Ernst, St. Louis, Mo.; W. E. Chamberlain, Philadelphia, Pa.;
R. R. Newell, San Francisco, Calif.; R. B. Taft, Charleston, S.C.;
R. S. Stone, San Francisco, Calif.; B. P. Widmann, Philadelphia, Pa.;
F. O. Coe, Washington, D.C.; and U. V. Portmann, Cleveland, Ohio.

"Physicists: Same as before.

"My reasons for changes are (1) Erskine and Dresser have shown no active interest in the committee; at least have not attended meetings, (2) Stone would be a good man to replace Dresser because he is working with "supervoltage" apparatus and neutron radiation, (3) Widmann would replace Erskine because he is a member of Committees of the A.R.R.S. and Radium Society, (4) As we suggested at our last meeting Coe should be added as a member of the American Standards Association. Also he is on the A.R.R.S. Committee."

CHAPTER 8. DEVELOPMENTS OF X-RAY GENERATING EQUIPMENT

Supporting Measurement Systems

In the use of x-ray generating equipment, including the high-voltage sources, it was crucial to be able to control the x-ray output so that variations in the beam intensities would be as much under 1 percent as possible. This meant careful control of the high voltage as well as the tube current. For high-voltage machines, giving wide variations in voltage waveform, it was also necessary for some purposes to understand the significance of whatever voltage measurements were made. In some instances this might involve waveform analysis.

In the earlier days, high peak or crest voltages were measured with a spark gap, the most common of which was described as the "needle gap." There were fairly reliable, engineering tables available, giving the voltage corresponding to a particular needle gap separation. The tables were for a real needle gap--sewing needles of described dimensions. A difficulty with this was that after one or two sparkovers between the needle points, the needles would be sufficiently damaged so as to alter the breakdown electric field and hence the separation of the points for a given voltage. In the case of commercial equipment, it was even worse because the so-called needle points were frequently tips of 1/4-inch rods that had been ground down to a point, making a solid angle of about 60°.

A better technique was the use of sphere gaps with reference to standard spark gap voltage tables. For this purpose, by the midtwenties, most x-ray installations included a sphere gap consisting of two spheres approximately 5 inches in diameter, the separation of which would be controlled by strings from a safe distance. However, the readings were subject to errors because of the deposits of dust on the spheres, and pitting of the spheres due to sparkovers. Thus, they could not be used for moment-to-moment voltage control.

Water resistors in glass tubes had been tried, but were not reliable. Again in the 1920's, various experimenters made their own electrostatic voltmeters, usually in the form of a lightweight dumbbell-shaped electrode placed in an electric field between some larger spheres, so that the field force would cause a rotation of the dumbbells against a torsion suspension. There were various ways of calibrating these voltmeters. Taylor used one of them inside a copper-shielded box about 3 feet on each side. Though it was satisfactory for control purposes, its damping period was inconveniently long. Still another useful type was made by Dr. W. W. Nicholas, who joined the X-Ray Group in 1928 (Ref. 23).

This was the situation facing the laboratory following the installation of its 200-kV constant potential generator. At this point, the Shallcross Manufacturing Company in Philadelphia marketed some noninductive wire-wound resistors, with resistances as high as 1 megohm for a unit approximately 1 1/2 inches long. The ceramic (Isolantite) base was arranged in a series of bobbins, leaving thin fins running out between each to the circumference (Ref. 15). As each bobbin was wound to the desired capacity, the fine wire was then shifted to the next and its direction of winding reversed. By this means, the resistor was rendered noninductive to a considerable extent, and was quite adequate for the radiation laboratory purposes.

Each resistor was tested exhaustively at its full rating of 1 watt, which corresponded to a voltage drop of 1 kV. Tests made before and after loading rarely showed any change of resistance. There was a temperature coefficient of resistance which could be evaluated and expressed in terms of the current flowing at any time. Because such resistors were in the open and accumulated dust as the result of corona discharges, it was necessary to shield them. To accomplish this, groups of five resistors were placed on a rack inside a spun-metal container with holes for air ventilation and an insulating disk on the open side to which the next unit could be fastened. Each "pancake" could measure up to 5 kV and the assembly up to 100 kV (see photos No. 25, 26, and 31). With two of these assemblies, continuous voltage control was possible using the resistor stacks simply as a voltmeter multiplier.

Because of the success of this type of voltage measurement, the Shallcross Company began to manufacture them for use in various laboratories around the country.

For the 600-kV constant-potential voltage supply, built in the midthirties, a different voltmeter multiplier was used. The voltage multiplier made use of a new type of ceramic 1-megohm resistor, made by the Erie Resistance Company, with better stability and temperature coefficient characteristics than any other ceramic resistor available at that time (Ref. 62). Strings of 20 resistors each were slipped into thin insulating tubing and suspended between metal plates with rounded rings at the edges to reduce corona. Thirty such units were made, separated about 3 inches from each other by isolantite ceramic insulators. The entire stack made a reasonably well-shielded high-voltage column with the bottom plate at ground potential and the top plate at the full machine potential (see photo No. 27). This proved to be not quite as accurate as the wire-wound resistors, but agreement was within 1 or 2 percent and quite adequate for control purposes.

The last step in the development of this type of voltmeter multiplier involved the 1.4-million-volt installation in 1940. For this, a later type of Shallcross noninductive resistors were utilized which were 1 inch longer than the previous ones, and of special design to allow 12 pancakes or bobbins on each resistance unit and more fin area for cooling. Fourteen hundred such units, having large copper toroids at the edge to minimize corona, were divided equally among 100 shields. The individual assemblies of 14 resistors were each exhaustively tested before being put into use. (No known critical changes took place in them during their 20 years of use.) These in turn were assembled into 10 units of 14 megohms each, fed through from the top into a long insulating column containing voltage distribution rings at 10 equal intervals, and tied into the high-voltage stack which will be described later (see photos No. 28, 42, 43, and 44).

The several types of voltmeter multipliers described above had their maximum accuracy and reliability only for direct current with relatively small ripplage. This was a feature of all of the constant potential equipment which was made at the Bureau. It was found, however, that the same equipment could be used satisfactorily with 3-phase rectifier units where there was essentially a ripplage of 5 or 10 percent. This was to be compared with the ripplage on the constant potential generators, which was about one or two tenths of one percent per milliampere.

While the high-resistance voltmeter multipliers were also used on the mechanical rectifiers in the laboratory, they were not reading the peak voltage. Except for inductive losses, they were reading root-mean-square (rms) or effective voltage. As it turned out, this appeared to be an advantage. In an endeavor to obtain some better idea as to voltage waveform when using the mechanical or thermionic rectifiers, several inexpensive methods were tried. At the time, the laboratory had about five different kinds of commercial mechanical rectifiers available for study and comparison. In each of these, of course, it was necessary that the rotating rectifier switches be driven by synchronous motors and were thus tied-in precisely to the line frequency.

To examine the waveform, a capacity voltage divider was used. This consisted of a string of ordinary high tension mushroom-shaped insulators, hung in a string of 10 units with the bottom one tapped for voltage takeoff at a tenth of the peak. This was fairly satisfactory for mechanical rectifiers, and half- and full-wave rectifiers, but, of course, was of no use for constant potential. To examine the waveform, a very simple high-voltage string electrometer was used, designed on the same general principle as the idiostatic electrometer for measuring very small ionization currents. By means of a very fine wire (in place of the Wollastan fiber in the electrometer), the waveform could be followed and plotted by the motion of the wire when observed in the light field of a stroboscope which was tied into the same voltage supply as the synchronous motors driving the high-voltage switches (see photo No. 29). The method was crude, but adequate, though it was of no use in determining the fine structure of the waveform from the mechanical rectifiers which were very sawtoothed in nature due to the continuous sparking taking place. (Cathode-ray oscilloscopes were not yet available in those days.)

During this period the question of voltage measurement and control was also of concern at the Naval Research Laboratory where Dr. Ross Gunn developed a generating voltmeter consisting of a rotating sector that, alternately, was exposed to an electric field and then shielded, two times per revolution. The rotating sector could be either in the form of a flat disk or a rotating cylinder. When exposed to a constant potential field, the voltage change generated by interrupting the field could then be read with high accuracy on a potentiometer. When driven by a synchronous motor, this type of voltmeter was as accurate as the frequency control on the power company lines, and for many applications was very useful. One of these voltmeters was used as an auxiliary control for the NBS million-volt equipment to be described later. At high voltages, however, variations in corona due to dust could adversely affect the readings.

From the very outset of x-ray studies in 1927, the line voltage variations in the supply line were a serious problem. This was further aggravated as research activity developed throughout the Bureau, overloading many lines and subjecting them to frequent load changes. To overcome this, the X-Ray Laboratory procured a 25-kVA synchronous motor-generator set as an independent power source and located it in the power plant with extra heavy connecting lines to the East and the Northwest Building Laboratories to minimize power loss. Since the generator was driven by a synchronous motor with its own field exciter, the voltage was about as steady as could be obtained. Later, the field of the generator was excited by storage batteries, and hence, the line voltage itself could be varied over a wide range by means of a remotely driven resistor control system which was a part of the 200-volt storage-battery facility.*

*Unknown to the NBS staff was a serious problem that the x-ray machine operation was causing the Washington Airport, then located on the present site of the Pentagon. Incoming planes on their flight path down the Potomac River were frequently experiencing radio blackout at the most critical point of their landing operation. Airport officials contacted the Bureau expressing belief that the source of the radio interferences were the x-ray machines in the area. They were indeed correct. The interference was due to the mechanical rectifier at the Georgetown University Hospital x-ray installation, which was within a couple of thousand feet of the flight path, and which was similar to the installation at NBS. With the aid of the NBS Radio Section, it was determined that the overhead high-voltage system used in all x-ray plants acted like a large antenna array transmitting a high-frequency "hash" caused by the sparking of the mechanical rectifiers. Fortunately, the problem was immediately solved by use of a 3/8-inch copper tube helix, about 3 inches in diameter and 18 inches long, which was placed in each high-voltage lead next to the rectifier. This choked the high-frequency "hash," which was in turn grounded by disks placed along side of the helix. The system solved the problem at both the NBS and the Georgetown installations.

NOTE: The multi-disciplinary makeup of the NBS staff at that time (indeed throughout NBS history) was of enormous help to the x-ray research staff in solving the many technical problems encountered. Moreover, the ready cooperation and availability of such expertise not only expedited the progress of the programs, but contributed materially to the quality of the results. The reverse was equally true--the x-ray staff loaned its expertise and its unique equipments to advance other laboratory programs.

A high degree of cooperation was provided by the NBS Electrical Division, through Drs. F. B. Silsbee and F. M. Defandorf and their staff. Most helpful was Dr. M. B. Brooks, an outstanding expert on bridge and potentiometer circuits and their application. In addition, a wide range of calibrated electrical instruments of all degrees of accuracy were freely available on loan. For longer periods or permanent needs, purchase assistance of the best suited instruments was available. Not until after World War II did an intricate cost accounting force the addition of a price tag on such cooperation.

X-Ray Tubes

As noted earlier (p. 2), the Coolidge hot cathode x-ray tube had been introduced in 1913 and was used extensively during the war years. With the war over, radiologists, many of whom had received their radiological training in the Army, discontinued as rapidly as possible the use of gas tubes in favor of the hot cathode tubes. The bulk of the clinical applications were in the diagnostic area with tubes operating generally below 100 kV. For these tubes, satisfactory production facilities had been developed so that, by and large, there were no serious operating problems involving tube reliability and general performance.

By the end of the war, the so-called "universal" hot cathode tube had been designed to operate up to about 140 kV, primarily for therapeutic purposes. The use of this tube involved at least two major problems in the clinic. One was the necessity for new and higher voltage equipment. In spite of the fact that the hot cathode tube could rectify its own voltage, it operated better and more reliably on rectified voltage, which at that time was provided only by synchronously driven mechanical rectifiers. In the early twenties, there were some six or eight suppliers making rectifiers with different design characteristics and thus, with different voltage output and voltage regulation.

The second problem was that with 140-kV radiation, x-ray shielding became more serious. Relatively simple and effective heavy lead glass shields could be used with diagnostic

tubes, but these were not practical on the therapy tubes. Initial protection consisted of a lead glass bowl with a bottom opening for emitting the radiation to be applied to the patient, but with a wide-open top that allowed radiation scattering. This required the construction of lead-lined rooms and various kinds of baffles and cubicles to shield the operator. Attempts to completely enclose the x-ray tube in a lead shield were frustrated by the lack of technology. When suitable insulators did become available, initial efforts were to enclose the tubes in large lead-lined boxes with the high voltage applied through suitable insulator bushings. This, together with the high-voltage conductor systems from the rectifiers to the tubes, introduced problems because the capacity effect interfered with the voltage waveform and the control and regulation of the transformers.

During this same period of the 1920's, an even more serious problem was introduced when the hot cathode tube design was extended to operate at 200 kV with a tube current of 8 mA. As far as transformer equipment was concerned, an extension of the voltage range simply involved the use of engineering principles already developed for the 140-kV tubes. Similarly, the problems in shielding were again a matter of degree rather than kind. However, as the voltage went up, the problems seemed to increase exponentially with respect to the voltage.

The first hot cathode tubes were produced in Schenectady, under laboratory and research conditions where problems could be worked out of the production process by the General Electric staff, who had extensive experience in high-vacuum techniques, out-gassing procedures, and high-voltage operations. During the war, these tubes were manufactured by the Victor X-Ray Company in Chicago which, after some initial production headaches, turned out a reliable product. The next tubes, for operations at 140 kV, were again developed in Schenectady and produced in Chicago. But, at this point, the problems had multiplied and the tubes were less reliable than the diagnostic tubes. (Nevertheless, of all of the early therapy tubes made, the universal tube acquired a good reputation for its dependability.)

A major change occurred with the introduction of the 200-kV tube. Again, the first of these were being made in Schenectady, as research tubes rather than as routine "on-the-shelf tubes." These were supplied to institutions having effective capability of exploring their use.

However, commercial production demands, when undertaken by the Victor X-Ray Company, were such that the reliability of the tubes was unsatisfactory. At that time, the General Electric Laboratory and the Victor X-Ray Company were virtually the only tube suppliers in the United States.

It was during this period of discontent over tube performance (in the middle and late 1920's) that the Bureau of Standards initiated its major x-ray program. There was a wide variety of high-voltage generators in use, and hence a natural tendency for the tube manufacturers to blame poor tube performance on the generators, and vice versa. The Bureau undertook a study of a variety of commercial x-ray generating equipment to determine the operating characteristics of each, and to evaluate what should be expected from each in combination with various x-ray tubes in terms of quality of radiation, output, and tube performance.

Mechanical rectifier units for 200-kV generation were borrowed from the General Electric Company, Wappler, Kelley-Koett, and Waite and Bartlett. Also acquired were a constant potential set with a four-tube constant potential generator from the Wappler Company, and a half-wave kenotron rectifier set from General Electric (see photo No. 30).

By this time, the space available to the X-Ray Group had been substantially enlarged so that all of these pieces of equipment, in addition to the laboratory's own equipment, could be in place and operable at any time (see photo No. 31). An overhead high-voltage system was arranged so that any high-voltage unit could be placed on the line at any time. The line ran to three different shielded x-ray tube installations which could be connected in turn. With this flexible arrangement, studies of the tubes and the transformer rectifier equipment went on jointly. All of the manufacturers cooperated freely with the Bureau. The NBS laboratory staff took great care to insure that no particular advantage was given to any one manufacturer. For example, the tubes from all of the manufacturers were studied as were such items as line voltage stabilizers, transformer equipment, and measuring equipment. At the same time, the Standardization Committee of the Radiological Society of North America followed the experiments closely and took steps through society channels to make the information available to the profession. Taylor was also a member of the RSNA Manufacturer-Radiologist Committee which provided a very useful sounding board on the problems under study.

The x-ray tubes under study or in use for research purposes were suspended in a 4- x 4- x 8-foot box with 1/4 inch of lead surrounding it (see photo No. 31). High voltage was brought in through heavy insulators at the top. This provided adequate shielding of

personnel under most situations, and where the shielding was inadequate for high tube-loadings, personnel operations were handled by remote control. Work areas were surveyed with the portable survey instrument built in 1930 (see photo No. 31a and Taylor, 1967). While the personnel were not individually monitored, exposure levels in the working areas were determined and, after the first tolerance dose level was set in 1934, were found to be well below the level prescribed at the time.

Until the late 1920's, the Victor X-Ray Company's 200-kV therapy tubes were essentially the only ones available. A few were being brought in from Germany and Holland, but these were not notably superior to, or more reliable than, the American-made tubes. All tubes, while rated at 200 kV, were rarely operated at more than about 180 kV. This was in part because they were more steady and reliable at the lower excitation voltage. But the primary reason was probably one of economics. The clinics could not afford to keep replacing x-ray tubes whose lives were shortened under full-rated operating conditions.

With the market wide open for a good and reliable x-ray tube, the Westinghouse Company promptly initiated a production program. Moving quickly from the laboratory development to the production line, the company encountered essentially the same unsatisfactory results experienced earlier by the Victor Company, and ceased production. Meanwhile, the Machlett Company was starting to produce deep-therapy tubes. They had produced reliable gas tubes on a small scale for many years, and had not rushed into the hot cathode x-ray tube business during its early stages. However, by the late 1920's, they had produced a reliable deep-therapy tube. (Today, with a greatly expanded line, the company is one of the two or three principal suppliers in the country (Ref. 35).)

The original General Electric 200-kV, 8-mA tube was made of thin soda glass which had been used by them in all previous x-ray tubes. One of the causes of tube failures was glass puncture, thought to be related to deterioration of the glass by the intense x-ray bombardment. The glass also discolored to a deep purplish color which made it difficult to see the target in the tube.

Faced with this problem, General Electric introduced the same general model of tubes, but made with cerium glass. This tube discolored only slightly in use, becoming a pale yellowish-brown. Also, the tube operated more satisfactorily, though this might have been because each tube was more or less custom made. Meanwhile, the Westinghouse tubes, and later the Machlett tubes, were being made of heavy pyrex glass on the order of 1/4 inch in thickness, and operated more reliably than the thin glass tubes. Simultaneously, the General Electric Company introduced a heavy-wall pyrex tube. Thus, all three suppliers were supplying the customer with a very much improved product.

An interesting problem arose in connection with the thick glass tubes. In the Bureau's laboratory, it was quickly noticed that as the tube operated and became warmer, the output changed and, in turn, affected the voltage supply (Ref. 32).

Depending upon the tube and the generator, the output could decrease by as much as 20 or 25 percent in a few minutes of operation before reaching a steady state. As the output dropped, the voltage tended to rise, which required an adjustment in the filament current. Steady output could be maintained only in an installation such as that at the Bureau where these factors were readily known and controlled. However, this was not practical for clinical use.

Bureau studies showed that a blast of cool air on the bulb of the tube could change its operating characteristic and that suitable cooling of the whole tube could stabilize its operation from the outset. On learning of this situation, the Standardization Committee recommended that therapy equipment should be provided with beam monitors to insure that the output was uniform or corrected. Warnings were also made against making dose measurements under clinical conditions at only one arbitrary point in time after the start-up of the tube.

Fortunately, this did not remain a problem very long, because the introduction of the thick-wall pyrex tubes led to other avenues of improvement. By the middle 1930's, the open air tube had practically disappeared and was replaced by oil-cooled tubes having different glass configurations, making it feasible to much more easily provide the necessary shielding and cooling by enclosing the entire tube in oil. (Indeed it was not many years before the entire x-ray unit, including tube, transformers, and rectifiers, was to be contained in a single tank of oil and moved and operated as an entity.)

The studies of the radiation characteristics of different types of generators proved to be the most useful in demonstrating physical similarities rather than differences.*

*In addition to the cooperation of the equipment manufacturers, the Bureau benefited from a research fellowship under the then active Radiological Research Institute. This enabled the Bureau to bring on a Mr. Kenneth L. Tucker for about a year and a half.

When comparing generators with pulsating sources, such as mechanical rectifiers, with constant potential sources, it was found that comparisons on the basis of peak voltage would result in substantially different outputs and different radiation qualities. However, if both were measured in terms of effective voltage, or root-mean-square voltage (rms), both the outputs and the radiation qualities were much more comparable. Of course, the peak and the root-mean-square voltage for constant potential would be the same. The rms voltage measurements did not bring about exact equality, and the ratio between rms and peak voltage measurements varied noticeably with the current loading on the transformer. This was in part because, as already noted, the x-ray transformers were poorly regulated. Whereas a well-designed power line or instrument transformer would show a ratio very nearly 1:1.4 between rms and peak voltage, the deviations from this ratio were noticeable in the case of x-ray transformers. Poor regulation was desirable as a means of minimizing runaway situations in the event of tube overloading or someone coming into contact with a part of the high-voltage system. With an appreciable increase in current demand on the transformer, the voltage fell rapidly. This had a lifesaving effect on gassy x-ray tubes, whereas on a power transformer, a gas situation in a tube might result in an almost explosive current surge (Refs. 28 and 34).

Toward the end of the thirties, substantial changes in technology began to show up in equipment design. For example, with the new type of oil-cooled, oil-immersed tube mentioned above, it was possible to enclose the tube in relatively compact housings, providing both insulation and radiation shielding, thereby permitting very flexible radiotherapy installations. These tube housings were connected to the high-voltage rectifier systems by means of flexible high-voltage cables which would withstand potentials of 100 kV between the grounded shield and the conducting wire. The cables had the effect of adding a capacity to the high-voltage line, and this in turn had critical influence on some types of rectifying circuits. For example, on mechanical rectifiers this greatly increased the sparking at the synchronous switches and also tended to short out some of the high-frequency "hash." The first was a disadvantage and the second an advantage. However, because of poor regulation on the transformers, such a capacity load on the line frequency made substantial distortions of waveform, and this in turn affected the output in terms of a given peak voltage. On the other hand, measurement of the voltage in terms of root-mean-square voltage, rather than peak, again made outputs compare favorably with each other on the basis of rms voltage measurements. While rms voltage measurements seemed to be more significant, they were not practical from the point of view of clinical applications because they required apparatus such as that of the Bureau of Standards with high-voltage resistors used with voltmeters measuring rms voltage rather than direct voltage.

Because of the influence of the cables on the high-voltage systems, the replacement of mechanically rectified sources with thermionically rectified high-voltage sources was accelerated. This required a certain amount of "stiffening" of the transformer regulation, so that, with several other changes, the trend was toward more uniformity of output between generators. This also applied to voltage measurements because, except for constant-potential, peak-voltage measurements on any of the newer equipment yielded about the same output in terms of radiation quantity and quality.

X-Ray Quality Measurements

The term "quality" as applied to x rays is a loosely defined concept. It is generally related to the distribution of energy in the spectrum; that is, the part of the spectrum which impinges upon the patient after some degree of filtration. Quality has to do with the penetration and scattering of an x-ray beam as it enters material, e.g., tissue. As mentioned earlier, Taylor tried to establish the spectral composition of the x-ray beams soon after joining the Bureau of Standards. He started the construction of a double crystal spectrometer with which he had some experience at Columbia while working with Professor Richtmyer. However, as he became more familiar with the general field of radiation dosimetry and measurement, he realized that even if the spectrum could be measured with great precision, it would not be useful with respect to irradiation of a patient. It became clear that three different x-ray beams with different combinations of voltage and filtration would yield different spectra of no practical use. At this point, he stopped the work on

the spectrometer and wrote his conclusion in the paper "Absorption Measurements of the X-ray General Radiation" (Ref. 14).*

*At this time a German company marketed a small photographic spectrometer designed by Seemann. The result was obtained on a strip of photographic film which then had to be photometered to determine the radiation levels at the different spectral positions. This device came to the attention of J. W. DuMond, a prominent U.S. physicist working in fields other than x rays. He thought that here at last was the information that the radiologists had been looking for--a device which would tell them precisely what radiation they were working with. He wrote a paper entitled, "The Seemann Spectrograph Tells the Story" (DuMond, 1930), which caused confusion in the radiological profession because of his high standing as a physicist. The paper diverted a considerable amount of effort into undoing the idea that all the radiation quality problems had suddenly been solved.

There were at least three methods in vogue for expressing the quality of an x-ray beam. The first and oldest of these was known as the half-value-layer method. It was the thickness of material, such as copper or aluminum, which if placed in an x-ray beam would reduce its intensity to one-half.

Actually the half-value layer (HVL) would be most closely related to the chord connecting two points on the absorption curve to which it applied. For a still better definition, or description, a second half-value layer could be described. It was the thickness of material necessary to reduce to one-half the beam emerging from the first filter. This would, in essence, give three points on an absorption curve, which was obviously a refinement. It was useful to the extent that in comparing radiations from two different sources, a matching of two half-value layers would give better assurance that the radiations were the same. On the other hand, it would not help very much to tell how to adjust the beam of radiation in one institution to match that in another institution. (The HVL has limited usefulness in the multi-million-volt range, but the question was academic in the twenties and thirties.)

Another method made use of absorption measurements for obtaining an indication of the quality of an unknown heterogeneous beam. This method determined the wavelengths of the homogeneous radiation, which would be reduced in intensity by a given filter to the same degree as the heterogeneous radiation. The heterogeneous radiation would then be designated as having an "effective wavelength," equal to that of the corresponding homogeneous radiation. Duane, in proposing this method, used a filter of a given material of specified thickness for obtaining the effective wavelength of the radiation incident thereon. This would be done by reference to homogeneous radiation absorption data. A difficulty with this was the lack of data at the higher voltages then in use (Duane, 1927, 1928).

A. Mutscheller proposed another quality designation which he called "average wavelength." The average wavelength was obtained from the slope of the absorption curve at the point in question (Mutscheller, 1924). Thus, if the heterogeneous beam was filtered by some specified thickness of copper, the average wavelength would be related to the slope of the curve at the point corresponding to that thickness. Mutscheller divided his measurements into two classes: those for the portion of the absorption curve from zero to what he called the "homogeneity filter," and those for the portion of the absorption curve beyond that point. He defined the homogeneity filter as that value beyond which the absorption curve became a straight line, which, as it turns out, was a physical impossibility (Mutscheller, 1929; Taylor, 1929). (His measurement techniques were not sufficiently accurate for him to distinguish the curvature.)

In most of Mutscheller's published absorption curves, he showed such linearity above about 0.5 mm copper filtration in the beam. This was about the thickness of the filter for securing optimum elimination of the soft components of 200-kV radiation without unduly reducing the strength of the beam itself.

Taylor described a method (Ref. 14) called the "true effective wavelength," which was essentially the same as Mutscheller's average wavelength in that it defined the slope of the curve at a point. It differed from Mutscheller's in that it did not recognize the validity of a straight line relationship between the log of the intensity ratio and the filter. The method was considered to be more meaningful than the average wavelength.*

*The differences in viewpoints toward these several methods was always the source of lively discussion and occasional bickering during the physics sessions of the Radiological Society of North America meetings.

In due course the radiological community, generally, returned to the use of the half-value-layer method which has now been in continuous use since the mid-1930's. Nevertheless, it was true that for a variety of applications, it was desirable to have accurate absorption curves for filter materials, such as copper or aluminum, which would be used in the x-ray beams applied in therapeutic radiology. It was also necessary to have such curves for materials such as lead, steel, and concrete for purposes of designing protection shields. For these reasons, there had been a persistent effort to produce accurate absorption curve data in both tabular and plotted form. The Bureau issued the data several times as technology improved, and even within the last few years, new data developed by E. Dale Trout, have been made available through the NCRP (NCRP, 1976).

The first, rather comprehensive, set of absorption data for heterogeneous radiation through copper and aluminum filters were obtained by Taylor and Singer in 1934 (Ref. 38). They were the first that had been obtained under the rigorous control conditions then available almost solely at the Bureau of Standards. The beam was measured with the guarded-field ionization chamber using the current balance method and compensating system as described above. Voltages were controlled and measured by means of the shielded high-resistance voltmeter multiplier. A thick-walled x-ray tube was used for the higher exitation potentials and the soft glass, thin-walled tubes for the lower potentials, corresponding approximately to the voltage ranges in which such tubes were used in practice. Corrections were made for the thickness of the tube walls which, for the pyrex tube, was 4.8 mm. This was measured by means of a microscope, focusing first on the inner surface and then on the outer surface. In connection with these measurements, it was found that, for a given potential applied across a thick and thin glass tube, the absorption curves plotted on a semi-log plot were obviously displaced. If these two sets of data were plotted on separate sheets and placed together over a lightbox, one could be slid horizontally and vertically by a small amount to achieve a very close match between the curves. The displacement along the direction of the filter thickness axis gave a measurement of the equivalence of an x-ray tube wall thickness, in terms of the filtration being used. This has proven to be, at times, a very useful technique for determining the inherent filtration of an x-ray tube under the conditions of its use in a therapeutic installation (see p. 203).

The absorption data obtained in the Bureau laboratories extended to energies up to 180-kV constant potential, which is equivalent to some 230-kV peak generated by a mechanical rectifier or half-wave rectifier.

Early absorption data were also obtained in the energy range from 225- to 550-kV constant potential, using the facilities of a clinic in Lincoln, Nebraska. The x-ray beam in this case was filtered by a 6.4-mm steel wall and 13 mm of water, so that it was hard to directly tie to the lower energies which were much more lightly filtered. These were the first such data made for constant potential obtained at these elevated voltages. At a later time, new absorption data were obtained under better defined conditions, substantially modifying the curves obtained above 200 kV.

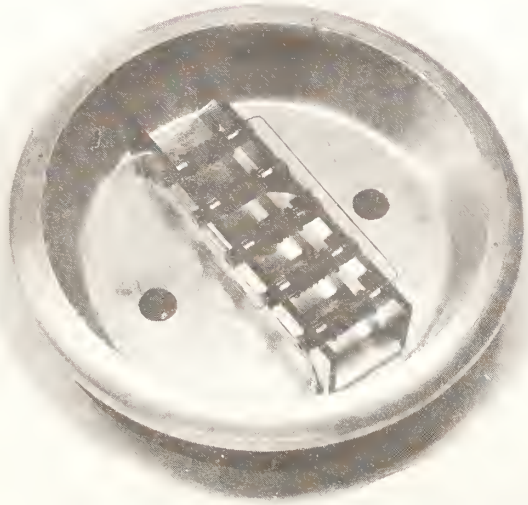

Photo No. 25. 5-kV shielded, wire-wound, high-voltage resistor unit (1930).

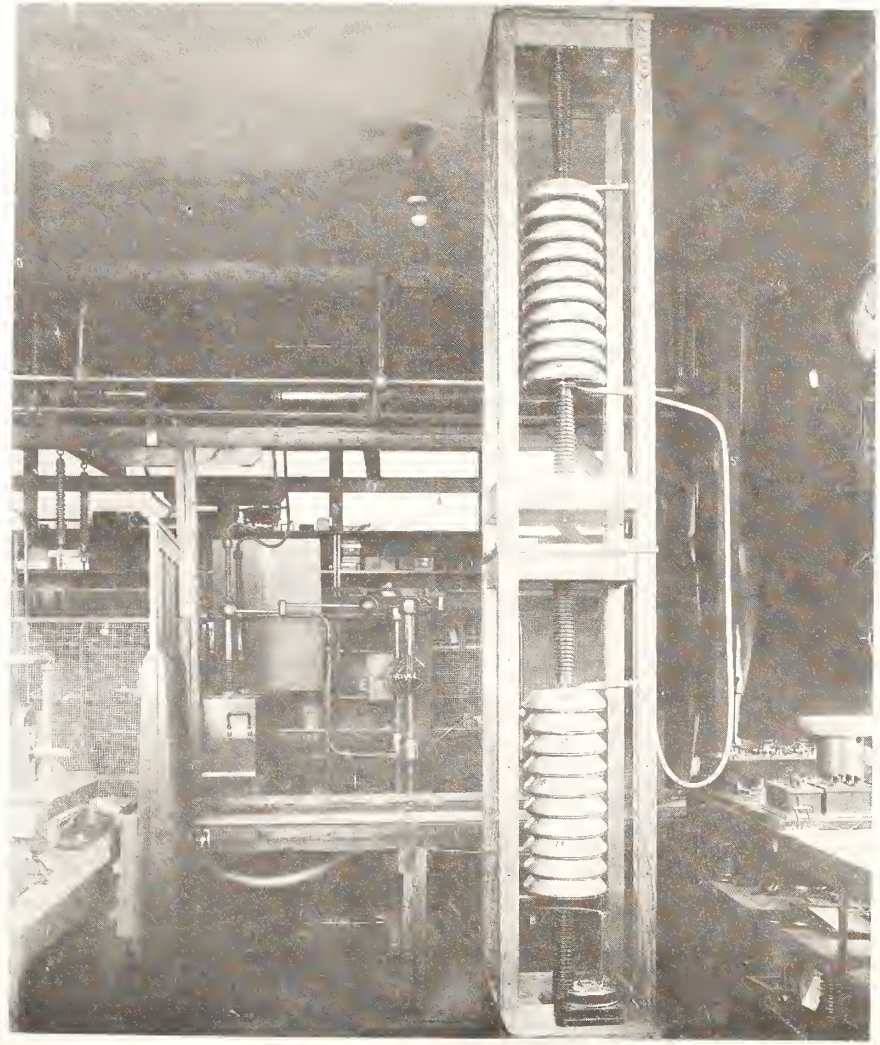

Photo No. 26. 100-kV shielded, wire-wound, high-voltage resistor assembly for voltmeter multiplier (1930).

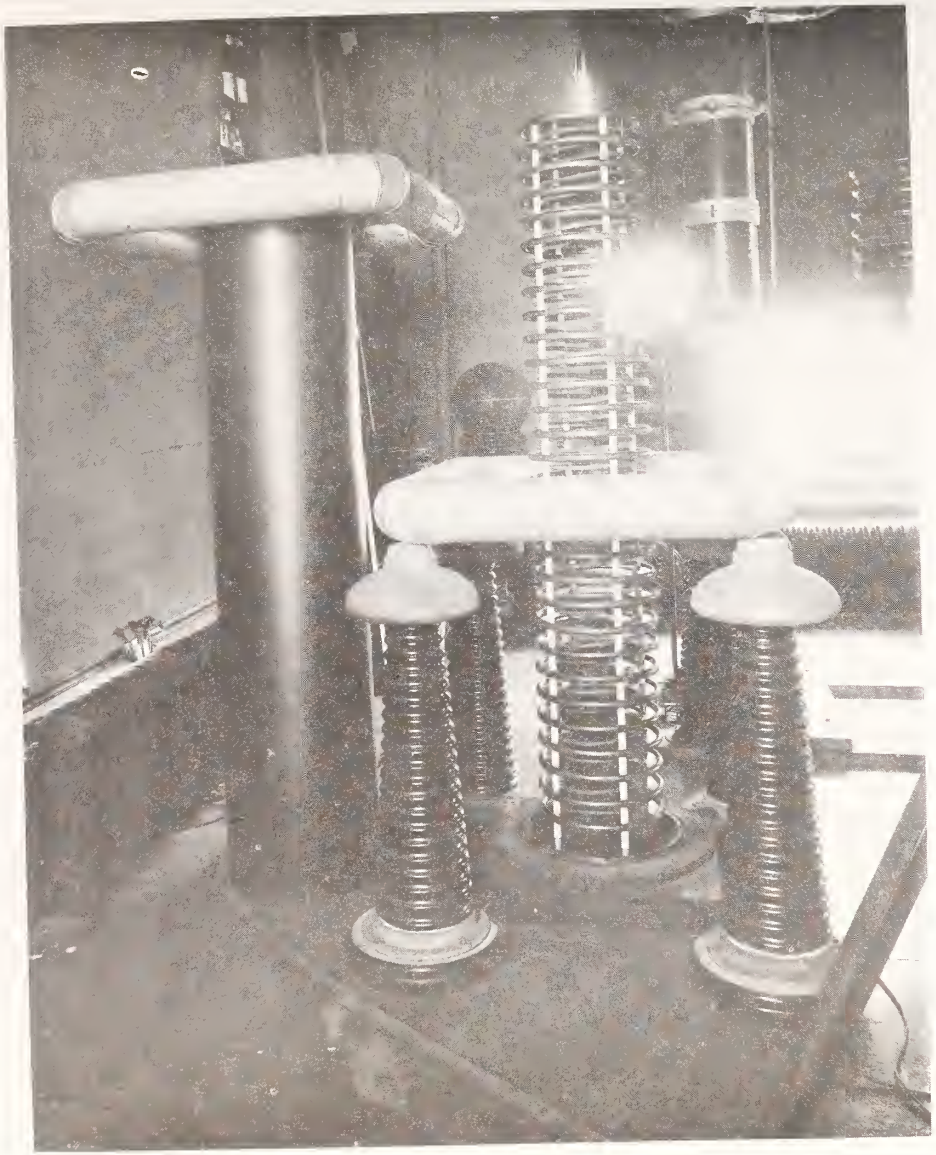

Photo No. 27. 600-kV shielded, high-voltage ceramic resistor assembly for voltmeter multiplier (1938).

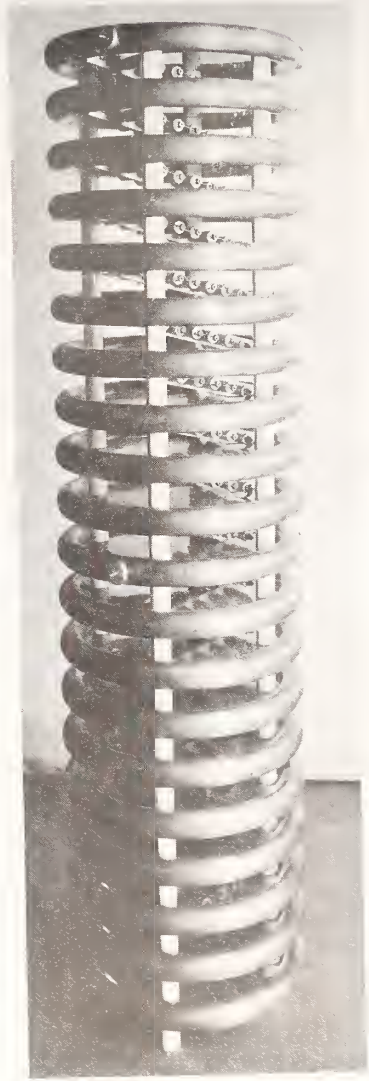

Photo No. 28. Two of 10, wire-wound, shielded, high-voltage resistor units for a 1,400-kV voltmeter multiplier (1940).

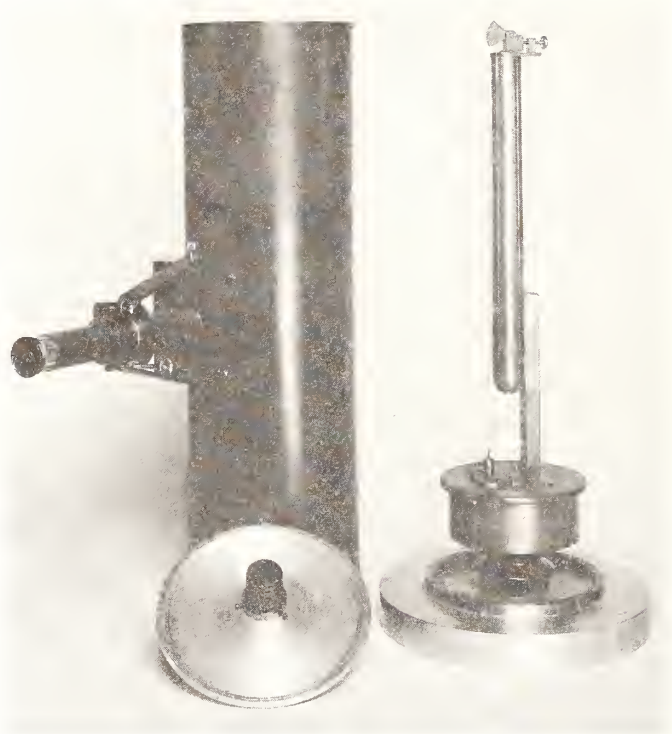

Photo No. 29. 10-kV idiostatic string electrometer-voltmeter (1931).

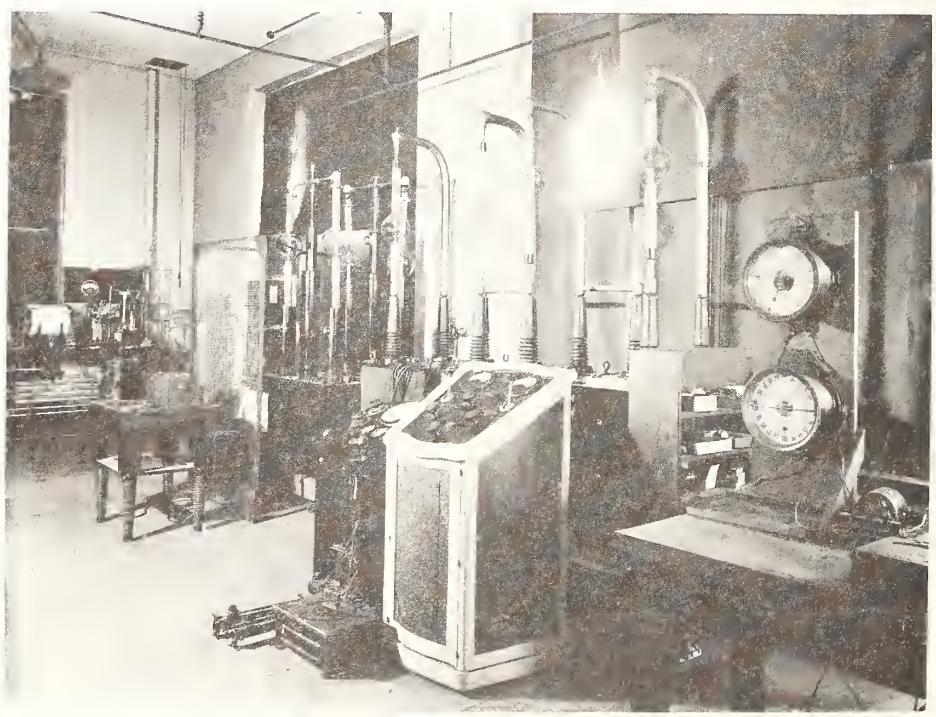

Photo No. 30. Two commercial, valve rectified, high-voltage x-ray generators under study (1932).

Photo No. 31. Test setup for study and comparison of x-ray tubes and generators (1932).

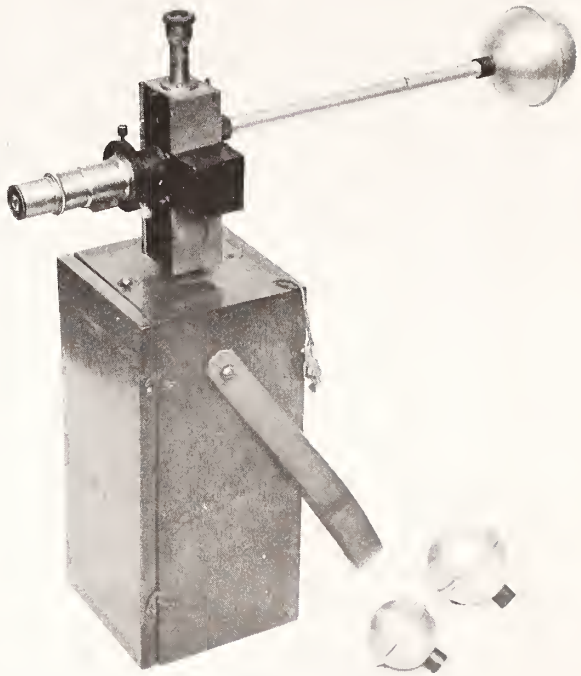

Photo No. 31a. Portable radiation survey meter, one of the first developed (1929).

CHAPTER 9. CERTIFICATION OF X-RAY PHYSICISTS (1934-1942)

In December 1934, the Standardization Committee held its regular meeting in Memphis, Tennessee at the time of the RSNA annual meeting. This was an unusually important meeting, and had a great impact on shaping the program at the Bureau of Standards for the next decade.

Special attention was directed to the extension of the x-ray measurement program into the higher voltage region. The Committee made a separate recommendation urging the Bureau of Standards to extend its standardization facility to at least 600 kV. The preparation of a standard treatment specification chart was also recommended, and while progress was made, this effort was never clearly carried to the point of complete acceptance by the radiological profession.

A landmark decision by the Committee was to establish a procedure for certifying the capability of radiological physicists to work with clinical applications of radiation. Following this proposal, the chairman made a motion for its acceptance by the Radiological Society at the next business meeting. This marked the first such effort to assure that the physics of radiology was being managed by well-qualified individuals. A little over a decade later, a physicist registry was established and managed by the American Board of Radiology. Following is the draft report by the Standardization Committee covering its 1934 meeting.

REPORT OF COMMITTEE ON STANDARDIZATION OF X-RAY MEASUREMENTS, MEMPHIS
December 1934
Draft

1. A general program of study for the next three years was agreed upon. The importance of a continued program lies in preparing ourselves for the 5th International Congress in 1937. Definite questions were left unsettled by the International Units Committee in 1934. While these will be studied by their subcommittee, it is important that the American views be well organized.

Accordingly, the points for our future consideration will be (1) the measurement of low-voltage x rays, high-voltage x rays and gamma rays in comparable terms; (2) the rewording of the definition of the roentgen to introduce greater simplicity and any new requirements necessitated by the measurement of gamma rays; (3) the clinical measurement of high-voltage x rays, with particular reference to existing type of dosage meter. Since the above points are essentially of a physical nature it was agreed to leave their study entirely in the hands of the physicists on the Committee until the time when completed devices are ready for clinical use by the physician.

2. For several years the Committee has discussed the preparation of a standard treatment specification chart. Its recommendations on this subject have been published as parts of the general reports, but not in a form suitable for general clinical use. Such a chart should, in addition to the necessary treatment factors, be accompanied by general directions for making the necessary measurements. When the report is adopted by the Committee, the Editor of Radiology should insist upon its uniform use in publications. (Discussion with some members of the Publication Committee indicated that they would accept our recommendations.) Preparation of the chart draft is now in the hands of Drs. Ernst and Portmann.

3. It was suggested that a similar treatment specification chart be prepared for the use of radium and radon but it was agreed that this should be delayed at least until the x-ray chart has been completed and tried out.

4. The mode of measuring and expressing a dose was again discussed and the Committee's earlier agreements were upheld and emphasized. All dosage measurements are to be made with the thimble chamber in free air. Measurements including scattered radiation may be given if desired, but only if accompanied by the proper free air measurement. The two shall be carefully distinguished. Attention was called to a motion presented before the American Roentgen Ray Society along similar lines.

5. It was recommended that the use of thimble chambers attached to the inner surface of compression cones be discouraged since their indications may be ambiguous when used for dosage measurement. Attention was directed to the use of indicating chambers built into the cone of shock-proof tube holders, pointing out that when placed near the tube, serious error may result from the large temperature changes usually undergone.

6. Attention was also directed to the wavelength dependence of thimble chambers. Most manufacturers exaggerate the range and degree of wavelength independence, frequently misleading the doctor into making false measurements. This applies particularly above 200 kV and to radium. It was suggested that the manufacturers be asked to be more exact in their statements and not make any specifications other than for the actual conditions and range of calibration. It may be pointed out that the National Laboratories of several countries have agreed that a tolerance of $\pm$1.5% shall constitute wavelength independence within a stated quality range.

7. There was a lengthy discussion on the subject of the calibration of x-ray machines in situ. There has been much abuse along such lines, particularly by people unqualified to make such calibrations. There has been open criticism of the physicist by the physician for frequently offering advice regarding the utilization of radiation in treatment, even to the extent of suggesting proper dosages. Complaint has also been made against manufacturers' agents where it has been alleged that improper calibrations have been given for sales purposes. It was decided, therefore, to set up a "Registry of X-Ray Physicists", listing physicists suitably qualified to make calibrations of x-ray machines. It is felt that this will offer protection both to the physician and physicist. It was agreed that a "Registered X-Ray Physicist" should meet the following requirements:

(1) Be a recognized physicist.

(2) Show a reasonable working knowledge of physics in the radiological field.

(3) Be familiar with the classical x-ray theory.

(4) Appear before a selected board for examination if deemed advisable by that board.

(5) Use only such dosage instruments as are approved by the Committee and tested by a recognized testing laboratory. (For example, Bureau of Standards, Cleveland Clinic, Memorial Hospital, Canadian National Research Council, Temple University Hospital.)

(6) Agree not to give out any medical information.

(7) Not be directly employed by any x-ray equipment manufacturer, agent, or distributor.

Under (4) the following board was selected: R. R. Newell, Otto Glasser, G. Failla, Chairman of the Committee (ex officio).

Any member of the board may examine an applicant, submitting a report and recommendations to the other members of the board for final action.

It was agreed by the Committee to immediately certify the following: C. B. Braestrup, Otto Glasser, R. Landauer, L. D. Marinelli, A. Mutscheller, P. A. MacDonald, A. Nurnberger, C. T. Ulrey, and J. L. Weatherwax, G. Henny.

In this connection the following motion was offered before the Radiological Society of North America in executive session, and adopted by the executive committee:

"I move that the Standardization Committee of this Society be authorized to set up a list of approved individuals who may give certificates of calibration of any type of x-ray equipment. This applies only to non-medically trained men."

It was tacitly assumed that any medically trained (radiologist?) person was competent for such work. At least the RSNA was not then proposing that its members be subjected to examination by physicists.

In communication #7 in September 1935, comments on the report of the 1934 meeting were circulated to the members. Since there were many comments indicative of the problems of that period, it is of interest to include them all.

COMMENTS ON STANDARDIZATION COMMITTEE REPORT OF DECEMBER 1934

Newell - Suggested including medical men in the resolution passed by the society. I believe, however, that any endeavor to include medical men in an action by an essentially physical committee would be keenly resented. My reply to him was as follows:

"I will try to make partial replies to your several letters of April 1st. - Your suggestion regarding the inclusion of medically trained men in the executive committee motion was considered. However, this particular resolution was started largely as a result of criticisms by the medical men against the physicists and it was felt as a result of this, that there might be some resentment of a physical committee telling a medical man what he could or could not do. The feeling was that what medical men did was a matter up to a medical examining board and not a physical board."

Failla - Under Par. 2 says "In this connection, charts prepared by the American College of Surgeons should be consistent. After the best type has been worked out, the College might be induced to adopt it also for the sake of uniformity", also under Par. 7, (6) question what is meant by "Medical Information".

Laurence - For second sentence under Par. 4, suggests "Descriptions of dosage are to include in all cases a statement of the quantity of radiation in roentgens measured in free air with a thimble chamber". This would avoid any implication that free air measurement is a complete description of the physical treatment. Suggests publishing the report in its present form.

Erskine - Under paragraph 2, "I believe we should recommend that the Editor of Radiology should insist upon a uniform nomenclature instead of permitting the use of K.V., Kv,m or kv., for example, in the same number. In paragraph 4, I am not quite sure that the "Committee's earlier agreements" are broad and inclusive enough. I think we are hampered by the definition of the unit in our attempt to find some simple, clear, easily understood method of expressing the dose received by the lesion."

Quimby - Add name of G. Henny to list under Par. 7.

Meyer - "For example, I do not believe that any opinions should be expressed or decisions made with regard to physical dose measurements without first being submitted to experienced clinical Radiologists, since my experience clearly shows that very often the physicist's view point is without consideration of the clinical aspect. In other words, what may be essential requirements of the physicist's laboratory may not be so important to the practical radiotherapist.

1. - "I frankly state that I am in disagreement with what I understand to be the recommended method of quality determination evidently originating in your laboratory in Washington and supported by some of the members of the committee.

2. - "I must say that I am anxiously awaiting the submission of a chart satisfactorily adaptable to the various methods of radiation therapy now in use. I have as yet seen none that I consider satisfactory.

3. - "There is no doubt that bedlam at present exists with regard to the specification of dosage with radium and radon, with proper designation more essential here than with respect to the x rays.

"The giving of advice by any lay individual, manufacturer's agents or physicists in matters, diagnostic or therapeutic, is not alone to be discouraged but condemned. This applies not alone to manufacturers of x-ray equipment, but also to certain of the radium and emanation plants that offer advice and instruction as well as the where-with-all to carry on treatments with their particular products wherein individuals with little or no experience at all proceed with dangerous diagnostic and therapeutic applications. Is this not close to, or an actual infraction of the Medical Practice Act?"

Glasser - Suggested not publishing Par. 5, 6, but taking care of such matter by correspondence. (The committee would not publish anything disparaging to the manufacturers, who on the whole, have been doing a good job, L.S.T.)

Asks why Failla, Taylor and others were not included under Par. 7. (This list was based on the assumption that those listed did consulting test work outside their own laboratories. Failla and Taylor do not - Glasser's name to be omitted also.)

Gendreau - Under Par. 7 suggests adding name of Mr. Joseph Demers.

Weatherwax - "I am heartily in favor of the paragraph but feel that you should go into greater detail in the third sentence. 'There has been open criticism of the physicist by the physician for frequently offering advice regarding the utilization of radiation in treatment, even to the extent of suggesting proper dosages.' I think this sentence should be explained in greater detail. If there is one thing that is generally needed today it is that there is a great need for better cooperation between the physicist and the radiologist. Both can learn a lot. One of the most difficult jobs I have to do is to fight shy of giving suggestions when I go out on calibration.

"I think the physicist can give the average radiologist much information upon standard optimum doses or toleration doses, threshold doses, and help work out an organization to deliver an optimum tumor dose with as little damage to the skin and underlying tissue as possible by arranging the number and direction of the fields of radiation. The well organized centers have little use for such knowledge but the radiologist in the small communities need help and will need it for a long time to come. These small communities treat the bulk of the cases, taken as a whole all over the country. If they are to be denied the physicist's help, then to my mind the progress of radiology is to be retarded.

"Rather than to set down too harsh rules governing the cooperation of the physicist and the radiologist, it might be well to give a few rules of diplomacy to physicists. I am not aware of ever forcing or attempting to force my ideas on radiologists and I think I have influenced many radiologists. I think the many visitors that we have to our department indicate that many radiologists want the cooperation of the physicist.

"I have had charge of the organization in our Hospital for the last eight years and in that time I have never given a radium or x-ray treatment although I have had a lot to do with dosage in general, and I think the physicist should.

"Let us for example, take a radiologist who has purchased his first deep therapy x-ray machine. Let us say that this radiologist has been using superficial therapy for quite a few years. Should these radiologists be allowed to blunder along over a lot of ground that we have already covered, or can we give this radiologist, who is hungry for knowledge, some standard technics worked out by well organized therapy centers, telling him these are safe technics and as soon as he has gotten experience that he should vary these technics to suit the individual patient under treatment. There is no organization at present to meet this need. There is usually considerable jealousy existing between contemporary radiologists so that they do not get so much information from each other. The large centers are usually too busy to do much teaching, so the radiologist is left to shift for himself. I have radiologists writing me and referring to the help that I have given them four, five, six and seven years ago. I refer to the work that is being done at the Memorial Hospital, our Hospital, foreign laboratories saying at the same time that these dosage technics are to be eventually changed for the particular patient under treatment. I am not saying this to be egotistical about what I have done but to bring out the need for information as to safe technics. The radiologist serving small communities and hospitals with limited equipment serves a greater population than the radiation centers. Many of these radiologists fear to go ahead and give radiation in sufficient quantity to produce results obtainable in the radiation centers with the result that their work stands still; since they do not give enough radiation they do not get results and because they do not get results, the surgeon will not have any faith in radiation therapy. It therefore becomes a vicious circle."

The general reaction toward this report seemed to be favorable toward publishing it in a somewhat modified form (as specifically stated in a few cases).

The comments given above, while very important, are for the most part very general and it is difficult to incorporate many of the ideas in a formal report. I wish the individuals would put their ideas into definite form and show just where to insert them in the report. It is virtually impossible for me to do so.

Also your further comments on the above extracts are solicited.

Since the last meeting I have received from Dr. Portmann copies of their standard treatment chart. It seems to me that this forms a very excellent basis upon which to

begin our discussions on this matter. Accordingly I am inclosing a copy with the explanation of the headings. Will you please send your discussion of this chart directly to Dr. Portmann.

Of the small cadre of radiological physicists in the mid-1930's, J. L. Weatherwax was among those with the longest experience in clinical operations, including widespread contacts with radiologists outside of the larger installations.

His comments on paragraph 7 of the above report were strongly countered by the views of Dr. William Meyer, a radiologist. These conflicts of opinion were gradually resolved to the satisfaction of both the physician and the physicist.

The final report of the Committee, published over a year later, included a number of items not covered in the draft. Because its recommendations would influence the RSNA and NBS for some years to come, it is included here in full (RSNA, 1936):

[Reprinted from RADIOLOGY, Vol. 26, No. 5, Pages 634–637, May, 1936.]

REPORT OF COMMITTEE ON STANDARDIZATION OF X-RAY MEASUREMENTS

I. PROGRAM

At the 1934 meeting of this Committee, a general program of study for the next three years was agreed upon. Definite questions have been left open by the International Committee on Radiological Units. While these will be studied by their executive committee, it is important that each individual country be adequately prepared to present its needs for consideration.

Accordingly, the points for our future consideration will be:

1. The measurement of low voltage x-rays, high voltage x-rays, and gamma rays in comparable terms;
2. The re-wording of the definition of the roentgen to introduce greater simplicity and any new requirements necessitated by the measurement of the gamma rays (*cf.* our report, RADIOLOGY, March, 1934, **22**, 289);
3. The clinical measurement of high voltage x-rays with particular reference to existing types of dosage meter;
4. The establishment of a standard treatment technic recording chart.

Since the first three points are essentially of a physical nature, it was agreed to leave their study entirely in the hands of the physicists on the committee until the time when completed devices are ready for clinical use by the physician.

II. TREATMENT CHART

For several years the Committee has discussed the preparation of a standard treatment specification chart. Its recommendations on this subject have been published as parts for the general reports but not in a form suitable for general clinical use. Such a chart should, in addition to the necessary treatment factors, be accompanied by general directions for making the necessary measurements.

When the report is adopted by the Committee, the Editor of RADIOLOGY should insist upon its uniform use in publications. Preparation of the chart draft is now in the hands of a subcommittee and will probably be agreed upon within a few months.

It was suggested that a similar treatment specification chart be prepared for the use of radium and radon but it was agreed that this should be delayed at least until the x-ray chart has been completed and tried out.

III. MEASUREMENT OF DOSAGE

The mode of measuring and expressing a dose was again discussed and the Committee's earlier agreements were upheld and emphasized. *The descriptions of dosage are to include in all cases a statement of the quantity of the radiation in roentgens measured in free air with a thimble chamber.*

Measurements including scattered radiation may be given if desired, but only if accompanied by the proper free air measurement. The two shall be carefully distinguished. Attention is called to a motion presented before the American Roentgen Ray Society along similar lines, about two years ago.

It is recommended that the use of thimble chambers attached to the inner surface of compression cones be discouraged since their indications may be ambiguous when used for dosage measurement. Attention is directed to the use of indicating chambers built into the cone of shockproof tube holders. If such chambers are placed too near the tube, serious error may result from the large temperature changes usually undergone.

IV. FILTER AND ABSORPTION DATA

The quality of x-radiation shall be expressed in terms of the complete absorption curve in a suitable metal. The absorption curve may be described in terms of (1) the equivalent voltage (constant potential necessary to yield the curve), or (2) the half value layer. The forefilter must always be given.

Effective wave length and average wave length designations shall not be used.

It should be emphasized that any quality determinations made through absorption measurements should be directly related to a full absorption curve and, hence, constitute a mode of describing the curve or some property of the curve. Depending upon the method employed, the full absorption curve is more or less nearly described. The "equivalent voltage" method gives all points on the curve, and the "half value layer" method gives two points, which is frequently sufficient. The "second half value layer" method gives three points, but necessitates obtaining enough data to set up the full absorption curve in any case.

The following filter combinations are recommended:

Kv. (approx.)	*Forefilter*	*Absorbing Filter (for quality)*
30–120	Aluminum	Aluminum
100–250	Copper	Copper
200–400	Tin or copper	Copper[1]
400–up	Lead	Lead

V. SECONDARY STANDARDIZATION LABORATORIES

In response to repeated requests, the National Bureau of Standards has set up the following general requirements for secondary x-ray standardization laboratories:

"General Requirements for X-ray Standardizing Laboratories

"1. Equipment must be in charge of a full-time physicist, whose special field is that of x-rays and whose basic training has been in physics.

"2. The standardizing laboratory must be equipped with the necessary working standards for effecting a complete calibration of the current measuring system.

"3. Standardizing equipment must be maintained in working order and position and not be disassembled except for purposes of normal maintenance.

"4. Basic parts of the standardization equipment must be compared against that of the National Bureau of Standards (or the National Research Council of Canada, if in Canada).

"5. Standardization equipment must be inspected *in situ* by a physicist selected by the N. B. S. or the N. R. C. C.

"6. Standardization equipment must be in accord with 'X-ray Standards and Units Standardizing Procedure of the National Laboratories' (Am. Jour. Roentgenol. and Rad. Ther., 1934, **31**, 815).

"7. Laboratory must file with the N. B. S. drawings showing all essential details of ionization chamber, tube container, and diaphragm system. Any subsequent changes must be filed with the originals.

"8. Calibration reports must conform with those of the N. B. S. and bear suitable serial numbers; copies of all reports to be sent to a central file at the N. B. S. at least every three months."

These requirements have been ratified by the several laboratories in this country which are already doing such work.

[1] Even though tin is slightly more discriminating, the difference is not enough to be of great importance, and it is desirable to keep the list of absorption materials small.

VI. REGISTRATION OF X-RAY PHYSICISTS

The Committee has discussed in detail the question of the calibration of x-ray machines in hospitals. In this connection our attention has been directed to numerous abuses along such lines by persons unqualified to make such calibrations. It was decided, therefore, to set up a Registry of X-ray Physicists, listing physicists who, in the Committee's opinion, are suitably qualified for consultation and to make calibrations of x-ray machines. It is felt that this will offer protection both to the physician and physicist. It was agreed that a Registered X-ray Physicist should meet the following requirements:

1. Be a recognized physicist working in the radiological field;
2. Show a reasonable working knowledge of physics in the radiological field;
3. Be familiar with the classical x-ray theory;
4. Appear before a board selected by the Standardization Committee for examination if deemed advisable by that Board.
5. Use only such dosage instruments as are approved by the Committee and tested by a recognized testing laboratory. (For example, National Bureau of Standards, Cleveland Clinic, Memorial Hospital, National Research Council of Canada, or Temple University Hospital.)
6. Not be directly employed by any x-ray equipment manufacturer, agent, or distributor.

In this connection, the following motion was offered before the Radiological Society of North America in executive session at the 1934 Memphis meeting:

"I move that the Standardization Committee of this Society be authorized to set up a list of approved individuals who may give certificates of calibration of any type of x-ray equipment. This applies only to non-medically trained men."

This resolution was subsequently adopted by ballot, after the meeting.

In accordance with requirement (4) the Committee has appointed, for an indefinite period, the following examining Board:

R. R. Newell,	G. Failla,
Otto Glasser,	L. S. Taylor

Any member of the Board may examine an applicant, submitting a report and recommendations to the other members of the Board for final action. All papers relating to an appli-

cant will be filed at the National Bureau of Standards.

In connection with the resolutions given above, the Standardization Committee of the Radiological Society of North America agrees that the following physicists meet the requirements of the Examining Board:

C. B. Braestrup,
Otto Glasser (1, 2, 3),
Robert Landauer,
L. D. Marinelli,
A. Mutscheller,
G. Failla (1, 2, 3),
E. Quimby (1, 2, 3),
P. A. MacDonald,
A. Nurnberger,
C. T. Ulrey (1),
J. L. Weatherwax (1, 2),
C. C. Lauritsen (1),
L. S. Taylor (1, 2, 3),
G. Singer (1),
G. C. Laurence (1, 2),
W. Stenstrom (1, 2, 3),
G. Henny (1),
J. K. Robertson,
K. S. Cole (1),
F. M. Exner (1).

It was also suggested that all such physicists must be members of some recognized radiological or physical society. This is for the partial purpose of evidencing their continued interest in radiological work. Numbers following the above names refer to society membership, *i.e.*: (1) Physical Society; (2) Radiological Society of North America; (3) American Roentgen Ray Society.

It should be emphasized that even more important than actual society membership is the frequent attendance of meetings.

It was further suggested that any physician offering consultation on physical measurements should submit to the same authority that is certifying a physicist offering the same consultation.

VII. MISCELLANEOUS

Last June, an informal meeting of radiation physicists was held at Minneapolis for the purpose of discussing super-voltage x-ray problems. It was proposed at that time that this Committee be the official outlet for the results obtained as a consequence of that meeting. A report will be published.

In response to numerous suggestions that this Committee study certain standardization problems in the field of diagnostics, it will prepare a questionnaire seeking the specific problems upon which standardization is desirable and feasible.

A sub-committee is studying the question of uniform definitions, abbreviations, and symbols of physical terms used in the radiological field. In the meantime, it endorses and recommends the use (1) of the definitions prepared by the American Standards Association (Project C-42), "Definitions of Electrical Terms" (2), "Abbreviations for Scientific and Engineering Terms," O.S.A. Standard ZIOi—1932, and (3) "Metric Abbreviations," Abridged Style Manual, Government Printing Office, 1933, pp. 60, 61, under Rules 79 and 79*a*.

For the Committee:

LAURISTON S. TAYLOR, *Chairman*,
U. V. PORTMANN, M.D., *Sub-chairman*

February, 1936

Registry of X-Ray Physicists

After the establishment of Examining Boards for the Standardization Committee, examination and certification of applicants proceeded in a reasonably normal fashion. Details varied from applicant to applicant but, on the whole, there were no difficulties and the program was received very well.

To insure uniformity in applications, a format was developed early in the program and used until the program was taken over by the American Board of Radiology. Following is a sample:

FOR SECRETARY'S USE ONLY --

Rec'd	: Training	: Action
No.	: Yrs. in Rad.	: Certifi. sent
Age	: Examined	:

Type or print all information

THE RADIOLOGICAL SOCIETY OF NORTH AMERICA
and
THE AMERICAN ROENTGEN RAY SOCIETY

Application for Examination

I hereby make application to The Examining Board of the Standardization Committee of the above societies for the issuance to me of a certificate of Qualification as a registered X-ray Physicist, and for examination relative thereto, all in accordance with and subject to its rules and regulations. I agree to disqualification from examination or for issuance of a Certificate of Qualification in the event that any of the statements hereinafter made by me are false or in the event that any of the rules governing such examinations are violated by me. I agree to hold the above societies, its members, examiners, officers and agents free from any damage or complaint by reason of any action they or any of them may take in connection with this application, the examination, the grade or grades given with respect to any examination, or for the failure of said cooperation to issue me such certificates.

1. Name _____ Date _____
2. Office Address _____
   Street City State
3. Date and place of birth _____
4. Education:
   Secondary School _____ years ____ to ____ incl.
   College _____ years ____ to ____ incl.
   Academic Degrees and years when obtained _____
5. Graduate Education:
   School _____ years ____ to ____ incl.
   School _____ years ____ to ____ incl.
   Degree _____ School _____ Year _____
6. Special training in Radiology, giving dates and Physicists under whose direction work was done. _____

. . . . . .

Other experience in Radiology (military, government, research, etc.

_____

7. Hospital or University staff appointments (past and present giving dates and titles:
   _____ exact dates ____ to ____ incl.

. . . . . .

8. Teaching appointments (past and present):
   Title _____ school _____ years ____ to ____ incl.

. . . . . .

9. How long have you worked with X-rays and/or Radium? _____
   (Number of years)
10. Do you limit your work within the field of Radiology? _____
    _____
    (approximate percentage)
11. Describe in general the roentgen-ray equipment at your command: ___

. . . . . .

12. Describe in general the dosage measuring equipment at your command:

13. State amount of radium at your command:_____
14. Medical and Physical societies in which membership is held:_____

15. Offices (past and present) held in above societies:_____

16. Names of two well-known physicists from whom information regarding the applicant may be obtained:_____

17. Scientific papers, books published (attach extra sheet if necessary):

TITLE	JOURNAL
_____	_____

18. Additional data and notes of explanation:

Signature_____ Address_____
(personal signature)

On October 28, 1935, General Communication #8 covered two important areas. The first was a proposed extension of the list of radiological physicists who might be certified without examination. The second was a set of eight general requirements proposed by the Bureau of Standards that should be met by x-ray standardizing laboratories. Following is a copy of communication #8:

General Communication No. 8 to Members of Standardization Committee 10-28-35
Subject: 1935 Report

(1) Sufficient replies have been received from communications Nos. 6 and 7 to clear up all major points of discussion and permit the early publication of a general report. This is now being prepared.

(2) The only outstanding question concerns the types of individual to be included under the "Registered Radiation Physicist" code. Should we endeavor to register every physicist working in the field of Radiology, or only those specifically engaged in calibration and such work? Some opinions favor including all workers; unless I hear to the contrary I shall include in the first list the following:

C. B. Braestrup	L. S. Taylor
Otto Glasser	G. Singer
Robert Landauer	G. C. Laurence
L. D. Marinelli	W. Stenstrom
A. Mutscheller	G. Henny
G. Failla	J. D. Leitch
E. Quimby	G. H. Henderson
P. A. MacDonald	J. K. Robertson
A. Nurnberger	E. L. Harrington
C. J. Ulrey	J. Demers
J. L. Weatherwax	M. M. Swarzschild
C. C. Lauritsen	K. S. Cole

I would suggest that all such physicists must be a member of some recognized radiological or physical society. This is for the partial purpose of evidencing their continued interest in radiological work.

It was also suggested by Newell that the following clause be added: "Any physician offering consultation on physical measurements should submit to the same authority that is certifying a physicist offering the same consultation."

Regarding the giving out of medical information by Registered Physicists it has been proposed that we delete Par. 6, since its nature seems to be so controversial, as indicated by Weatherwax and Meyer.

The above statements will be included in the report unless I am notified to the contrary.

(3) In response to repeated requests the National Bureau of Standards has set up the following general requirements for secondary x-ray standardization laboratories.

/S/ L.S.T.

The NBS requirements are given in full in the above report of the Committee's 1934 meeting (see p. 141), and subsequently in the report of the 1936 meeting given below (RSNA, 1937).

In July 1936, additional names were proposed for the Registry of X-ray Physicists as follows:

7/28/36 A. Mutscheller, N.Y.; L. B. Leppard, Dept. of Health, Ottawa; S. S. Sidu, University of Pittsburgh, Pa.; J. E. Morgan, Duke University; and B. R. Stephenson, Buffalo City Hospital.

Fifteen additional names were added to the Registry in December 1936, as announced by the SC/RSNA (RSNA, 1937). The report, including this and other items, follows:

Radiology 29, 237, 1937

COMMUNICATIONS

STANDARDIZATION COMMITTEE
December, 1936

I. Registry of Physicists.—Applications and examination results of 15 physicists were received by the Board and submitted to the Committee for approval. The following are certified as having met the requirements of the Board of Registry of X-ray Physicists (for previous certifications, see RADIOLOGY, May, 1936, **26**, 634):

M. M. D. Williams, Mayo Clinic, Rochester, Minnesota.
J. D. Leitch, Dept. of Health, Ontario, Canada
G. H. Henderson, Dalhousie University, Halifax, Nova Scotia.
E. L. Harrington, University of Saskatschewan, Saskatoon, Canada.
G. M. Shrum, University of British Columbia, Vancouver, Canada.
J. Demers, University of Montreal, Montreal, Canada.
M. M. Schwarzschild, Beth Israel Hospital, New York City.
J. E. Rose, Swedish Hospital, Seattle, Washington.
I. H. Blatz, Dept. of Hospitals, 414 E. 26th Street, New York City.
A. Omberg, 1273 Carr Avenue, Memphis, Tenn.
R. E. Pugh, 1944 Summit Avenue, Pasadena, Calif.
Irwin Vigness, University of Minnesota, Minneapolis, Minn.
L. Jacobson, Montefiore Hospital, New York City.
L. Rovner, Chicago, Ill.
M. C. Reinhard, State Institute for Study of Malignant Disease, Buffalo, New York.

The question was raised as to how sharp a line should be drawn between industrially employed physicists who are ineligible for certification and those physicists employed in strictly clinical, standardization, or consultative capacity. "Manufacturer," in the sense used by the Committee, means any person, group of persons, or company which is directly interested in the manufacture or sale of any article or equipment to the radiologist. The Committee desires, however, to place on record that it recognizes the high quality of the physicists employed by the various manufacturers and does not mean to imply in any sense

that they do not possess the same qualifications as a registered physicist. One of the principal purposes of the Registry is to establish a list of physicists available for consultative purposes without being hampered by any possible industrial affiliations.

It was agreed that any registered physicist who, subsequent to his certification, becomes affiliated with a manufacturer, shall automatically and without notice be dropped from the certified list. If such a physicist closes such an affiliation, a new application for certification may be submitted to the Board.

II. Certified X-ray Standardization Laboratories.—In the interests of radiology it is agreed by the Committee that all dosage meters in clinical use shall be calibrated and certified by a recognized x-ray standardization laboratory, meeting the following requirements:

1. Equipment must be in charge of a full-time physicist, whose special field is that of x-rays and whose basic training has been in physics.
2. The standardizing laboratory must be equipped with the necessary working standards for effecting a complete calibration of the current measuring system.
3. Standardizing equipment must be maintained in working order and position and not be disassembled except for purposes of normal maintenance.
4. Basic parts of the standardization equipment must be compared against that of the National Bureau of Standards (or the National Research Council of Canada, if in Canada).
5. Standardization equipment must be inspected *in situ* by a physicist selected by the N. B. S. or the N. R. C. C.
6. Standardization equipment must be in accord with "X-ray Standards and Units Standardizing Procedure of the National Laboratories" (Am. Jour. Roentgenol. and Rad. Ther., 1934, 31, 815).
7. Laboratory must file with the N. B. S. drawings showing all essential details of ionization chamber, tube container, and diaphragm system. Any subsequent changes must be filed with the originals.

In this connection, it should be noted that the physicist in charge of such standardization laboratories need not necessarily be a Registered Physicist, but that in any case his general qualifications must meet at least as high a standard thereas.

The following laboratories have satisfied the necessary requirements under the above classification:

1. National Bureau of Standards, Washington, D. C.
   Physicists: Lauriston S. Taylor, George Singer.
2. National Research Council of Canada, Ottawa, Canada.
   Physicist: G. C. Laurence.
3. Cleveland Clinic, Cleveland, Ohio.
   Physicist: Otto Glasser.
4. Memorial Hospital, New York City.
   Physicists: G. Failla, Edith Quimby, Leo Marinelli.
5. Temple University, Philadelphia, Pa.
   Physicist: George Henny.
6. Victoreen Instrument Company, Cleveland, Ohio.
   Physicists: John A. Victoreen, Lawrence Frazer.

The above laboratories are certified to make calibrations for x-rays generated by potentials up to 200 kv. (peak). There are at date of writing, no laboratories certified to make calibrations for x-rays generated by potentials in excess of 200 kv. (peak). It is hoped that the National Bureau of Standards may be in such a position within a short time.

III. Sub-committee on Treatment Data Recording Charts (U. V. Portmann, Chairman, E. C. Ernst, Edith Quimby).—Final agreement was reached on a standard form for the recording of all treatment data. This will be published separately at an early date. With this will be published a full description of the entries with necessary details and precautions in making any measurements and calculations involved.

In addition, it is recommended that the radiologist in charge of an x-ray department maintain a day-book for recording all changes in tubes and apparatus, calibration changes, and calibration reports. It is suggested that all individual treatment records be initialed by technician and radiologist on such factors as filter, target-skin distance, and dose administered. This is particularly important for legal purposes.

In connection with the calibration of clinical x-ray installations, the importance of the field size used for the measurements was stressed. Particularly in the case of oil-immersed tubes and super-voltage tubes, it is

found that the dosage rate tends to increase with increase in field size. This is due principally to scattering from the large volume of oil. A sub-committee under Dr. Otto Glasser was appointed to prepare a report on this and related questions.

IV. Sub-committee on Units, Definitions, and Nomenclature (R. R. Newell, Chairman).—A full report was deferred until the next meeting of the Committee. In the meantime, it was suggested that the Journal's Publication Committee refer any questions relating to units to Dr. Newell, who is representative of the Radiological Society on the Radiological Units Committee of the A.I.E.E.

V. Sub-committee on Diagnostic Standardization.—A detailed report was prepared by Dr. G. Henny, Chairman, but action relating thereto was deferred until the next meeting, before which time members may have a better opportunity to study his report.

VI. Measurement of Super-voltage X-rays and Gamma Rays.—The Committee feels that adequate measuring standards for super-voltage x-rays and gamma rays are non-existent at this time. Work by Jaeger, Mayneord, Friedrich, Failla, Gray, and others is recognized as having great importance but not yet carried to the point where definite standards may be safely established. It seems evident that free air measurements will be impracticable for gamma rays.

Choice of the type of standard ionization chamber for super-voltage x-rays should be very definitely left open for the present. Thimble chambers can undoubtedly be used for clinical purposes, although the ultimate correct design is open to question. Work by Glasser and by Victoreen up to 400 kv. appears to contradict other equally reliable measurements of the so-called "saturation wall thickness." Four or five mm. of bakelite or carbon wall thickness may be unnecessary and actually undesirable at lower voltages. They do not find strong evidence for the flat-topped wall-thickness curve. Hence, at least for super-voltages up to 400 kv., we may reasonably question some of the similar earlier work.

Open air ionization chamber measurements have been made up to 800 kv. Mayneord showed a definite divergence at about 250 kv., but this was due to use of too small a parallel plate chamber. Up to the point where his chamber size became the limitation, his measurements showed good agreement between open air and thimble ionization. Lauritsen and Jaeger have shown that under suitable laboratory conditions free air chambers may be used for accurate measurements of super-voltage x-rays. Hence, we feel that *thimble chamber standards should not be adopted for x-rays until all the possibilities of free air measurements have been adequately explored.*

VII. Filters for X-ray Treatments.—The following approximate filter combinations are recommended by x-ray treatment work:

Copper + 1 mm. Al;
Tin + 0.25 mm. Cu + 1 mm. Al;
Lead + 1 mm. Sn + 0.25 mm. Cu + 1 mm. Al.

For the last two, it is desirable to have such a combination as to effectively cut off all wave lengths longer than 0.1 Å. In this connection, it is important to stop the passage of large amounts of radiation near the absorption limits of the particular filters used. Further details will be given in Dr. Glasser's later report.

VIII. Quality.—For clinical purposes, a statement of the fore-filter and half value layer gives an adequate quality description (fore-filter includes inherent filtration of tube and container). Use of a second half value layer is unwarranted. For fundamental and more accurate quality descriptions, the full absorption curve (or data) may be given. Only through the latter may other different quality measurements be related.

Quality measurements up to 400 kv. shall be made with copper. It is recognized that while tin is more discriminating, the difference from copper is not important. Moreover, it is desirable to keep down the number of filter materials. Tin, in pure and uniform sheets, is not easily handled and its use may involve uncertain errors if special precautions are not taken. Lead absorption data are probably desirable above 400 kv. but here again copper may possibly be sufficient for practical purposes.

The manufacturers shall be asked to give a statement with each x-ray tube or the inclosure of the exact copper equivalence of the walls, and the voltage for which this applies. On permanent apparatus this may be marked on the apparatus itself.

IX. Miscellaneous.—It is suggested that the manufacturers provide "pre-reading" voltmeters with more open scales and more reliable calibration than is frequently done. Since the technician relies upon this meter so much

for control purposes, it is essential that the instrument be reliable and readable.

Registered physicists, calibrating an x-ray plant, should see that all protection requirements are met, and report any omission to the radiologist in charge. A copy of such a report should be retained.

It is suggested that radiologists or hospitals installing new apparatus choose in advance the physicist who will do the subsequent calibration work. Advance consultation on apparatus, protection, and dosage measurement may be of great assistance to the radiologist and help to avoid expensive later changes.

Attention should be directed to the accurate timing of dosage. Errors in clock controls may be very considerable for short exposures of high dosage rate. The question of timers is being studied by Dr. Weatherwax and Dr. Chamberlain.

It was suggested that advance copies of this report be sent to members of the International Committee on Radiological Units and others interested.

LAURISTON S. TAYLOR, *Chairman*

One of the principal subjects of discussion at the December 1940 meeting of the tandardization Committee involved the Registry. Following are excerpts from notes on the tandardization Committee meeting of December 4, 1940.

NOTES ON MEETING OF STANDARDIZATION COMMITTEE, R.S.N.A.
Cleveland, December 4, 1940

The idea was introduced by Dr. Portmann of having the Board of Radiology take over the operation of, and responsibility for, the registry of x-ray Physicists. This had been suggested to him in informal conversations with various people during the past year.

Objection was raised that such formality might involve expense and therefore necessitate application fees.

It was agreed that if the Board of Radiology were to take over the Registry of Physicists those already registered should in general be carried over without further examination after which the registry would be divided into three classes;
Class A - Existent registered physicists;
Class B - Physicists of possibly good qualifications but still questionable;
Class C - New Physicists.

Laurence suggested that new applicants be given a written examination and an oral examination by the nearest board member. This suggestion was made in order to minimize the traveling expenses of an applicant and would take away from any individual the onus of having to fail the applicant. Newell made the proposal that the registry be kept in the hands of the present standardization committee and at all costs be kept away from any control by the American Medical Association.

The committee agreed to use a special letterhead in all correspondence relating to the Registry of X-ray Physicists. This would have as a major heading "The Radiological Society" seal, as a subhead, "Examining Board, Registry of X-Ray Physicists, Standardization Committee". The cooperation of the American Roentgen Ray Society is to be sought in this connection with the idea eventually of having a combination letterhead. Dr. Portmann will be sent a letter asking him as a member of the executive committee of the Roentgen Ray Society to secure authorization for such a step.

The following motions were made:

Newell: Moved; That no investigation be made looking to the Board of Radiology taking over any portion of the activity of the Standardization Committee and the Registry of X-ray Physicists. This motion was defeated 5 to 4.

Laurence: Made a motion proposing that all registered physicists be given a written examination which examination is to be prepared by two members of the board. The applicant shall also be given an oral examination by two members of the board or representatives thereof. The written examination is to be prepared by a group including at least one radiologist and the oral examination group shall likewise include at least one radiologist. This proposal was adopted.

Inherent Filtration. This question has been presented to the committee informally by most of the x-ray tube manufacturers who feel that it is a very urgent problem from the commercial point of view (see report prepared by T. H. Rogers).

Newell insists there is no need to talk about inherent filtration and that the manufacturer should simply state their filtration and let the radiologist use the information as he wishes. The following proposal was made by him; that the manufacturer be informed of the committee's belief that for radiographic tubes the proper designation of capacity is not in terms of roentgens but in terms of focal spot size and permissible loading (kv, ma, time) for reasonable tube life. The actual filtering materials and thicknesses built into the tube should also be specified.

Taylor recommended the appointment of Dr. F. O. Coe to this committee as a representative of the American Standard Association. Since Dr. Coe is already a member of the A.R.B.S. committee in similar capacities, his membership on our committee would coordinate any activity in this work. The committee recommended (that this matter) be taken up with the executive committee.

Note: L. S. Taylor prepared a memorandum to the Executive Committee and handed it to Dr. Childs and was informed that he, Dr. Childs, is already a delegate from the R.S.N.A. to the A.S.A., but that he would take the matter up with the executive committee. He suggested the possibility of appointing Dr. Coe to act as his representative with the Standardization Committee.

Discussion of Bulletin No. 1.

1. Any further revisions should include some information on calibration methods to be used with small cavity chambers.

2. Measurement of radiation from intra-cavity tubes are in a very unsatisfactory state at present. It was suggested that special vaginal measurements be made when using such tubes.

3. The cost of the bulletin reprints should be marked thereon.

4. It was recommended that the charts be printed separately, being arranged as loose leaves and be obtainable separately.

Dr. Glasser gave a brief analysis of his preliminary study on report forms submitted by various members of the Registry of X-ray Physicists. A breakdown of this analysis will be presented by him in the near future.

It was decided to divide the membership of the Registry of X-ray Physicists into several different categories and also to keep the list up to date with regard to the activities of its members. Certain members of the list do not do outside calibration work and their names shall be marked with an asterisk which indicates that they are available for consultation only. The following names are dropped from the list:

C. T. Ulrey, I. Vigness, by virtue of their no longer being active in the x-ray field, C. C. Lauritsen is to be placed upon the inactive list temporarily. It was recommended that registered physicists be encouraged to come to the society meetings and to cooperate with the standard committee. It was also urged that the Standardization Committee endeavor to assist the registered physicists any way that it can. The draft of the registered physicists' application form, prepared by Taylor, was accepted by the committee and it was recommended that copies thereof be prepared by mimeograph for future use.

On the question of field uniformity (in accordance with Jacobson's paper), it was recommended that registered physicists make off-axis measurements of the radiation intensity on all new installations where the tube has been charged. It was recommended (with caution) that the radiologists make occasional film checks to look for gross nonuniformity of his field. The film cassettes should be covered with about 1 mm of copper or 14 mm of lead in order to hold the exposure within reasonable limits.

Dr. X's name was again brought before the committee and the objection raised to his practice of prescribing the dosage for radiologists to use in their treatments. This is the second time his name has been presented in this connection. He was reprimanded after the first occurrence several years ago. The committee decided to take no action until a formal complaint was presented to it.

The following members attended the meeting: Dr. U. V. Portmann, Dr. Otto Glasser, Dr. R. R. Newell, Dr. K. W. Stenstrom, Dr. J. E. Weatherwax, Dr. G. C. Laurence, and Dr. George Henny.

Among the members of the Standardization Committee there was general, but not enthusiastic, agreement that the Standardization Committee should investigate the possibility of having the Registry of X-Ray Physicists taken over by the American Board of Radiology. Their feelings were probably influenced by the fact that, as a committee of individuals, they had no real mechanism for operating and financing such an operation, whereas such a mechanism was offered by the American Board of Radiology. The principal opponent to this plan was Dr. Newell, who was so highly regarded that his comments were simply not overridden by a vote. His pursuit of the question is well illustrated by excerpts from correspondence with him in August and September, 1941.

The matter was not of great immediate concern to the Committee members until, without consulting them, Portmann addressed a letter of August 21, 1941 to the secretaries of the several radiological organizations and the AMA, proposing the transfer of the Registry to the American Board of Radiology (ABR). Excerpts from the ensuing correspondence are given below. (Such copies of the available correspondence were obtained from the Secretariat of the RSNA, rather than the original files of the Standardization Committees.)

Replies to the proposal varied. For example, Dr. Peirce, Secretary of the ARRS wrote,

> "I see no objection to such qualifications being done by the ABR but, on the other hand, do not see that the present arrangement under the Committee on Safety and Standards, ARRS or the Standardization Committee, RSNA demands change."

Newell wrote that he could see no reason for asking the American Medical Association or the American Board of Radiology, which the AMA was sponsoring, to take over this work.

Portmann's response (9-10-41) to Newell was not unexpected. He pointed out the fact, well known at the time, that the primary objectives of the American College of Radiology (ACR) were to "foster and improve the economic status in educational faculties of radiologists." He expressed further doubts that the ACR would be interested in other projects, such as standardization of apparatus or the examination and certification of physicists. Portions of the correspondence follow.

Portmann to Childs, Secretary, Radiological Society of North America — August 21, 1941

A few years ago the Standardization Committee of the Radiological Society of North America instituted a registry for radiological physicists also sponsored by the Committee on Safety and Standards of the American Roentgen Ray Society. This registry is composed of physicists who have been examined and certified as competent by others in their vicinity.

It occurred to me that since all radiologists and their Society are interested in the competence of physicists, and since radiologists subject themselves to examinations by a board delegated by the different Societies, that it should be the function of the American Board of Radiology to also examine radiological physicists and certify those found competent. The matter was offered for informal discussion to a group of registered physicists most of whom favored the idea and asked me to present it to proper authorities for consideration.

The Trustees of the American Board of Radiology are representatives delegated by the Section on Radiology of the American Medical Association, the American Roentgen Ray Society, the Radiological Society of North America, the American College of Radiology, and the America Radium Society. The Board cannot take action upon such a matter without instructions and authorization from the societies which it represents. Therefore, I am writing to the secretary of each society asking him to present this letter to their executive committee, then if they believe that it would be advantageous for their constituted authority to examine radiological physicists and to certify those found competent, to instruct and authorize their representatives in the American Board of Radiology to investigate the feasibility of the plan and take whatever action they may deem advisable.

(Newell to Portmann) — August 27, 1941

I have read with interest your circular letter to the secretaries of all radiological societies, concerning the taking over of the certification of physicists by the American Board of Radiology.

I see this difference between radiological certification and physicists certification:

Assurance that a specialist in any branch of medicine is well trained concerns, of course, primarily the rest of the medical profession who are going to refer patients to him. But it also concerns every patient who will be glad to note on the wall of the doctor's office a certification that he is an accepted specialist. Such certification, therefore, is wisely sponsored by the American Medical Association.

The physicist who is going to standardize the radiologist's apparatus, however, is employed by the radiologist and is not seen by any of the radiologist's patients. His certificate is not seen by any of the patients, nor is his certificate seen by any of the physicians who refer patients to that radiologist. The certification and its importance, and the hiring or not hiring of a given physicist is entirely a matter within the radiological profession itself, and does not spread beyond the radiological profession into the medical profession generally.

As I look at these facts, I am inclined to say: Leave the certification of physicists where it is, namely in the hands of the Radiological Society. Or at the widest ask the American College of Radiology to undertake the certification. I see no reason for asking the American Medical Association or the American Board of Radiology which the A.M.A. is sponsoring to take over this work.

I am sending a copy of this to each member of the Standardization Committee of the Radiological Society, and to the secretaries of several radiological societies. I hope you do not find this action of mine officious. It seems to me that if any of us sees

the problem in a different light from which you see it, it is really his duty to state his ideas to those whom you are circularizing in regard to the proposition.

I imagine the matter will not be decided hastily, but that we will have a chance to converse about it at the next meeting of the Radiological Society, in the Standardization Committee.

(Portmann to Newell) September 10, 1941

Your reaction and response to my letter to the secretaries of the national radiological societies was expected. Your opposition to my idea of having the American Board of Radiology examine and certify radiological physicists was manifest when some of us discussed it in Cleveland last fall. I do not find your expression of disagreement "officious". Surely we have respect for the other fellows opinions.

Before commenting upon your arguments, let me state that my letter to the secretaries of the several societies was unauthorized and personal. It was my idea of the proper procedure to get the matter before the American Board of Radiology because this body is authorized by the Societies only to examine and certify physicians. Therefore I suggested that if the executive committees should be favorably impressed with my proposal for them to instruct and to authorize their representations in the A.B.R. to consider the matter and take whatever action deemed expedient.

First you comment upon differences between physician-patient and physician-physicist relationships regarding the significance of certificates "on the wall". Certainly the matter of whether or not certificates are hung on the wall depends upon personal preferences and is trivial. Differences in the relationships mentioned do exist but also there are pertinent similarities, some of which I will point out to you. (1) Physicians (radiologists) are concerned with the qualifications of physicists to specialize in standardization of apparatus etc. because they have encouraged, if not required, physicists to submit to examinations and registry. (2) Physicists, some forty-four of them, have been willing to submit to examination and/or registry because they want physicians to recognize their special qualifications. (3) Physicians prefer to consult and employ physicists who they know are qualified because they have been and are registered. (4) Physicians and physicists have found it expedient to publish lists of qualified registered physicists. (5) Physicists asked for, and received certificates of special qualification in radiology which were issued jointly by the Standardization Committee of the Radiological Society of North America and the Committee on Safety and Standards of the American Roentgen Ray Society. These certificates were signed by Lauriston S. Taylor and me without special dispensation.

You state "leave the certification of physicists in the hands of the Radiological Society". I assume you mean the Radiological Society of North America. It is true that this society originated and has fostered the registry of physicists. But it seems to me that this activity now has broader aspects of concern to all radiologists in the United States and Canada no matter what may be their affections or affiliations regarding radiological societies. You and I have been members of the Standardization Committee of the R.S.N.A. almost since its inception. I feel sure that this committee has no expectation of limiting or requiring radiologists to accept its registry of physicists as "the one and only" or final authority. As a matter of fact, the committee on Safety and Standards of the American Roentgen Ray Society has been equally interested and cooperated in the registry of physicists and lists have been published in the "American Journal of Roentgenology and Radium Therapy" which were the same as those that appeared in "Radiology". These lists were compiled and published with authorization from a physicist, Lauriston S. Taylor, who is chairman of both standardization committees of which you also are a member. Will you agree that it would be absurd for each of the four radiological societies in this country to have its own and different registries of physicists? Would it not be better to have one organization representative of and responsible to all societies to examine and certify physicists? The only organization that does represent, and is the responsibility of all societies, is the American Board of Radiology.

You suggest "at the widest ask the American College of Radiology to undertake certification of physicists". But is this organization "the widest"? The ambition of the College is to enroll every diplomate of the A.B.R. Perhaps there are some, though members of other radiological societies who will not, or can not, join the College for economic or other reasons yet would like to, and should have something to say about qualifying physicists whom they may consult. Although the college is a representative

body, can it assume any greater authority than any other radiological society? In addition, when you made your suggestion about the College you could not have taken into consideration the fact that physicists can not be members of it. The registered physicists have no organization of their own. They can be indirectly represented on the A.B.R. through affiliations with the A.R.R.S. and/or the R.S.N.A. No doubt they would like to have a little something to say about the matter. As a Fellow of the A.C.R. I understand its primary objectives are to foster and improve the economic status and educational faculties of radiologists. I doubt that it would be interested in other projects such as standardization of apparatus etc. or the examination and certification of physicists, at least it has not been active in these directions nor has it a committee for "Standardization".

You wrote "I see no reason for asking the American Medical Association or the American Board of Radiology which the A.M.A. is sponsoring to take over this "work". I certainly would not favor, and did not propose to ask the A.M.A. to "take over this work".

Where did you get the erroneous idea that the A.M.A. ever has sponsored, or dominates, or is responsible for the A.B.R.? According to my dictionary, a sponsor "is one who binds himself to answer for another's defaults". Do you think that the A.M.A. would assume responsibility for the defaults of the A.B.R.? Perhaps I had better inform you about how the A.B.R. is constituted and the limits of its authority. The Radiological section of the A.M.A., the American College of Radiology, the American Roentgen Ray Society, the Radiological Society of North America, and the American Radium Society each have three representatives who are trustees of the American Board of Radiology. These trustees are responsible to and report to their respective organizations about the actions of the A.B.R. The representatives of the Radiological Section of the A.M.A. have no more to say about the activities of the A.B.R. than those of any other society. The A.B.R. was organized by, represents, is responsible to, and is sponsored by all radiological societies only to examine physicians aspiring to specialize in one or several branches of radiology and to certify those found qualified. Therefore the A.B.R. is not authorized, but I think should be instructed to examine and certify physicists.

Now why do I think that the A.B.R. "should be asked to take over this work of examining and certifying physicists". (1) Because it is a corporate body and the only one representing all radiologists in the United States, concerned with the qualification of physicists whom they employ or consult regarding standardization of apparatus or ability to collaborate in scientific investigations pertaining to the physical aspects of radiology. (2) Because the A.B.R. represents registered physicists indirectly and physicists assist it in conducting examinations. (3) Because the A.B.R. has had experience, is well organized, has been successful and is capable of conducting examinations in radiology including physics. With minor modification of procedures it could examine physicists as well as physicians and better than any other groups or committees from radiological societies. (4) Because every radiologist who has appeared before the A.B.R. realized that certification by it is desirable and assurance of qualification. Therefore, physicians and physicists would have greater confidence in certification of physicists by the A.B.R. than registry by any one or two committees with doubtful, if any authority.

Dr. Robert Stone's response was addressed to the members of the Executive Committee of the RSNA (9/13/41). The principal paragraph of his letter follows:

"Dr. Newell brings out the point that the American Board of Radiology represents not only the Radiological Society but the American Medical Association, and functions mainly as an examining board of the Specialty Board from the American Medical Association. He feels that as such it has no interest in the certification of physicists, who are primarily an interest of radiologists. The certificate of the American Board of Radiology means to radiologists something that they can hang in their office to let their patients know that they have qualified as specialists. It means that they can have their names in the books of Specialists that rest on the desks of most other specialists, so that one can readily determine what radiologists are qualified in any given city, or community, in the United States or Canada. The so-called medical physicists need only be known to radiologists. They do not need to be selected by a body which broadcasts the fact to the entire medical profession and the public that such and such a physicist is approved. On the other hand, they do not need to be known to

radiologists throughout the country, and under the present circumstances, are known through the two committees of the Radiological Society and the American Roentgen Ray Society. Therefore, it seems to me, as it did to Dr. Newell, that it would be better to leave things as they are with regard to the physicists, or turn the duty of certifying them over to the American College of Radiology. I personally think that it would be a good move for the American College of Radiology to take over the certification of physicists and then they could let such list be sent to all radiologists who are members of the College. We hope that the College will very shortly include all radiologists in this country."

Portmann's response to Stone (9-19-41) again indicated his apparent acceptance of only those ideas which happened to match his own. His letter included the following:

"Apparently, you had not received my answer to Newell when you wrote. I hope that this may have helped to dispel some of the fog that seems to hang over San Francisco.

"Your principal point, suggested by Newell's letter, seems to be that physicians, not radiologists, and the public would not be interested in lists of certified physicists, whereas, they are interested in lists of radiologists certified by the A.B.R. and in the A.M.A. directory. I doubt that many physicians other than radiologists, and certainly not the public, have the A.B.R. registry on their desks or ever refer to it, but they may occasionally refer to the A.M.A. directory. Can you think of any valid objection to the A.B.R. printing a list of physicists they may certify in the back of the booklet listing diplomates, for easy reference to them? Do you think this would be a convenient arrangement? For example, the names of associate members of the A.R.R.S. and R.S.N.A., most of whom are physicists, are printed in a separate grouping of membership.

"Do you know of any published list of registered physicists to which radiologists can refer, except those published about once a year in the two radiological journals? Within the past month I had an inquiry from a registered physicist (L.R.) asking where he could find a listing of his colleagues.

"Of course the A.M.A. published the names of specialist physicians certified by the examining boards, including the A.B.R. But just because the A.B.R. might list certified physicists separately is no reason to suppose that the A.M.A. would necessarily do likewise.

"Perhaps the correspondence about this matter will serve as meat, not too hard to digest by those who may have the courage or take time enough to chew on it."

After a review of the 1940 Committee proceedings by the members, a final report of the Committee was completed in September 1941, though there is no record that it was published. Among other things, the report included an up-to-date listing of the x-ray physicists. (This is probably the last report by the RSNA Committee. The United States became involved in the European War in December 1941, and most such committee activities came to a full stop.)

REPORT OF THE STANDARDIZATION COMMITTEE OF THE RADIOLOGICAL SOCIETY OF NORTH AMERICA
Cleveland, December 4, 1940

A proposal was introduced of having the Board of Radiology take over the operation of and responsibility for the registry of x-ray physicists. The question was debated at the last meeting of the committee held in Cleveland in 1940. Final decision on the matter has not been reached yet. Objections were raised that such formality might involve operational expenses and therefore necessitate application fees. There is a question as to whether this is justifiable in consideration of the nature of the work which is to be carried out. It was agreed that if the Board of Radiology were to take over the registry of physicists, those already registered should be carried over without further examination, after which the registry will be divided into three classes: Class A, existent registered physicists; Class B, physicists of possible good possibilities, but still questionable; and Class C, new physicists. It was proposed and adopted that

new applicants be given a written examination and an oral examination by the nearest board member. The written examination is to be prepared by a group including at least one radiologist, and the oral examination group shall likewise include at least one radiologist. This suggestion was made in order to minimize the traveling expenses of an applicant and would take away from any individual the onus of having to fail an applicant. It should be pointed out that in general the physicists carrying out control calibration work are people of limited income as compared with the practicing radiologists, and it is therefore, necessary to avoid burdening the physicists with any additional expense beyond what is absolutely necessary.

Dr. U. V. Portmann, who introduced the question of having the Board of Radiology take over the registry of x-ray physicists, was appointed a sub-committee of one to approach the Board of Radiology informally on this question and make a report to the Standardization Committee at its next meeting in 1941. This report is not yet available.

Dr. Glasser gave a brief analysis of his initial study on report forms submitted by various members of the x-ray physicists. A breakdown of this analysis will be presented to the committee in the near future.

It was decided to divide the membership of the x-ray physicists into several different categories and also to keep the list up to date with regard to the activities of its members. Certain members of the list do not do outside calibration work, and their names shall be marked with an asterisk, which indicates that they are available for consultation only.

The following names were dropped from the registry of x-ray physicists:

C. T. Ulrey, I. Vigness, and C. C. Lauritsen by virtue of their no longer being active in the x-ray field.

It was recommended that physicists be encouraged to come to these society meetings and to cooperate with the Standardization Committees. It was also urged that the Standardization Committee endeavor to assist the registered physicists in any way it can.

A standard application form for registry of x-ray physicists was accepted and has been in use satisfactorily on all new applications before the Board of Registry.

Certificates of registry have been sent to all physicists approved thus far by the Standardization Committee through its examining board.

Inherent Filtration. The question of inherent filtration built into x-ray tubes has been presented to the committee informally by most of the x-ray tube manufacturers. They feel it is a very urgent problem from a commercial point of view. The committee has not yet reached a decision on this point. One viewpoint is that there is no need to talk about inherent filtration, and that the manufacturer should simply state his filtration and let the radiologist use the information as he wishes. The following proposal was made but not acted upon as yet:

That the manufacturer be informed of the committee's belief that for radiological tubes the proper designation of capacity is not in terms of roentgens, but in terms of focal spot size and permissible loading (kv, ma, time) for reasonable tube life. The actual filtering materials and thicknesses built into the tube should also be specified.

Treatment Field Uniformity. The paper on field uniformity by Miss Lillian Jacobson was brought to the committee's attention. The committee recommends that registered physicists make off-axis measurements of radiation intensity on all new installations or installations where the tube has been changed. It is also recommended that radiologists make occasional film checks to look for gross non-uniformity of his field. The film cassettes should be covered with about 1 mm of copper or 1/4 mm of lead in order to hold the exposure to within reasonable limits.

Technical Bulletin No. 1. The following suggestions were agreed upon:

1. Any further revisions should include some information on the calibration methods to be used with small ionization chambers.
2. Measurements of radiation from intra-cavity tubes are in a very unsatisfactory state at present. It was suggested that special vaginal measurements be made when using such tubes.
3. The cost of reprints of the Technical Bulletin No. 1 should be marked thereon.
4. It was recommended that the charts be printed separately, be arranged as loose leaves and be obtainable separately.

The following is the latest accepted list of x-ray physicists:

Aebersold, P.C., University of California
Blatz, H., Department of Hospitals, New York
Braestrup, C.B., Department of Hospitals, New York
Cole, K.S., College of Physicians and Surgeons, New York
Corrigan, K.E., Harper Hospital
Demers, J., University of Montreal
DuPont, C.A., Strong Memorial Hospital
Exner, F.M., Crocker Institute
Failla, G., Memorial Hospital
Glasser, Otto, Cleveland Clinic
Harrington, E.L., University of Saskatchewan
Henderson, G.H., Dalhousie University
Folson, T.R., Memorial Hospital
Hackney, A.W., Stanford University Hospitals
Hudson, J.C., Collis P. Huntington Memorial Hospital
Henny, G., Temple University Hospital
Jacobson, L., Montefiore Hospital
Laurence, G.C., National Research Council of Canada
Leppard, L.B., Department of Health, Canada
Landauer, R., Highland Park, Ill.
MacDonald, P.A., (Canada) Cancer Institute
Marinelli, L.O., Memorial Hospital
Marvin, J.F., University of Minnesota Hospital
Morgan, J.S., Duke University
Mutscheller, A., New York
Nurnberger, Carl E., Detroit, Mich.
Omberg, A.C., Vanderbilt University Hospital
O'Neill, D.B., University of Pennsylvania
Patterson, R.A., Rensselaer Polytechnic Inst.
Pugh, R.E., Pasadena, Calif.
Quimby, E., Memorial Hospital
Reinhard, M.D., (N.Y.) State Institute for Study of Malignant Disease
Robertson, J.K., Queen's University
Rose, J.E., Marine Hospital
Rovner, L., University of Chicago
Robb, C., Philadelphia General Hospital
Schwarzschild, M.M., Beth Israel Hospital
Shrum, G.M., University of British Columbia
Sidhu, S.S., University of Pittsburgh
Singer, G., National Bureau of Standards
Stenstrom, W., University of Minnesota
Stephenson, B.R., Edward J. Meyer Memorial Hospital
Taylor, L.S., National Bureau of Standards
Warren, S. Reid, Jr., University of Pennsylvania
Weatherwax, J.L., Philadelphia General Hospital
Williams, Marvin M.D., Mayo Clinic

The following names are at present before the Board for consideration: Scott W. Smith, H.D. Doolittle, Frank E. Hoecker, Arthur P. R. Wandlund, C. J. Garrahan, and Victor Hicks.

Respectfully submitted for the Committee,
Lauriston S. Taylor, Chairman

Acting on his own initiative (as noted on p. 152), Dr. U. V. Portmann, Vice Chairman of the RSNA Standardization Committee, wrote to the radiological societies on August 21, 1941, proposing that the registry of physicists be transferred to the American Board of Radiology.

The first that Taylor, or any non-radiologist member, was aware of this action was on receipt of an August 14, 1947 letter from Portmann as follows:

"The American Board of Radiology decided to set up a procedure for registering radiological physicists at its last meeting in Atlantic City. I am surprised that you did not hear from Kirkland regarding this. A Committee was appointed of which you are Chairman, to go over the list of those physicists already certified by the RSNA and to eliminate those that are unqualified or undesirable. There is also to be a procedure

for examination for those who will apply. They will be certified by the American Board examiners consisting of a committee of two physicists and a radiologist who may be examiners for the Board at any session. I suggest that you go over the list which you have and decide whether you believe they should be certified."

On August 20, 1947, Dr. B. R. Kirklin, Secretary of the American Board of Radiology wrote to Taylor as follows:

"I am enclosing a copy of the recommendations of the Committee appointed by the Board. The Board approved these recommendations and the President has directed me to submit to you the following names from which your group will select three according to Article 8 of the enclosed Resolution, to act as an examining board: Dr. Lauriston Taylor, Chairman, Dr. Edith Quimby, Dr. Otto Glasser, Mr. J. L. Weatherwax, Dr. Kenneth Corrigan, and Dr. G. Failla. The President has also appointed Dr. U. V. Portmann from the Board of Trustees of the American Board of Radiology to be a member of your Board of Examiners."

This whole procedure was met with a great deal of concern on the part of the physicists and others on the Standardization Committee. Apparently the plan had been set up and discussed during the war years, while the physicists were otherwise engaged. Their concern was not so much the idea itself, but the way it had been handled. It took a fair amount of effort on the part of the Chairman to prevent an open and public revolt on the question. Following is the report that Portmann submitted to the American Board of Radiology:

Report of a committee appointed to make recommendations
regarding certification of Radiation Physicists by the
American Board of Radiology
August 20, 1947

Physicists consulted (Ed. Note: It was never possible to identify those "physicists consulted.") by your committee were unanimously of the opinion that they would prefer to have the American Board of Radiology organize a procedure for examination and certification of physicists so that those who pass the examination in physics receive a certificate in Radiation Physics from the American Board of Radiology.

On the basis of instructions from the Board of Trustees of the American Board of Radiology and consultation with physicists your committee will make certain recommendations.

1. We recommend that a "physicist" shall be defined as one whose training and experience be in the study and applications of the interactions between matter and energy in the fields of mechanics, acoustics, optics, heat, electricity, magnetism, radiation, atomic structure, and nuclear phenomena. To qualify as a professional physicist he must have at least eight years of training and experience in physics. Toward this experience, four years of formal collegiate education with major emphasis on physics may be credited, year for year if it leads to a Bachelor's Degree, five years if it leads to a Master's Degree and seven years if it leads to a Doctor's degree. This collegiate work must have been done in a recognized institution by which is meant one which appears in the list of institutions approved by the Association of American Universities. A Radiation Physicist is a professional physicist as defined above, who has had at least two years of specialized training in radiologic physics including the study and measurement of ionizing radiations.

2. We recommend that the Constitution and By-Laws of the American Board of Radiology be altered and changed to enable examination and certification of Radiation Physicists.

3. We recommend that a group of physicists who have been registered under the auspices of the Radiological Society of North America be granted the certificate of Radiation Physicist by the American Board of Radiology.

4. We recommend that the Board of Examiners of Radiation Physicists consist of one Trustee of the American Board of Radiology appointed by the President and three examining physicists.

5. We recommend that the Board of examiners of physicists act as a committee on credentials and shall determine whether applicants are qualified to take the examination in Radiation Physics.

6. We recommend that the nature and conduct of the examination of physicists shall be the responsibility of the three examining physicists who shall select the chairman of the examining board.

7. We recommend that the examining board of physicists shall present their recommendations regarding certification of Radiation Physicists to the Board of Trustees of the American Board of Radiology and that the recommendations be given the same consideration as given recommendation of examining boards of Radiologists. The examining board of physicists may be present when their recommendations are being discussed. The Board of Trustees of the American Board of Radiology should issue certificates in Radiation Physics to those candidates recommended by the examining board in physics and approved by the Board of Trustees.

8. We recommend that the Board of Trustees of the American Board of Radiology nominate six certified physicists from whom all of the certified Radiation Physicists elect three by mail ballot to serve as an examining board of physicists.

9. We recommend that the fee for examination of Radiation Physicists be ten dollars.

Respectfully submitted, U. V. Portmann, Chairman, Ross Golden, Edwin C. Ernst.

Following up on Kirklin's letter, Taylor, after informal communication with several committee members, wrote to him on August 27th, recommending the selection of Drs. Quimby, Corrigan, and Failla as members of the examining board. He pointed out that it was not clear how the Board of Examiners of Physicists were to be selected, and whether it would consist of three members plus a Chairman, or whether the Chairman would be included in the three members.

Then, on August 29th, Taylor sent out a circular letter to all physics registrants.

August 29, 1947

I have been told informally that the American Board of Radiology has set up a Review Board for Registered X-ray Physicists. While I have not been told just what the examination and certification procedure will be, I assume that this information will be forthcoming at an early date.

As you know, some months ago, we sent a questionnaire to all registered physicists. Since there have been many inquiries regarding the services of such individuals, I would like to recommend that a summary of the replies to this questionnaire be published in the journal at an early date. Enclosed is a draft copy of a suggested report on this subject. In this copy I have not filled in the addresses of registered physicists but in the published copy I plan to do this in order to make the individuals more easily available to the radiologists. I would appreciate it if you could go over this draft and let me have any comments or suggestions which you may care to offer.

I have also prepared a map of the United States showing by circles the radius of activity of the different physicists and at the same time demonstrating how poorly covered certain areas in the country may be.

There is a wealth of material in the questionnaires which I have not attempted to discuss at this time. I have made some sort of breakdown of this, however, and would like to bring it up for a general discussion at the first meeting of our Review Board.

Enclosure.

August 26, 1947

REGISTRY OF X-RAY PHYSICISTS

On February 13, 1947 a questionnaire, of which a copy is attached, was sent to all physicists who have been qualified by the Board of Registry. This was for the purpose of bringing the list up to date, determining latest addresses, finding out to what degree the physicists were active in calibration and consulting services, and over what radius of action they would normally operate.

The names may be divided into three categories:

1. Those physicists whose work is largely confined to research activities, consulting, or only to the particular institution with which they are associated.

2. Those physicists who carry on active calibration and survey work within their own institutions and for the general public upon request.

3. Those who could not be located, did not return their questionnaires or are known to be deceased.

It is recommended that the first group be designated as Research and Consulting Radiation Physicists. It should be indicated whether or not they are available for occasional or special calibration and survey services.

It is recommended that the physicists in the second group be retained on the active registry and be indicated as available for general calibration services.

It is recommended that the names of the people not returning questionnaires be dropped from the active registry of x-ray physicists with the understanding that they can be reinstated at such time as they may demonstrate that their interim experience has kept them abreast of developments.

The following comprise the names of physicists concerned primarily with research and consulting activities:

P. C. Aebersold
K. S. Cole
M. Dauer
G. Failla
V. Hicks
G. C. Laurence
L. D. Marinelli
C. E. Nurnberger
J. K. Robertson
W. Stenstrom
L. S. Taylor
E. Quimby
M. M. D. Williams
H. M. Parker

The following comprise the names of individuals engaged in active calibration work:

C. B. Braestrup
F. W. Chambers
K. E. Corrigan
J. Demers
O. Glasser
A. W. Hackney
G. H. Henderson (?)
G. Henny
F. E. Hoecker
L. Jacobson
H. E. Johns
R. Landauer
J. F. Marvin
McDonald (?)
R. E. Pugh
M. E. Reinhard
C. Robb
J. E. Rose
S. S. Sidhu
G. R. Stephenson
S. A. Warren
J. L. Weatherwax
S. W. Smith
L. Rovner

The following people who have in the past been certified by the Board are disqualified either for failure to return the questionnaire, commercial connection, or deceased:

I. H. Blatz
C. A. DuPont
F. M. Exner
T. R. Folsom
E. L. Harrington
L. B. Leonard
J. E. Morgan
A. Mutscheller
A. C. Omberg
D. B. O'Neill
M. M. Schwarzschild
G. M. Shrum
R. A. Patterson

The answers to the questionnaire disclosed numerous facts of general interest. In the first place, it has been found that in the United States and Canada there are only about 24 registered physicists who are in a position to undertake radiation calibration and radiation survey work. Of these, less than half are in a position to devote as much as 50% of their time to such work--there are only 8 who devote 100% of their time to these activities. Judging by the large number of requests that have been received asking for advice on this subject, there is a demand for much more service of this sort than can presently be supplied. It might be pointed out that the Army and the Veterans Administration both require calibrations and inspection services by registered physicists. Needless to say, there have been a great many instances in which such services could not be obtained in the desired locality.

The geographical distribution shows that the coverage over the country as a whole is spotty. The northeast section centered around New York and Pennsylvania appears to be

heavily covered and yet it is believed that the available services in these areas are not in excess of the demand. The whole south and southwest is essentially uncovered except as one or two individuals may be in a position to make prolonged 1000-mile tours a few times per year. (We have had a large number of calls from Army and Veterans Administration requesting services south of Ohio.) Of the west coast only California can be said to be covered, although there is one individual who will make occasional long trips into bordering states. States like Washington, Oregon, and Idaho have no coverage. The coverage in Maine, New Hampshire, Vermont, and Massachusetts appears to be too thin. This applies equally to all of the states south of Virginia and in the central west. It is recommended that the Board encourage people to provide consulting and calibration services in these areas even though it may not justify a man spending a 100% of his time in the area. The availability of part-time people would be of great benefit.

The fees charged for calibration services appear to vary over unreasonably wide limits, although analysis of this question is difficult since it depends so much on local and individual conditions. It will appear, however, that of the fees charged, few, if any, can be considered as excessive considering the services supplied. It appears on the face, that many of the fees are unreasonably low.

Sample copies of routine calibration reports were submitted by all people in group 2. These appeared reasonably consistent and most of them gave all of the information which would be of value to the radiologist. Occasionally the physicists have supplied other data in their report which may be considered as unnecessary but which is no doubt justified in many cases by the specific wishes of the radiologists. These data are usually computed from the direct calibration data, mainly, for ready reference by the radiologists.

There was considerable divergence of opinion regarding the physicist's own recommendations as to how often a therapy installation should be calibrated. The majority appeared to feel that at least once in six months was necessary, with the provision that a new calibration should be made after the installation of a new tube.

The opinions regarding the minimum requirements for qualification as a radiological physicist were likewise quite variable. It was almost unanimously agreed that the physicist should hold at least an A.B. or equivalent degree. The majority also felt that experience in the clinical x-ray department is essential. The requirements in this matter range from none to five years but the majority were of the opinion that 6 to 12 months was essential. It is interesting to know that in this regard, the more experienced individuals were the ones who were most likely to recommend longer training. More than half of the questionnaires indicated that an experience of 10 calibrations made for the public should be adequate. The majority of the remaining questionnaires indicate that 50 calibrations would be adequate.

At one of the recent radiological meetings there was an informal gathering of about a dozen physicists who happened to be in attendance. A general confab was held regarding the problems which they have encountered in their work. A great many worthwhile and interesting ideas were brought out and everybody seemed to profit by the opportunity to discuss his problems with his co-workers. The questionnaire indicated unanimous interest in holding future gatherings in conjunction with annual meetings or radiological societies. It is recommended therefore that provision be made for holding such a meeting of the societies at some specified time and place. It is further suggested that we use one of the society classrooms at such a time as it may not be otherwise used. Arrangements to do this can be made in advance with the secretary of the society and a notice of this effect printed in the announcement of the meetings.

As it is now, it is believed that many of the younger or lesser known physicists have tended to stay away from the society meetings feeling that they did not have sufficient interest in common with the other people attending. This is certainly not the case. In the first place, there are usually a number of radiological physicists, particularly from the older group, in attendance. Secondly, it is believed to be highly desirable for the radiological physicists to mingle with the radiologists to discuss their common problems, to make themselves known, and to give and receive such beneficial advice and information as they may be able to.

Enclosure: Not included here

Kirklin, in responding to the letter of August 27th from Taylor, said,

"My interpretation is that the Board of Trustees is to appoint six radiation physicists, which they have already done, and consisting of yourself as Chairman, Dr. Edith Quimby, Dr. Otto Glasser, Mr. J. L. Weatherwax, Dr. Kenneth Corrigan, and Dr. G. Failla, and that you in turn are to send a mail ballot containing the names of these six physicists to each of the radiation physicists already certified by your group and ask them to vote for three of the six who will act as the Examining Board of the American Board of Radiology. Those receiving the highest number of votes obviously will constitute the examining board of three.

"Dr. Portmann has been appointed by the American Board of Radiology to serve with your examining committee....."

The ballot mentioned above was ultimately sent on September 4, to 37 of the physicists who had been registered by the Standardization Committee. Failla, Glasser, and Quimby were the three individuals finally selected.

Glasser, in reviewing the suggested classification of registered physicists into three groups, felt that a better plan would be to list all qualified registered physicists in one group and star those who did more extensive calibration work. The reason behind this idea was the fact that only eight physicists in the list devoted 100 percent of their time to calibration work. The rest spent some or all of their time in research and consulting activities. The tendency at that time was that the radiologists themselves calibrated their own apparatus or were qualified to do so in order to pass the Board

On September 18th, the special committee named by the ABR met to consider final plans for establishment of the Board to examine radiological physicists. Following is their final statement:

October 23, 1947

TO THE TRUSTEES OF THE AMERICAN BOARD OF RADIOLOGY

Many years ago the Standardization Committee of the Radiological Society of North America assumed the responsibility of examining physicists as to their qualifications to calibrate x-ray apparatus and in uses of radium. The only recognition given those qualified has been a letter from the Chairman of the Committee and listing of their names a few times in "Radiology".

Many radiologists and physicists believe that examination, certification, and registry of physicists in radiology is a proper function of the American Board of Radiology in compliance with its published purposes. First: to encourage the study and promote and regulate the practice of Radiology. Second: to elevate the standards of Radiology by encouraging its study and improving its practice. Third: to determine the competence of specialists in Radiology, to arrange, control and conduct investigations and examinations, and to test the qualifications of voluntary candidates for certificates to be issued by the Board. Fourth: to serve the public, physicians, hospitals, and medical schools by preparing lists of practitioners who shall have been certified by the Board.

For these reasons the American Board of Radiology was requested at its last meeting at Atlantic City in June 1947 to undertake examination, certification, and registration of physicists. A special committee was appointed to study details of procedures and make a report. The special committee includes Kenneth E. Corrigan, G. Failla, Otto Glasser, Edith H. Quimby, Lauriston S. Taylor, (and) James L. Weatherwax to represent physicists and U. V. Portmann to represent the Board.

The special committee met at Atlantic City on September 13, 1947. It was deemed advisable to invite others to participate in the discussion. Those invited were Edwin C. Ernst, Robert S. Newell, and Robert B. Taft, who are Diplomates of the Board and C. B. Braestrup, George C. Henny, and M. M. D. Williams who are qualified physicists.

As a result of the discussion, your special committee makes the following recommendations:

1. We recommend that the American Board of Radiology arrange for examination of physicists according to procedures herewith suggested and that those found qualified be granted diplomas in the following categories:
   (a) Diplomates in Radiological Physics.
   (b) Diplomates in X-ray and Radium Physics.
   (c) Diplomates in Medical Nuclear Physics.

2. We recommend that the American Board of Radiology assume no legal or financial responsibility for examination, certification, or registry of physicists, but that this be a separate activity; that after January 1, 1948, the Secretary-Treasurer of the Board provide printed forms for applications for examinations of physicists in the categories designated according to standards suggested herewith; that he accept examination fees to be assigned to a special account to be disbursed and audited under the supervision and direction of the Board.

3. We recommend that the following qualified physicists be granted diplomas by the Board, in the categories designated, without further examination on payment of a fee of $25.00.

(a) Diplomates in Radiological Physics

P.C. Abersold, Oak Ridge, Tenn.
C.B. Braestrup, New York City
K.E. Corrigan, Detroit, Mich.
G. Failla, New York City
Otto Glasser, Cleveland, Ohio
H.S. Hayden, Detroit, Mich.
G.C. Henny, Philadelphia, Pa.
G.C. Laurence, Ottawa, Can.
L.D. Marinelli, New York City
K.Z. Morgan, Oak Ridge, Tenn.
H.M. Parker, Handford, Wash.
Edith H. Quimby, New York City
J.K. Robertson, Kingston, Can.
J.E. Rose, Chicago, Ill.
W. Stenstrom, Minneapolis, Minn.
L.S. Taylor, Washington, D.C.
J.L. Weatherwax, Philadelphia, Pa.
M.M.D. Williams, Rochester, Minn.

(b) Diplomates in X-ray and Radium Physics

F.W. Chambers, Bethesda, Md.
M. Demar, Washington, D.C.
A.W. Hackney, San Francisco, Calif.
F.E. Hoecker, Lawrence, Kan.
Lillian Jacobson, New York City
P.A. McDonald, Winnipeg, Can.
J.F. Marvin, Minneapolis, Minn.
C.E. Nurnberger, Detroit, Mich.
H.E. Pugh, Buffalo, N.Y.
M.C. Reinhard, Buffalo, N.Y.
C. Robb, Philadelphia, Pa.
L. Rovner, Cambridge, Mass.
S.S. Sidhu, Pittsburgh, Pa.
S.W. Smith, Washington, D.C.
B.R. Stephenson, Buffalo, N.Y.
S.R. Warren, Philadelphia, Pa.

4. We recommend that the final decision of the Board be published in radiological journals and elsewhere as may seem feasible, for the information of radiologists and physicists.

5. We recommend that each candidate for examination in physics be required to furnish with his application evidence that he has met the following standards:

(a) Satisfactory moral and ethical standing.
(b) Holds himself to be a specialist in the category of physics designated in his application.
(c) Is a citizen of the United States or Canada. Candidates from other countries must be permanent residents of that country and native citizens thereof.
(d) Holds a degree of Bachelor of Arts or its equivalent and has majored in physical science or engineering.
(e) Is a member of the American Physical Society or similar organization of Physicists.

(f) After graduation from college has had at least one year postgraduate experience in radiation physics or in a radiation physics laboratory.
(g) Candidates for examination in Radiological Physics in addition to requirements specified in paragraphs a.b.c.d.e. and f. must have had at least one year of experience associated in a Department of Radiology approved by the Board and throughout one of the two years specified has had experience in medical application of artificial radioactive materials. With the application he must submit personal records of calibrations of one low voltage and one high voltage x-ray apparatus, personal records of one x-ray protection survey and one protection survey for radioactive materials.
(h) Candidates for examination in X-ray and Radium Physics in addition to requirements specified in paragraphs a.b.c.d.e. and f. must have had at least one year of experience associated in a Department of Radiology approved by the Board. With the application he must submit records of personal calibration of one low voltage and one high voltage x-ray apparatus and personal records of one x-ray protection survey and one personal protection survey for radium.
(i) Candidates for examination in Medical Nuclear Physics in addition to requirements specified in paragraphs a.b.c.d.e. and f. must have had one year of experience in physical procedures relative to medical application of radioactive materials.
(j) Applications shall be endorsed by a Diplomates of the American Board of Radiology and a Diplomate in Radiological Physics who have personal knowledge of the experience, training, moral and ethical standing of the applicant and that he is qualified to take an examination.
(k) A fee of $25.00 shall accompany the application which will be refunded if the application for examination is not accepted.
(l) Applications shall be submitted to the Secretary of the American Board of Radiology. He shall forward them to each special examiner to be appointed by the Board to examine, and they shall decide about the candidates' fitness to be examined. Those approved for examination shall be informed by the Secretary of the Board when and where to appear.

6. We recommend the following procedures in regard to the examination and certification of physicists.
(a) The American Board of Radiology shall appoint a group of special examiners comprising three Diplomates in Radiological Physics and one Diplomate in Radiology.
(b) Examinations shall be conducted at the time and place of meetings of the Board of Radiology.
(c) Examinations in radiation physics shall be oral though practical or written examinations may be required. Examinations will be designated to test the candidate's knowledge and his fitness to practice physics in the category for which he applies. Accordingly, the examination may include questions regarding the structure of matter, construction and operation of x-ray apparatus, the use of radiation measuring instruments, determination of radiation quantity and quality, protection from x rays, radium and artificial radioactive substances, preparation of radioactive applicators, determination of dosages of x rays, radium and radioactive substances and other questions relative to the category in which the candidate applies.
(d) Each candidate shall be examined separately by each special examiner who shall grade him. No candidate shall be recommended for certification whose general average grade is less than 75 percent or has been graded less than 70 percent by any examiner.
(e) The examiner who represents the Board shall report to it the results and recommendations of the special examiners and those approved for certification shall be granted diplomas in the category recommended by the examiners which may not be that for which application is made.

The names and addresses of those certified shall be published appropriately, according to their certification, in the Registry of Diplomates of the American Board of Radiology.
(f) Candidates who fail an examination shall not be admitted to another examination until one year has elapsed. They must submit another application for reexamination at least sixty days prior to the next meeting of the Board and pay an additional fee of $10.00.

(g) A diploma may be revoked if in the opinion of the Board a mis-statement of fact has been made in the application or any other communication to the Board or its representatives, or for expulsion from a scientific society for misconduct. Under such circumstances the diplomas must be returned to the Board and the name of the individual shall be omitted from the Registry of Diplomates.

Respectfully submitted, Kenneth E. Corrigan, G. Failla, Otto Glasser, Edith H. Quimby, Lauriston S. Taylor, James L. Weatherwax, and U. V. Portmann.

Changes in the Registry of X-Ray Physicists were, of course, necessary for various reasons. For example, a physicist who had withdrawn earlier from the registry was reinstated when he gave up his commercial connection.

Several individuals were dropped because they were no longer active in calibration services. Others were dropped because of failure to reply to correspondence, or because they could not be located. Meanwhile, new applications were on hand. Following is the list of registered x-ray physicists, as of January 22, 1948.

(Taylor to Kirklin) January 22, 1948

I have a recent letter from Portmann with regard to the certification of physicists. The attached list gives the best addresses which I have available for those who have already been certified.

In addition to this list we should probably be prepared to add the name of Mr. Y, who was certified some years ago but his name was withdrawn from the register because of his commercial connection. He has now given up this connection and is therefore eligible for reinstatement on the list of physicists under category B.

Because of inactivity in the field the following individuals have been dropped from the list in accordance with the discussions in Atlantic City:

I. H. Blatz	G. H. Henderson
K. S. Cole	R. A. Patterson
J. Demers	M. M. Schwarzschild

These people have been certified in the past.

There was some discussion about the inclusion of Dr. Y's name and I noticed that it is not included in the list which I received from Portmann on October 23rd. There was some discussion regarding his ethics and the question was raised about his being dropped at least pending some further investigation. I do not recall the exact decision on this. Perhaps Dr. Portmann who kept the minutes of that meeting would be able to supply this information.

I made a survey about a year ago about the activity of registered physicists. The following people did not reply to the correspondence or could not be located and hence were dropped from the registry:

F. M. Exner	A. Mutscheller
T. R. Folsom	A. C. Omberg
L. B. Leppard	D. B. O'Neill
J. E. Morgan	

In addition to the above names, we have applications pending from the following individuals:

G. H. Cameron	C. J. Garrahan
D. B. Cowie	H. E. Johns
Frank Dreisinger	V. Peterson
R. E. Fearon	W. W. VanAllen
S. Feitelberg	T. J. Wang
G. Ferlazzo	E. C. Lee
E. Focht	

In reply to all recent inquiries that I have had with regard to the registry of physicists I have taken no action on the applications since this work was being taken over by the Board of Radiology. Accordingly, I think your office should follow-up the

various inquiries which we have had on the including cases in order that we may determine whether or not the individuals are still interested.

LIST OF REGISTERED X-RAY PHYSICISTS

Dr. P. C. Aebersold
Dr. C. B. Braestrup
Comd. F. W. Chambers, Jr.
Dr. K. E. Corrigan
Dr. G. Failla
Dr. Otto Glasser
Mr. A. W. Hackney
Mr. H. S. Hayden
Dr. G. Henny
Dr. F. E. Hoecker
Miss L. Jacobson
Mr. J. E. Rose
Dr. S. S. Sidhu
Mr. B. R. Stephenson
Dr. L. S. Taylor
Mr. J. L. Weatherwax
Dr. P. A. MacDonald
Dr. L. D. Marinelli
Dr. J. F. Marvin
Dr. K. Z. Morgan
Dr. Carl E. Nurnberger
Dr. H. M. Parker
Mr. R. E. Pugh
Dr. E. H. Quimby
Dr. M. C. Reinhard
Mr. C. Robb
Dr. J. K. Robertson
Dr. L. Rovner
Dr. K. W. Stenstrom
Dr. S. W. Smith
Mr. S. R. Warren, Jr.
Dr. M. M. D. Williams

The examination and certification of physicists proceeded under the Board of Radiology and developed into a strong and ongoing program. The transfer, and the way it was handled did, however, cause the gradual dissolution of the professional comradery that had developed between physicists and radiologists during the preceding 2 decades. It led to a break in the long and close cooperative programs between the radiological profession and the National Bureau of Standards.

The overall process permanently alienated a number of physicists who had been strong supporters of radiology and radiological physics. The reason for this was not the movement of the operation into the Board of Radiology, because the transfer was generally recognized as a reasonable and proper solution. The difficulty lay primarily in the fact that the change had been largely spearheaded by three radiologists who, while excellent in their field of radiology, had little competence in physics or its applications to radiology. Most of the critical actions taken in this transfer were done without the knowledge of the small group of radiological physicists who, not only were the ones principally concerned, but who were also the ones included after all the arrangements were essentially completed.

CHAPTER 10. RADIOLOGICAL PHYSICS PROGRAMS

Radiation Shielding

The NBS programs on radiation shielding and protection design had many ramifications ranging from studies and tests of materials to theoretical analyses of the attenuation and scattering problems. Through all of this, there was strong input to the programs from the radiological societies and industry. The programs initially grew out of efforts primarily by the American Roentgen Ray Society in the early 1920's (see p. 4) and, later, the Advisory Committee on X-Ray and Radium Protection, which provided an unofficial relationship between the Bureau of Standards and outside groups. These collaborative efforts became a major preoccupation of the staff until the formation of the National Council on Radiation Protection (NCRP) under a Federal Charter in 1964. The workings and relationships of the Advisory Committee and the NCRP are not discussed in this report since they have been the subject of exhaustive treatment (Taylor, 1979).

In addition to the Committee activities, laboratory work in the thirties included studies of the protective value of materials used for radiation shielding. From time to time, articles on protective materials were published in outside journals which discussed the applications of the research results to topical problems.

These researches continued until about 1960 and included some very sophisticated theoretical analyses as well as fundamental experiments on x-ray scattering, penetration, and diffusion in large bodies of material by wide ranges of radiation energies. This work will be discussed in detail up to 1955, and covered briefly with references to the bibliography, for the period of the late 1950's and early 1960's.

In the midthirties, George Singer undertook a study of protective glasses in which the prime protective materials were lead and barium (Ref. 49). Routine samples of glass used in industry and batch analyses from the manufacturers were obtained. (A batch analysis is based on the raw material that was put into the melt.) At the same time the Glass Section at the Bureau of Standards made up a number of glass samples with known batch analyses. The basic requirement of all these samples was that they be relatively clear with a minimum amount of color. Some heavier protective glasses were also made, but were unacceptable from an optical point of view.

As expected, the protective coefficient of glass containing only lead oxide showed little variation as a function of x-ray energy. (As noted earlier, the protective coefficient is the ratio for any piece of material, of the equivalent thickness of lead to the thickness of the material in question (Gorton, 1918).) (See p. 3.)

For glass containing lead only, in the form of lead oxide, the protective coefficient for glass containing 46 percent PbO would be 0.15, while for 70 percent PbO glass the protective coefficient would be 0.29. Higher contents of lead oxide resulted either in excessive coloration or excessive fragility of the glass.

Protective coefficients for lead-barium glasses showed considerable energy dependence over the range studied, extending from about 90 to 195 kV. For example, a glass containing 62 percent lead and 10 percent barium had its protective coefficient decrease from 0.33 at 90 kV to 0.29 at 195 kV. In general, the addition of the barium was more important to the glass because it minimized the coloration without contributing importantly to the protective coefficient. As a result of the studies, it was possible to develop a series of empirical relations for the protective coefficient of either type of glass (Ref. 49).

By the latter half of the 1930's, substantial developments in the x-ray industry were leading to the widespread use of 400-kV oil-immersed x-ray sources, and installations at still higher energies, up to a million volts, were in production. For example, the General Electric Company was making a 1-million-volt voltage resonance transformer, the coils of which surrounded a long multi-section sealed-off x-ray tube. These x-ray units were highly reliable for both medical and industrial purposes. At that time, General Electric and the Kelley-Koett Company were also promoting open high-voltage installations for medical purposes, and the High Voltage Engineering Corporation was producing belt generators operating on the Van de Graaff principle, primarily for medical purposes.

Because these higher energy installations required large amounts of protection, it was impractical to provide such protection in the form of sheet lead. The most economical protection, and one which also served a structural function, was ordinary vibrated commercial concrete, such as that used in good building construction. For special circumstances, concrete employing steel punchings or iron ore as a part of the conglomerate provided even higher protective values. However, such special applications were relatively limited because of the cost. For this reason, it was becoming increasingly important to have better data on the shielding properties of standard concretes. To provide this information, a program was set up under Singer, Taylor, and Charlton in 1937. This was designed to cover the range from 200- to 400-kV constant potential (Ref. 63).

The apparatus for carrying out these studies was the recently completed 600-kV installation (see p. 189) and the pressure ionization chamber (previously discussed, p. 45). The x-ray tube was a 400-kV oil-cooled tube with thick glass walls, designed to operate in air. Since the 600-kV x-ray generator operated with the positive side grounded, the sealed-off x-ray tube was simply mounted on the floor in the vertical position (see photo No. 32). Surrounding the tube was a stack of porcelain insulator rings about 10 inches in diameter inside and 18 inches outside, thus, providing some shielding. In addition, the tube was located in a room shielded for the 600-kV installation and separated from the laboratory space by a 3/4-inch lead door. The x-ray beam was diaphragmed down to a diameter of about 1 cm, thus permitting measurements of the radiation attenuation under what is described as "narrow beam" conditions. The concrete being tested was made under controlled conditions in the Cement Section of the National Bureau of Standards.*

*Note that, by this time, the Bureau of Standards had regained its original name "National Bureau of Standards."

Another form of protective concrete was that of solid building blocks that could be erected by ordinary masonry procedures. However, commercially available building blocks were manufactured with densities less than that for vibrated concrete. A good commercial concrete has densities ranging from about 2.36 to 2.40 g/cm^3 or, expressed in engineering language, 146 to 152 pounds per cubic foot, while the densities of solid concrete building blocks range from 2.0 to 2.1 g/cm^3. Cinder blocks would be much less.

The lead equivalence of concrete varies considerably with thickness and with voltage. For example, at 400 kV the lead equivalence of concrete for thicknesses ranging from about 100 to 400 mm varies from about 4 to 25 mm of lead.

For a given concrete thickness of, say, 250 mm, the lead equivalence varies from 5 to 15 over a voltage range of 200 to 400 kV. Following the narrow beam study, four papers were published calling attention to the substantial difference between broad beam and narrow beam attenuation (Refs. 28, 95, 197, and 120). However, as we will see, narrow beam measurement does not really define the true attenuation of the radiation impinging upon the concrete wall.

In 1944 a paper by Folsom and Focht drew attention to the fact that particularly at the higher voltages (million volt range) the effective attenuation of protective walls was less for broad x-ray beams (Folsom, 1944). In 1945, after the laboratory had returned to a peacetime schedule, a new and more comprehensive study of the problem was undertaken by Braestrup together with Wyckoff and colleagues. Attenuation data were obtained using large concrete test slabs subjected to 1- and 2-million-volt x rays and the radiations from radium and cobalt-60. The studies were reported in a series of papers (Refs. 81, 95, 107, and 120).

Tables were developed giving the thickness of concrete required to provide a given degree of protection against broad beams of x rays generated at 1 and 2 MeV and for various tube currents and distances. This clearly demonstrated the necessity for using broad beam rather than narrow beam attenuation data for protection design. Because of photon scattering in the concrete, the radiation levels at any point on the emerging side of the wall would show a higher dose rate. Thus, walls designed using narrow beam data could underprotect personnel.

A little later, a similar study using narrow beams of x rays was carried out by Singer, Wyckoff, and Day on the relative thickness of lead, concrete, and steel required to protect against x rays generated between 200- and 1,400-kV constant potential. In this case, the x-ray tube target was lead and the x-ray beam was taken off at right angles to the electron beam passing only through the water cooling jacket.

As in previous studies, the thickness of the lead equivalence of concrete varied markedly with voltage. For example, the ratio of concrete thickness to lead thickness as a

function of tube voltage decreased from about 60 at 200 kV to 8 at 1,400 kV. Similarly, the ratio of steel thickness to lead thickness as a function of tube voltage decreased from about 13 to 1.5 over the same voltage range (Ref. 87).

The narrow beam attenuation study was followed immediately by a broad and narrow beam attenuation comparison, again, in lead and concrete. This was carried out by Wyckoff, Kennedy, and Bradford. In this case, the x-ray beam was again generated at a lead target but taken off in the direction of the electron beam; that is, axially with the x-ray tube. The basic quality of the beam, on passing through the thin lead target and the water cooling jacket, was different from that taken off at right angles.

When the laboratory was constructed, provision was made for just such an experiment by providing a large, deep pit in the sub-basement, immediately under the target of one of the x-ray tubes. Thus, scattered radiation could not go around the edges of the test slabs and be measured. Furthermore, measurements at large distances from the source could be made so that the radiation beam was more nearly parallel, allowing one beam size to be used for all concrete thicknesses. For these measurements, concrete slabs 6 feet square and 6 inches thick were placed successively over the opening to a total thickness of about 4 feet. Three-inch slabs were available for smaller variations and thicknesses (see photo No. 33).

An ionization chamber mounted on a wall rack in the pit could be moved from the top to the bottom of the pit. It could also swing sideways so as to determine the intensity across the beam (see photo No. 34). All of the measuring equipment was remotely controlled from the control room in a shielded location about 30 feet away.

Families of curves over the voltage range of 500- to 1,400-kV constant potential were obtained for both broad beam and narrow beam conditions and different thicknesses of lead and concrete. As observed in earlier experiences, the attenuation of the narrow beam was always substantially greater than that of the broad beam. For example, for a 1,400-kV broad beam through 60 mm of lead, the roentgens per minute per milliampere was 0.7 at 1 meter below the slab and only 0.3 for the narrow beam. For a 600-kV radiation through 20 mm of lead, the corresponding values were 6 and 4. In general terms, the extra thickness of lead required for broad beam protection conditions over narrow beam protection conditions varied from approximately 10 percent at 500 kV to 25 percent at 1,400 kV.

Similar curves were obtained for concrete using beam diameters at the point of incidence of about 1, 13, 26, and 37 inches. There was a progressive decrease in the attenuation as the beam diameter was increased, but the difference between the 26- and 37-inch beam was relatively small.

For concrete protective barriers, the difference between narrow and broad beam thickness requirements was found to be on the order of 1 1/2 to 2 half-value layers* depending upon

*Half-value-layer (HVL) is the thickness of a material that will attenuate a radiation beam to one-half its incident value.

the portion and the kilovoltage of the curves considered (Ref. 95).

As a logical consequence of these studies Braestrup and Wyckoff later prepared a definitive paper on the protection requirements of 1- and 2-million-volt x-ray installations (Ref. 94). These studies were of special value to the NCRP and subsequently to the ICRP in developing protection recommendations.

Having worked out the techniques for attenuation measurements for broad and narrow x-ray beams, Wyckoff and Kennedy completed the studies by obtaining similar data for concrete and lead using cobalt-60 as the radiation source (Ref. 120). For the experimental setup, they used large transformer pits in the floor of the high-voltage laboratory. The ionization chamber--a bakelite cylinder some 10 cm in diameter and 10 cm in length--was mounted on a long extension arm and track projected from a metal tower which permitted measurement distances up to 7 meters above the radioactive sources. Similar studies were made for the gamma rays from radium (Ref. 107). For both sources, curves and data were developed and presented in a form useful for radiation shielding design.
Attenuation studies made for x-ray beams at 90° to the electron beam showed very little difference from the 0° beam and were not published.

One of the final studies relating to the protective properties of structural materials for high energies was an evaluation of the attenuation of gamma rays at oblique incidence on protective barriers. Because of the high cost of shielding, it had become increasingly important to take advantage of any design characteristics which would provide the proper protection, but at a reduced cost. Qualitative predictions suggested that, for a given slant thickness, one should expect for thin barriers a decrease in transmission for increasing angles of incidence and, conversely for thick barriers, an increase in transmission for increasing angles of incidence.

Radiations used for these experiments were gamma rays from cobalt-60, cesium-137, and gold-198, and the shielding materials were lead and concrete. At angles of incidence of 0°, 50°, 60°, and 70°, the results showed that for larger oblique angles the Compton scattered radiation may have shorter path lengths through the barrier. This obliquity effect must be considered in designing barrier requirements. For diverging cones of radiation the effect was not too significant for attenuation less than 100. It was also shown that in the range of energies below 0.7 MeV, the addition of a layer of lead behind a low atomic number barrier could be effective in reducing the thickness requirements of the barrier.

While these studies were in progress, there was another series of theoretical investigations being carried out by U. Fano and his coworkers. Over a period of several years, his group did a series of theoretical studies on the penetration and diffusion of hard x-rays through thick barriers. These studies will be covered in separate discussions of the theoretical programs (see p. 308). They were of interest to the experimental program and there were cross ties between them. Fano also supervised some experimental studies of his own design to check the theories. These had a direct relationship to protective barriers but were not done under barrier simulating conditions.

Ionization of Liquids

There had always appeared to be some logic to the idea that, since the process of radiation interaction with biological materials almost certainly resulted from an ionization process, measurements of the ionization in liquids should provide a useful approach to understanding problems of radiation dosimetry. Stahel in Belgium had made some such proposal but had not appeared to follow through on it. The same idea occurred to Taylor and Mohler in the early 1930's and they set out to do some exploratory studies of the process.

For preliminary study, carbon disulfide (CS_2) was selected, primarily because it could easily be obtained in pure form and had a very high resistivity. The first ionization chamber consisted of aluminium disks within a sealed glass vessel into which the carbon disulfide could be distilled. A plate separation of 1 mm or less was used and field strengths across the plates went up to the normal maximum of about 60 kV/cm.

Because of the dense ionization and high recombination of ions in a liquid, it was impossible to obtain ordinary saturation conditions. However, as early as 1908 Jaffé had studied columnar ionization and recombination, and developed a method for measuring the total ionization. By plotting the reciprocal of the current against the reciprocal of the field strength, Mohler and Taylor found that for fields above 20 kV/cm, the line became straight and could be extrapolated to the equivalent of an infinite field strength, thus yielding a value for the reciprocal current which would apply to the particular conditions. Comparison gave the ionization per unit volume in liquid carbon disulfide as 2,600 times that of air under standard conditions. The absorption was 1,910 times that of air and the energy of ionization per ion pair was about 0.75 times that of air (Ref. 39, see also Jaffé's original paper, 1908). An analysis of the data also indicated that the number of ions produced in the liquid by the absorption of a quantum is about the same as that in a gas, and that the current resulting from the ionization is greatly reduced by intercolumnar recombination. Mohler made some comparisons of the results with those obtained by Wyckoff and Rivers leading to a convenient expression for the bactericidal effectiveness of columns of ionization, whether or not one took the view that killing depended upon striking a certain spot in the cell (Ref. 40).

In an effort to find the best measurement system and interpretation of the results, more detailed studies were made by Taylor under a variety of conditions to assess the possible value of liquid ionization measurements for biological purposes. The aim of the studies was to investigate the use of liquids more suitable to biological application than carbon disulfide. Such liquids should have the following properties: low conductivity when not exposed to ionizing radiation, a relatively large increase in conductivity when exposed to ionizing radiation, an effective atomic number of about that of water (7.5), low viscosity, a density of about one, and low volatility. Of the various other liquids tested, ligroin and tetrahydronaphthalene (tetralin) were most suitable. Their main disadvantage lay in showing a relatively small increase in conductivity as compared with CS_2 when exposed to x rays, and it was for this reason that the CS_2 was used in much of the exploratory work. It gave large ionization currents and was relatively easily purified; however, the dark current was about 10 times that of ligroin. In earlier experiments by Jaffé and others, it had been thought that purer liquids were not suitable for studying columnar ionization. Later, however, they decided that pure liquids could be used. The resistivity in the Bureau studies was on the order of 10^{14} ohm-meters.

Several different types of ionization chambers were used. Some were in the form of grids, made of very fine aluminium wires strung across a quartz plate with a hole through the center (see photo No. 35). This gave an ionization volume 15 mm in diameter and 1 mm thick. Use of the wire grids minimized wall absorption and scattering. Other chambers were made of very thin aluminium plates, separated by quartz spacers and perforated just enough to insure that there were no gas bubbles trapped in the ionized volume.

One of the things noted in the earlier studies was that the conductivity of carbon disulfide, when exposed to x rays, varied with the time of application of voltage and the time of exposure. Since it was suspected that this might be due to the buildup of some kind of polarized or barrier layer, extensive studies of the phenomenon were undertaken (Ref. 52).

For example, in working with an ionization chamber exposed to x rays, the current rose almost instantly to a maximum with the first application of voltage and then decreased to an essentially steady value in about 10 minutes. This was repeated after each increase in voltage, but when the voltage was decreased by steps, the opposite occurred; the current started at a low value and slowly rose to a final steady value. Thus, to obtain the same steady state current after increasing the voltage, a lapse of 20 or 30 minutes was required between the change in voltage and the final measurement. This, of course, made meaningful measurements very uncertain.

To be certain that the space charge had no effect other than to effectively decrease the average field strength in the cell, direct current measurements of the ionization were made at times as low as 0.005 seconds, before any space charge could build up. To accomplish this, an electromechanical square-wave generator was used to apply the voltage to the ionization chamber. It utilized a variable speed commutator system and high-vacuum switches that could operate up to 10 kV and thus provide a basic waveform consisting of four quarter-portions. The first quarter would ground the high-voltage plate, the second quarter would apply a negative voltage, the third quarter again grounded the plate, and the fourth quarter would apply a positive voltage. The frequency could be varied from about 2 to 2000 cycles per minute.

The ionization current measuring system was controlled by a commutator on the same drive shaft that permitted current measurements at any position in the cycle from 0.005 seconds after a voltage change up to a delayed period on the order of 6 seconds. Using this system it was possible to make consistent ionization measurements before any barrier charge had developed. The measurements could also be made quickly because it was not necessary, as before, to wait many minutes after each change of voltage for ionization current to reach a steady state. Thus, using the inverse current extrapolation technique, it was possible to make meaningful measurements which promised to be of value in relationship to ionization phenomena in biological materials.

The work reported above was all done prior to about 1936. At this point, new developments and programs taking place in the x-ray field diverted attention from this project. The work was resumed after the war, but NBS' increasing involvements with the atomic energy age made it impossible for Taylor to personally continue the work. An assistant was assigned to extend the project by taking advantage of some of the new electronic developments that had taken place during the war. Unfortunately this effort proved to be abortive, and the program was dropped, never to be resumed.

Lenard Rays

Lenard rays are electrons produced by high-voltage acceleration in a vacuum and passed through a low-absorption window into free air. These are named for Lenard who discovered them in the 1890's. In comparison with x rays, Lenard rays are very quickly absorbed in tissue. Hence these rays had useful dermatological applications as well as possible industrial applications. In 1926, Coolidge described a high-voltage hot-cathode tube of the Lenard type which directed high-speed electrons through a thin (0.0015 in) metal window into open air (Coolidge, 1926) (see photo No. 36). He later constructed a three-section cascade type tube, operating at 900-kV peak and 2 mA on an induction coil. In 1929, C. M. Slack developed a similar tube with a concave glass window about 1 micrometer thick which operated up to at least 350 kV in a single stage.

Though such sources of radiation had been used in only one or two experimental situations, it was anticipated that they would come into more general use. Taylor, therefore, devised a system for measuring the Lenard rays under conditions which could apply to biomedical applications. To assist him in making such measurements, both Coolidge and Slack provided samples of their one-stage Lenard-ray tubes.*

*The Coolidge tube operated on pumps while Slack's was a sealed-off construction.

Because these tubes operated on an available end-grounded high-voltage source, Taylor had made some measurements of Lenard rays and reported on them in 1928. This was done as a side study during the construction of the constant potential source for x-ray standardization which, after all, was the primary objective of the program at that time (see photo No. 37).

Since cathode rays are simply negatively charged particles (electrons), they could be measured to a rough approximation under defined conditions by a small plate placed outside the Lenard-ray tube. This was done using a flat circular plate about 1 cm in diameter surrounded by a 6-cm guard plate in the same plane. While there were a number of known or suspected sources of error, such measurements were believed to be reasonably meaningful. This led Taylor to propose that "A unit cathode ray beam may be described as that having an electron current density of 1 esu per square centimeter normal to the direction of the beam, and be designated by the symbol "L"." This preliminary work gave way to other priorities and was not resumed until about 1931.

At that time it was decided to try to avoid some of the uncertainties rather than correct for them. Thus a Faraday-type collector that would subtend a definable portion of the electron beam was developed and tested. The shielded Faraday collector was arranged with a 1-cm diameter opening and a shield inside which could be moved back to increase the volume of the collector. In the center was a flat stem which, when projected forward, would enter the 1-cm opening with a narrow gap around it. Measurements made with the stem and volume in different positions and at different distances from the window of the Lenard-ray tube permitted an evaluation of the effects of electron scattering or electron loss (Ref. 20). Though it was concluded that measurements of this type would be suitable for biomedical application, the need to apply the technique never really materialized.

One of the first ideas for the measurement of Lenard rays in air was by means of a simple ionization chamber allowing the passage of an electron beam between two plates. However, because of the dense ionization, it was impossible to saturate such chambers and the method was abandoned. Later studies on the dense ionization in liquids led to the application of Jaffé's theory of columnar ionization to the measurement of electrons. A number of different chambers were tried but one, following the general design of the guarded field, free-air chamber for measuring grenz rays, proved to yield the most meaningful results. Field strengths as high as 9 kV/cm were used in simple parallel-plate ionization chambers without reaching a real plateau in the ionization current. Using this small free-air chamber with a 2.9-mm beam and a field strength of only 130 V/cm, the maximum ionization current was about 28 percent of the saturation value (Ref. 51).

By plotting the reciprocal current against the reciprocal voltage, the data yielded essentially straight lines which could be extrapolated to zero value of the reciprocal voltage, thus yielding a saturation value. There appeared to be various theoretical reasons why this should not work, especially because Jaffé's theory of columnar ionization was not considered to be valid when there was a serious overlap of the columns and intercolumnar recombination. However, the fact that the reciprocal current-voltage relationship was found to be linear at high field strength, regardless of the sign of the potential on the plate, lent strength to the application of the principle for obtaining the real saturation current by extrapolation methods.

While this question was not pursued, it was found later that very intense x rays in the form of a very narrow beam could be measured directly in a free-air chamber at reasonable field strengths. Larger beams could also be measured in the same chamber by means of the reciprocal current-voltage relationship and extrapolation. Theoretical reasons for this have not been established (Ref. 124).

Calorimeter

As mentioned above, one of the programs that Taylor proposed in 1927 was that of measuring the actual energy in an x-ray beam by means of some kind of calorimeter (see p. 31). One such unit was developed consisting of a hollow conical receiver made of a series of lead rings about 1/8 inch thick which permitted radiation to enter the hollowed cone at the large end and be almost completely entrapped and absorbed in the lead. Each lead ring was insulated from the next by a single layer of lacquer. A thermocouple consisting of very fine wires, so as to minimize loss due to heat conduction, was embedded in the periphery of each ring. The cold junctions for the thermocouples were of course located outside in a controlled temperature bath. Some dozen such thermocouples were connected in series to a

special bridge circuit developed just for the purpose by H. B. Brooks of the Electrical Division. The whole system was held on ivory pins inside of a heavy brass well designed to hold the temperature fairly even.

It was soon found that for the ordinary beam intensities available outside of the lead room, the system was not sufficiently sensitive. However, when placed in an insulated box very close to the x-ray tube, there was enough energy absorption to show a noticeable temperature drift.

Taylor, on his trip to Europe in 1928, had visited the laboratory of Dr. W. Rump, who had published a recent paper describing an air-bubble calorimeter which he used in a study of the efficiency of x-ray production (Rump, 1927). Though suitable for this application, the calorimeter did not appear to be practical for Bureau purposes and so the whole project was discontinued.

Along with the new technologies of the forties and early fifties, very sensitive thermistors were developed. By using these developments, Steve Domen, J. Wyckoff, and others of NBS ultimately made successful calorimeters for measuring the very high energy radiations produced by the betatrons (Refs. 328 and 335).

Efficiency of Production of X Rays

Soon after joining the Bureau's staff in 1927, Taylor realized that the x-ray program was handicapped by the lack of a strong theoretical physics capability. Thus, toward the end of the first year, he recruited Dr. W. W. Nicholas, of Cornell, who had held a 2-year National Research Council Fellowship and had worked on the theory of the continuous spectrum of x rays from thin targets.

Nicholas was imaginative in theory and skillful as an experimenter. In outlining his work program, he was given almost complete freedom in his choice of work as long as it involved the general x-ray field and had some reasonable connection with problems of radiation measurement, dosimetry, or protection. Mr. C. G. Malmberg, then a sub-professional laboratory assistant, was assigned to work with Nicholas, and a junior professional, Mr. C. F. Stoneburner, was brought in to work with Taylor and Singer.

While at the Bureau, Nicholas completed and published two very creditable papers on work started at Cornell on continuous spectrum x rays from thin targets and on the efficiency of production of x rays (Refs. 5 and 16). He later studied the phenomena of discharges in gases. Although he never developed any particular interest in, or made any helpful contributions to, the field of radiation dosimetry, he was a valuable man to have on the staff. In 1932, he sought permission to write a book on radiation biology, despite the fact that he had had no experience in the field other than some side reading. When the Director refused such permission as being too far afield from the Bureau's mission, Nicholas resigned and Malmberg was transferred to another Division in the Bureau.

Skin Erythema

By 1927, proposals had been made for what was called a "tolerance dose," the amount of radiation that could be tolerated by radiation workers without injury. This applied generally to physicians, nurses, technicians, and those working in industry. The difficulty was that the tolerance dose was expressed in terms of threshold skin erythema and there was not yet an agreement as to how many roentgens it would take to produce an erythema under defined conditions. This was a very pressing problem. Because agreement on the magnitude of the roentgen was near, it was critical to put the tolerance dose situation on a more quantitative basis (see p. 30).

With this in mind, Taylor tentatively arranged to conduct a program at the Bureau designed to establish a better relationship between a threshold skin erythema and the dose measured in roentgens. Because this would involve biomedical experiments outside the area of recognized NBS competence, Taylor secured the collaboration of the Public Health Service physician assigned to NBS at that time. The general plan was to place an individual on a rolling table which could be pushed under one of the large lead boxes containing a 200-kV x-ray tube. By means of an adjustable opening in the bottom, a field of x rays about 2 inches across could be applied to the mid thigh of the experimental person. Three or four exposures were made on Taylor at levels which seemed fairly high but which did not show any signs of an erythema. Concern about going to higher doses led to a reassessment of the situation, and to the conclusion that the field was too small to provide the necessary level of backscattered radiation. As it turned out, Küstner of Germany was completing a similar study under clinical conditions which involved considerable numbers of people. His paper on

the subject was ultimately accepted as definitive and became the baseline for the evaluation of a skin erythema in terms of roentgens (Taylor, 1979, 3-009).

Even after Küstner's evaluation of the threshold erythema dose in terms of roentgens, there was still substantial uncertainty as to the validity of his finding as well as those of others (Küstner, 1927). The literature of that period contained many reports on similar studies but none were as sound as Küstner's.

One study by Otto Glasser involved a survey of a number of institutions around the country where reasonably satisfactory dosimetry was being practiced. Part of the problem hinged upon the confidence in the standardization measurements available at that time. Other uncertainties were due to the wide range of parameters used in the different studies. Glasser presented a paper on his study at one of the RSNA meetings (Glasser, 1930). In discussing this paper, Taylor reported on the status of the x-ray standardization agreements between the Bureau of Standards and other laboratories. Failla questioned the techniques used by Glasser, especially the influence of fractionating the total dose that was described as resulting in a threshold erythema. Following is a portion of the discussion of Glasser's paper:

"LAURISTON S. TAYLOR (Washington, D.C.): In Dr. Glasser's paper he mentioned the comparison by Dr. Behnken of the standard chambers of the Reichsanstalt, in Berlin, and those of Dr. Glasser and Prof. Duane. At the instigation of radiologists, the Bureau of Standards several years ago commenced the problem of setting up a standard ionization chamber, and within the last month checked its standard unit against that established by Dr. Glasser some years ago. I tried to make such a comparison last spring with the laboratory of Prof. Duane, but unfortunately the portable instrument carried to Cambridge was damaged in transit back to Washington and the work was of no avail, thereby delaying for nearly six months the actual calibration of instruments by the Bureau. To avoid this difficulty a second time, we had Dr. Glasser come to the Bureau with two instruments, the Victoreen and the condenser dosimeter, both of which previously had been calibrated at the Cleveland Clinic. We worked at two voltages at the Bureau--140 and 150 kilovolts--and two filtrations, 2 mm. and 4 mm. of aluminum. We chose these two particular sets of conditions because at such operating factors, the wall correction for the small chambers would be comparatively large, so that if we obtained a fairly close agreement at these values, the indication would be that the agreement should be at least as close at higher voltages. I will say only that, repeating runs in the morning and afternoon of the same day, making, in all, I believe, six separate calibrations, the average agreement was about 2 percent. I do not recall which unit was the larger. There was no reason to believe, in advance, that there would be any particularly large difference between the Bureau of Standards unit and that of Dr. Glasser. However, we thought it necessary to make such a calibration, to make sure that there were no errors before we proceeded with the calibration of outside instruments. Beginning with the first of the year (1930), the Bureau will be in position to calibrate any ionometers or dosage meters sent in which have with them some means of calibration control. It will be perhaps dangerous and misleading to calibrate any instruments which do not have a radium or a uranium oxide control for checking the calibration before and after transit, inasmuch as there is a possibility of damage occurring to the electrometer system which could not be otherwise detected. In brief, any such instrument sent in after the first of the year will be calibrated in terms of the international r-unit, and we hope that this work will prove of sufficient value so that the various institutions may avail themselves of our assistance.

"M. M. SCHWARZSCHILD (New York): We have been using 4,000 r-units for some time, with a filtration of 2 millimeters of copper and 200 K.V., constant potential. The dose is given in a short time, usually in seven days, over a series of portals, so that the total dose reaching the tissue is given in about twenty-one days. The dose of any portal, the total 4,000 r-units, is usually given in the course of one week. The mucous membrane reaction begins on the twelfth day, reaches its maximum about the twentieth day, and is completely healed at the end of the twenty-fifth day. The skin reaction starts on the twenty-second day, reaches its maximum on the thirtieth day, and heals on the fortieth day. The skin reaction is rather unusual, there being complete exfoliation and complete re-healing new skin being formed over the portion radiated. The dose was measured with three different instruments, a Victoreen, a Mecapion, and a Siemens dosimeter. The three meters gave approximately the same results, 4,000 r for the

complete skin dose. The dose was given with heavy filtration, 2 millimeters of copper, and high voltage--an effective wave length of about 0.11 Angstroms.

"G. FAILLA (New York): When I received Dr. Glasser's questionnaire, I was somewhat surprised to find that it contained no direct request for the specification of voltage and filter in connection with the statement of the erythema "dose" in r-units, which the radiologist was expected to make. Nor was there any reference to the time interval during which the total dose was administered. I am not surprised, therefore, to find an extreme variation of "doses" from 250 to 4,000 r-units in the answers received. The results of this questionnaire bring out more clearly than I have ever been able to do at previous meetings, the pitfalls of the indiscriminate use of the r-unit.

"The confusion which is evidenced by the results of the questionnaire has arisen from the fact that too much emphasis has been put on the quantity factor of the dose of radiation. The quantity factor is the only thing which is measured in r-units, but this alone by no means defines the dose. For instance, an erythema may be produced under certain conditions by the administration of 1,200 r-units at one sitting. The same degree of erythema so far as external appearance is concerned, may be produced by 2,400 r-units, administered under the same conditions as above, but in fractional treatments extending over a period of several weeks. From this example it is perfectly evident that the erythema dose specified only as 1,200 or 2,400 r-units is meaningless. If other important factors are not stated, one cannot form an estimate of the biological effect which a certain amount of radiation, stated in r-units, might be expected to produce. Thus in the above example the administration of 2,400 r-units would lead to a severe skin reaction if given at one sitting, but produces a slight erythema if the same amount is spread over a sufficient length of time.

"In order to avoid any possible misunderstanding of my remarks I wish to state that I am not opposed to the use of the r-unit but to its misuse. Every radiologist should express the quantity of radiation administered to a patient in r-units, but with a full appreciation of the fact that this alone does not specify the dose of radiation. The notion that an erythema dose or any other dose can be expressed in r-units is erroneous, but is rather common. To a large extent this is due to the usual practice of calling physical instruments which measure the intensity of the quantity of radiation "dosimeters" or "dosage meters." I would suggest, therefore, that in the future such instruments be called ionometers or ionoradiometers, and thus avoid the implication that by the reading of an instrument of this sort a radiologist can express the dose of radiation administered to a patient.

"OTTO GLASSER (Closing): In regard to Dr. Failla's question, I would like to call attention to No. 6 in our questionnaire, "Do you measure these doses for various conditions of radiation?" All of the answers on the questionnaire gave definite data on the radiation quality used. We would not have used them if they had not contained this information. In several places in our paper we called attention to the fact that this is absolutely essential."

Combining X-Ray Programs

In about 1934, while the country was still in the Depression, there developed an awareness that several different laboratories in the Bureau of Standards were engaged in some aspect of x-ray crystal structure analysis, as were other Government laboratories (including the Naval Research Laboratory and the Department of Agriculture's Fixed Nitrogen Laboratory on the grounds of the American University). It was believed by some that effective savings could be realized if these programs were consolidated and attached to the x-ray measurement programs underway at the Bureau primarily for medical applications.

The Depression economy actually resulted in the discontinuance of the Fixed Nitrogen Laboratory under Dr. Cottrel. Because of this action, Dr. Sterling Hendricks became available and was temporarily transferred to the Bureau's Atomic Physics, Radium, and X-Rays Section to work with Taylor. His primary work was on a study of the possibility of consolidating the x-ray and the crystal structure programs.

Hendricks spent several months examining the proposed consolidation of the programs at the Bureau and the other Government laboratories. His final conclusion was that no attempt should be made to combine any of the laboratories. The problems involving crystal structure

were different in every case, and the value of the experimental procedures depended entirely upon the outlook and the applications of the individual scientists in relation to their basic programs. For example, one program at the Bureau was in the chemistry laboratory, another in the ceramics laboratory, and a third in the plastics laboratory. There was no chance of finding an individual who was adequately trained in all three or even two such fields, and interested in such a multidiscipline approach. As Hendricks remarked at the time, to consolidate these activities would be the same as assigning everybody to the carpenters shop because he used a hammer.

In any case, the concept was abandoned, although the study by Hendricks was to be of considerable future value. Meanwhile, Hendricks relocated in another part of the Department of Agriculture where he did outstanding work until his retirement many years later*

*Renewed interest in program consolidation developed during the later 1970's. By that time, x-ray and other ionizing radiation programs were being carried out in nearly every Government agency. There were great cries about waste and duplication. Hence, steps towards consolidation of key responsibilities were proposed and are being tried out at this time.

Undersaturation of Thimble Ionization Chambers

In the normal studies of the x-ray group, small but important problems would frequently develop. One such problem was the discovery that thimble ionization chambers with volumes on the order of five-tenths to three cubic centimeters would, when exposed to very high levels of radiation, be subject to loss of ionization current due to a lack of saturation. Most of these small ionization chambers operated at field strengths of 50 V/cm or less, and they were designed for measuring dose rates on the order of 50 to 100 roentgens per minute. Because some of them had cylindrical symmetry, field strengths varied considerably. Voltage saturation characteristics for these conditions had been adequately studied. However, if the same chamber were exposed to radiation from a rotating-target diagnostic x-ray tube, operating at a peak current of 1,000 mA for one to a few cycles, the saturation conditions were quite different. Instantaneous intensities of the beam could easily be on the order of thousands of roentgens per minute at peak values. Under these circumstances the ionization would be so dense that extensive recombination of the ions would occur before they reached the collecting electrodes. This situation would be worse if such a chamber were used to measure radiation from a betatron in which the radiation pulses are very narrow and very high. It was found that under some conditions, saturation voltages could not be reached before the occurrence of an electrical breakdown between the outer shell and the collecting electrode (see p. 173).

(It has already been noted how the use of very small diameter beams would solve this problem to some extent but this technique was not applicable to the thimble chamber.)

As one means of circumventing the difficulty, Taylor and Day did some experimental work on so-called vacuum chambers. The vacuum chamber was essentially a two-electrode chamber operated at modest voltages and under moderately good vacuum conditions so as to prevent a gas discharge between the electrodes.

In operation, the electrons ejected from one electrode were transferred to the other and measured. Depending upon the asymmetry of the field, the direction of the ejection, and the plate geometry, differential currents of conveniently measurable magnitudes were yielded. The operational characteristic of such a chamber depended upon the polarity. When a positive potential of some 20 to 30 volts was applied to the outer electrode, suitable operating currents resulted. The approach seemed profitable, but because of other project needs, the research was temporarily discontinued. When Day shortly afterwards left the Bureau of Standards, the project was never revived (Ref. 124).

Shoe-fitting Fluoroscopes

In early 1940, a request was received for information about so-called "Foot-o-scopes," the shoe-fitting fluoroscopes that were found in many shoe stores. Because of this inquiry, George Singer made informal arrangements with several shoe stores in the Washington area to make radiation measurements in and about such x-ray machines. Radiation levels at the position of the foot were relatively high, but much lower at the trunk of a child's body. Scattered radiation in the general area of the machines was variable. Though exposures of the trunk were low, it was felt that there was no justifiable reason for having any exposure at all. Such exposures showed only that there was a foot in a shoe and provided no useful

information to the purchaser. This opinion was supported by an orthopedic physician and a podiatrist.

As a result of these surveys and discussions with the shoe managers, the use of the equipment was discontinued in every case. The matter was later referred to the Advisory Committee on X-Ray and Radium Protection and to the American Medical Association, which had expressed interest in the problem.

Perturbations in the General Radiation Programs (1940-1948)

Because of the many disturbances, new influences, new developments, and new pressures during this period, it is appropriate to present a brief "State of the Union" summary centering about the Radiation Physics Program at the Bureau of Standards. Some of the organizational factors which were to play important roles in the future of the radiation programs will also be discussed.

In the early fall of 1940, Taylor was asked to organize a research and development program on proximity fuzes for bombs and rockets. It was the first such Bureau program to be organized and he was given carte blanche to draw anybody he needed from the Bureau's staff. His first choice was Dr. Allen V. Astin, who was later to become Director. Some outstanding staff members, such as J. L. Thomas from the Electrical Division, H. F. Stimson from the Heat Division, T. B. Godfrey from the Heat Division, and F. L. Mohler from the Optics Division, were also selected. The bulk of the space adjacent to the main bay of the new High-Voltage Laboratory was converted to various phases of the fuze program. For all practical purposes, Taylor, although available for consultation, played no active role in the radiation programs until after the war. George Singer, who had joined the staff in 1927, took charge and was assisted by Dr. H. O. Wyckoff, who had joined the staff in 1941, and A. L. Charleton.

In the spring of 1943, Taylor withdrew from the Bureau's fuze program to organize Operations Research Sections for the Eighth Fighter Command and later for the Ninth Air Force in Europe. After VE Day, he returned to Washington where he served for a few months as Director of Operations Research for the Continental Air Command to insure its proper organization and continuity.

Meanwhile the x-ray programs under Singer were principally concerned with providing the Army with much needed assistance in the design and specification of field x-ray equipment and working with the American Standards Association on the development of an industrial safety code for the use of x rays, especially in military applications. Because of the rapidly expanding needs for operations research studies in the Ninth Air Force, Taylor returned to the country in September 1943 and recruited H. O. Wyckoff, F. L. Mohler, and Frank Manov from the Bureau.

As already noted, Dr. Fred Mohler, Chief of the Section on Atomic Physics, Radium, and X Rays, gave up his normal research to work with Taylor on proximity fuzes. After the war, Mohler undertook some much needed research on mass spectrometry in a new section under that name. In 1940, L. F. Curtiss was split off and made head of his own Section on Radioactivity. Also in the early 1940's, Dr. C. A. Skinner, Chief of the Optics Division, retired and was replaced by F. H. (Sugar) Bates.

Immediately after the war, the Director of the Bureau, Dr. Lyman Briggs, retired, and was succeeded by Edward U. Condon, a theoretical physicist. Condon, the first Director to be appointed from outside of the Bureau organization, had considerable influence on the new directions of the radiation physics program. For a broader insight to the changes that were developing, see "Measures for Progress--A History of the NBS," Chapter VIII (Cochrane, 1966).

While still working for the Continental Air Command at Bolling Field, Taylor started informally to replan the Bureau's x-ray programs. By this time Frank Day had been a staff member since 1942, and Harold Wyckoff rejoined the group in the summer of 1945. Before Condon officially assumed his new duties as Director, Dr. Briggs and Taylor agreed on an immediate plan to expand the radiation programs, including the acquisition of two betatrons, one for 50 MeV and the other for 100 MeV. Also, at this time, Bates retired as Chief of the Optics Division and was replaced on an acting basis by E. C. Crittenden, with Taylor as his Assistant Division Chief. After assuming the Directorship, Condon soon decided to reorganize the Optics Division to correct the disjointed conditions resulting from recent war activities. Following some shifts of existing Sections and the addition of new ones, Condon, himself, took the position of Chief of the new Atomic Physics Division. Dr. R. D. Huntoon, who had been active in the ordnance programs during the war, became his Assistant Division Chief. Condon's presence in the Atomic Physics Division was extremely valuable,

but his duties as Director soon made it impractical for him to play a continuing leadership role in the Division.

After a couple of years, Dr. Condon gave up the Atomic Physics Division and Huntoon became its new Chief, with Taylor again as the Assistant Chief. Shortly thereafter, as a part of the Bureau's expansion, Huntoon was made Director of a new branch of the Bureau of Standards, the Corona Laboratories in California, and Taylor took his place. By this time, extensive reorganization, regroupings, and program expansions had taken place.

With the Director's encouragement in 1946, important changes were initiated in the nature and direction of the Bureau's radiation and research programs. A new opportunity to strengthen the theoretical physics capability arose in 1946, and Dr. Ugo Fano, who had worked at the Coldspring Harbor Marine Biological Laboratory, was brought in to organize some theoretical programs. He had interest and experience in the field of radiobiology, and was given carte blanche to organize appropriate radiation oriented programs. There was no stipulation that these had to be directly related to radiation protection or dosimetry, which had been and still were the backbone programs of the Section. Although he engaged in a wide range of basic activities, he also took a strong interest in, and made important contributions to, the radiological programs. In 1948, Fano recruited Dr. L. V. Spencer, who is still one of the theoretical research leaders in the Radiation Physics Division.

As a result of the wartime experience with military x-ray equipment, intensive postwar activities continued primarily through the Veterans Administration. Because their radiation facilities had become inadequate and largely outdated, the Government was about to embark upon an extensive purchasing program. When the Bureau was asked to assist in the design, specification, and testing of the replacement equipment, Dr. Scott W. Smith, who had research experience with one of the x-ray companies, was brought in to head up this work. Smith recruited Robert Brueckmann, a physicist with electronic interests, and L. Dobak, who had many years of experience as an x-ray equipment service representative.

At this point, Wyckoff, in early efforts to reestablish the programs in radiation standards, dosimetry, and protection, recruited four individuals to work with him: W. L. Edwards, H. F. Gibson, R. J. Kennedy, and George Kamm.

Other perturbations were to occur. In early 1946, George Singer died suddenly of a heart attack and Wyckoff assumed his responsibilities. Also, the x-ray group within the Section of Atomic Physics, Radium, and X Rays became a separate Section with Taylor as Chief. Then in 1947, Taylor's services were requested by the new Atomic Energy Commission. Arrangements were made for Taylor's temporary transfer to the Atomic Energy Commission, where his responsibility was to organize a new branch of Biophysics within the Division of Biology and Medicine under the direction of Dr. Shields Warren. The arrangement was for a 1-year tour with an option to make it permanent if Taylor so wished. However, Taylor, in commuting each day to AEC on Constitution Avenue, would stop by the Bureau of Standards from time to time and, thus, continued to play an informal role in the program developments within the X-Ray Section. He returned to the Bureau on a full-time basis in September 1948.

By the end of 1948, the X-Ray Section had grown to a staff of 12 professionals and 4 or 5 sub-professionals and clerical assistants (Section 10 of Division IV, Atomic Physics).

During this same period of reorganization and growth, there were factors outside of the Bureau which were to have important influences on the direction of the radiation program. One of these was the reorganization, in 1946, of the informal Advisory Committee on X-ray and Radium Protection, and its expansion into the new, more formalized, National Committee on Radiation Protection (and, later, Measurements). Taylor was Chairman of the new committee and Wyckoff, Smith, and Fano all had important roles in its subcommittee activities (Taylor, 1979, 7-001 to 7-204).

Changes in the radiological organization had also taken place. The Standardization Committee of the Radiological Society of North America and the American Roentgen Ray Society, of which Taylor was Chairman, had been discontinued and reorganized within the American College of Radiology under the designation of Commission on Radiological Units, Standards and Protection (CRUSP). As a commission of the college, the Chairman was required to be a physician, so Taylor became the Vice Chairman. This grouping gradually moved away from the kind of guidance the Bureau had sought in the past (see ch. 15).

Also during the war years, the Registry of X-Ray Physicists, which had been a part of the joint standardization committee activities, was shifted into a section of the American Board of Radiology. The Radiological Society of North America, not wanting to give up its position and prestige in the measurement field, organized a new committee known as the Physics Committee. This was initially under Taylor's Chairmanship, and later under Wyckoff's (see ch. 17).

Other changes were the result of the greatly increased research at the Bureau sponsored by the Atomic Energy Commission, the Department of Defense, the Veterans Administration, and other agencies, substantial portions of which were of interest to the X-Ray Section. With these added programs came increased administrative responsibilities and complexity. Along with these factors came the first signs of the decline of useful output per employee as involvement with management, administration, red tape, and paper work increased.

AEC Division of Biology and Medicine

Soon after the passage of the Atomic Energy Act of 1946, the Atomic Energy Commission, then located in the old Public Health Service building on Constitution Avenue, started organizing its operating divisions, one of which had three branches--Biology, Medicine, and Biophysics--under Dr. Shields Warren. As mentioned earlier, Taylor was asked in mid-1947 to join the Commission and organize the Biophysics Branch. Even though the request came at the very time that the Bureau had obtained authority and funds to build up its own x-ray programs, Taylor agreed to a temporary tour of a year with AEC to get their program organized.

During his year there, the Health Physics Training Programs at Oak Ridge, Rochester, and Hanford were established and initial implementation took place. Also established and begun was "Project Gabriel"--then a very highly classified investigation on the extent and effects of the worldwide distribution of strontium-90 resulting from weapons testing. These programs were completed about a year later. Long-range support for certain radiation programs was also instituted. One such example was the study of radium workers by Prof. Robley Evans at MIT. Having initiated these programs, and having recruited a replacement for himself, Taylor returned to the Bureau of Standards in the fall of 1948.

Radiological Instrumentation

In about 1949, Taylor received a phone call from the Department of the Army concerning the possibility of the X-Ray Section undertaking a study and testing program for the various kinds of radiation measuring and detecting instruments with which the military had to gain experience. Following subsequent discussions, it was tentatively agreed that the Section would carry out such a program at the financing level of approximately $200,000 a year. Before arrangements were firm, a similar call was received from the Navy's Bureau of Ships concerning the same problem and the same range of financing. The fact that a similar request had come from the Army did not alter the Navy request. Again, before arrangements were completed, a third request came from the Air Force following very much the same pattern. With the three services accounted for, plans got underway for a program to meet the requirements of all three. Then, to the Bureau's surprise, a fourth request came in from the Navy's Bureau of Air which apparently had no closer connections with the Bureau of Ships than it had with the Air Force. So by this time, the project proposals from the four military service organizations were on the order of $800,000, which could have supported other work for which the Bureau had not yet acquired funds.

The situation did not move that way. Instead, the four organizations were invited to a meeting at the Bureau during which the Bureau agreed to carry out a program designed to meet the needs of all the Services for a total of about $250,000. The results of work submitted by any Service would be available to all. This not only meant enormous savings in time and money, but for the first time, made it possible for each Service to learn about similar activities in the other Services.

The program proved to be very successful also in other respects. Within a very short time, the Instruments Branch of the Atomic Energy Commission and the Federal Civil Defense Administration joined the program. All pertinent Government agencies were thus linked together through the Bureau's program, resulting in very substantial savings of money over the several years the program was in effect.

Operation Greenhouse

In 1949, another request came to the laboratory from the Atomic Energy Commission (Los Alamos) and the Department of Defense for major assistance at some forthcoming nuclear weapons tests to be held on the Eniwetok Atoll in 1951, under the code name "Greenhouse." The desired Bureau assistance had two major components. The first, and by far the most demanding, was that for measuring the intensity and the spectral distribution of the prompt radiation produced during the first few milliseconds of a detonation. The second part was

the development and implementation of a program for film dosimetry in and around test structures and open spaces.

Funding for the total project was on the order of $6 million and the lead time was approximately 2 years. The X-Ray Section agreed to undertake the work and Dr. Harold Wyckoff was designated as the principal project officer. Because the staff of the Section was small at that time, it was estimated that 20 to 30 people must be added quickly to accomplish the mission within the time limits.

One major difficulty was the marginal state of technology of that period since there had never been any real opportunities to test new developments. For the most part, such tests could only occur on the firing site with no opportunity to repeat precisely the same conditions.

For spectral analysis, a series of magnetic Compton-scattering spectrometers were designed and built from scratch, largely under the direction of Dr. J. Motz (see p. 311). These had to be located in heavy underground shelters with collimating tubes from each of the battery of spectrometers directed to the point of bomb detonation. Working in these buried shelters, the staff had to install and test equipment of a complexity that would have taxed their endurance in a fine air-conditioned laboratory. It was necessary to duplicate the spectrometer setup for each of several tests because the equipment was destroyed by the bomb burst within a few hundredths of a second after it had performed its function.

Measurements of the intensity required a battery of radiation detectors, again located in a collimating system pointing to the shot point atop a 1,500-foot tower. This system, designed and built in the United States and then shipped to Eniwetok, consisted of a huge steel frame on which all the collimating tubes were mounted and directed at the blast point. After being positioned 100 yards or so from the shot tower, the entire collimating system was encased in some hundreds of tons of concrete. The radiation detectors were inserted in the back end of the collimators.

Recording the data from both sets of equipment was to be carried out in bomb shelters about 1 mile from the shot point. This required the laying of some hundreds of coaxial cable lines between the detecting and measuring equipment and the shelters.

Let us examine the status of technology at that time. For prompt radiation measurements at such extremely high levels, the application of any known type of ionization chamber was considered to be impractical. However, the scintillation phenomenon in certain materials subjected to ionizing radiation had just been developed and was in various stages of laboratory experimentation by Kallman and others. The original application of scintillation invoked such materials as anthracene and similar organic materials. Though the basic phenomenon was not new, it required extensive exploration into relatively untouched technology. Also needed were means of the detection, amplification, and transmission of the flashes of light from the scintillating crystal. This called for electron-multiplier phototubes; again, a relatively new development requiring tubes and technology which were not yet on the market.

Another need--also a marginal technology--was that of transmitting the signals from the phototube to a recording system 1 mile away. And finally, there was the recording itself, which was to be accomplished by means of a magnetic tape recording. Again, this was a new technology scarcely out of development stages.

The Radio Corporation of America supplied a considerable number of phototubes for the tests, with about one in three tubes actually meeting the special test needs. Because some 400 channels were required for recording the test data, twenty-channel recorders were developed, each equipped with a 4-inch-wide magnetic tape so data recorded simultaneously on the tape could be tied together and thus avoid the difficulties that separate, single-element tapes would have introduced. Since the total duration of the phenomenon under study was only a few milliseconds, the tape speed had to be very high--approximately 2,000 feet per second--to obtain the required analysis of variations within that time. To accomplish this, the tape drums had to be started a few seconds before the shot and accurately timed to reach the maximum necessary speed before running out of tape. This would allow the record to be obtained within the last 200 or 300 feet of the tape which was on drums about 16 inches in diameter. This part of the program was under the direction of Jack Smeltzer (USAF retired) who had been in one of Taylor's Operations Research Sections in the Ninth Air Force.

Because the requirements were beyond the capability of existing equipment, it was fortunate indeed that the Shoup Engineering Company in Chicago, under the guidance of Mr. Alan Shoup, had the courage to undertake the development and production of the required devices. Also needed and developed was equipment that could replay the tapes and extract

the necessary information. Though some of the equipment failed during the tests, enough units worked to provide the information needed by the Weapons Design Groups.

The third task was to develop a new film badge, which would have relatively flat energy response and cover a wide exposure range. These badges were to be used in various model structures for purposes of analyzing radiation shielding of conventional buildings, and in such military equipment as tanks and air-raid shelters. The required film badge was developed by Dr. Margarete Ehrlich. It consisted essentially of a thick molded plastic container surrounded by lead and tin which acted as filters to provide a relatively good energy independent response by the film (Ref. 137). In this connection, the Instrument Shops under Frank Brown performed beyond the call of duty by developing, on short notice, the techniques for molding the plastic containers and forming the filters. The film badge operation was very successful and thousands of badges were used and processed on the Eniwetok Islands during the tests.

The film badge was later used extensively at the Nevada weapons test site to obtain photon dose-distance information. For this purpose, the badges were supported on stakes in a radial pattern in unobstructed terrain around the shot tower.

That such a crash program could be successfully carried out by a conventional Government research organization was due in part to the recent experience of many of the personnel in such wartime R and D programs as proximity fuzes, radar, and operations research. (Today, if such spectral measurements, intensity measurements, etc. are required, one needs only to go to a catalog and buy all the required equipment.) Though the crash program had been disruptive, the NBS laboratory did gain experience that served it well in later Bureau studies. A considerable number of scientists and engineers had been brought into the crash program, many of whom stayed on for some years to work on other programs, and a few who are still there today. However, when the AEC began to explore the possibility of the Bureau having a more permanent and extensive role in its weapon testing program, it was decided that this might be done better by some commercial organization. The Bureau did agree, however, to continue some telemetry programs of radiation measurements for subsequent testing that took place in Nevada at the weapons test site. Within a short time, Louis Costrell had not only devised such systems to work over a few miles, but such that the recording could be done by telephone lines in Washington.

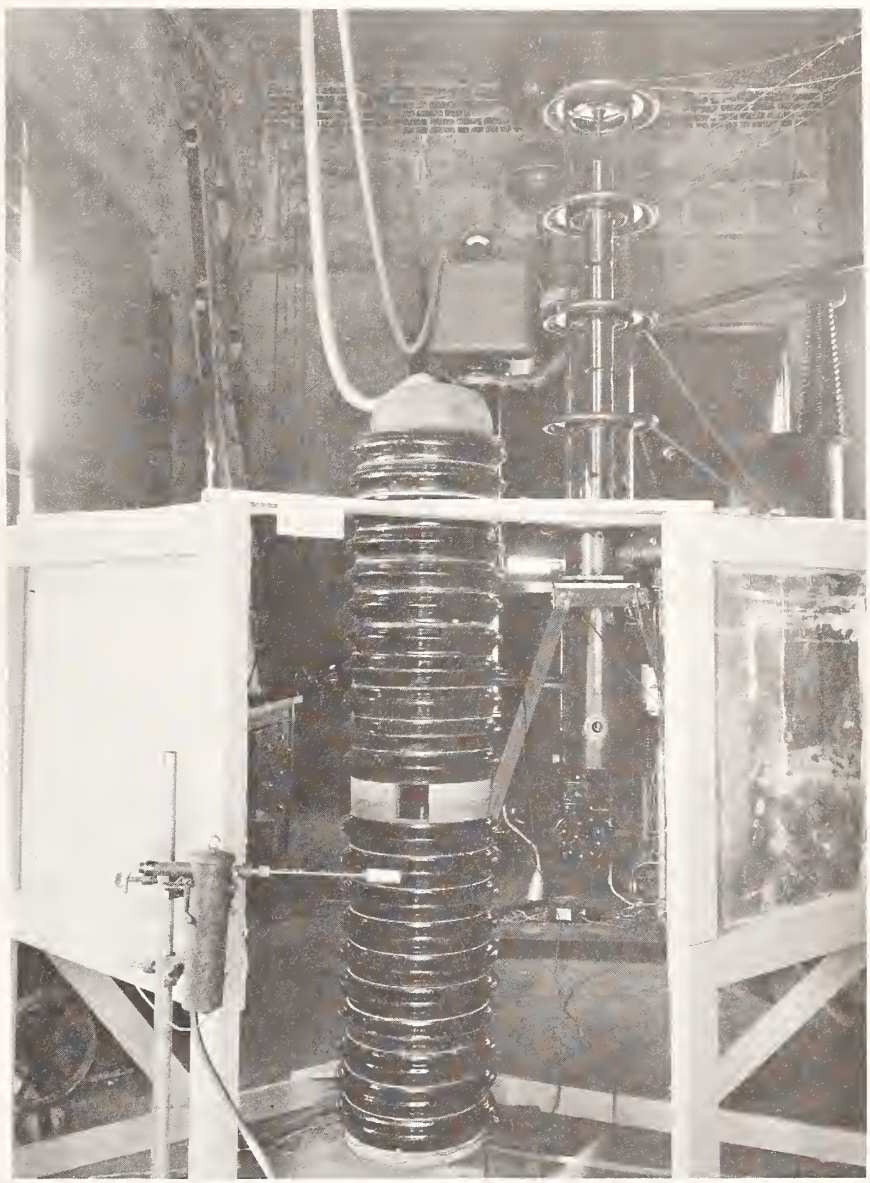

Photo No. 32. 400-kV x-ray tube with ionization chamber and FP-54 amplifier. 600-kV x-ray tube in background (1936).

Photo No. 33. Concrete test slabs over measuring pit for broad beam attenuation studies (1950).

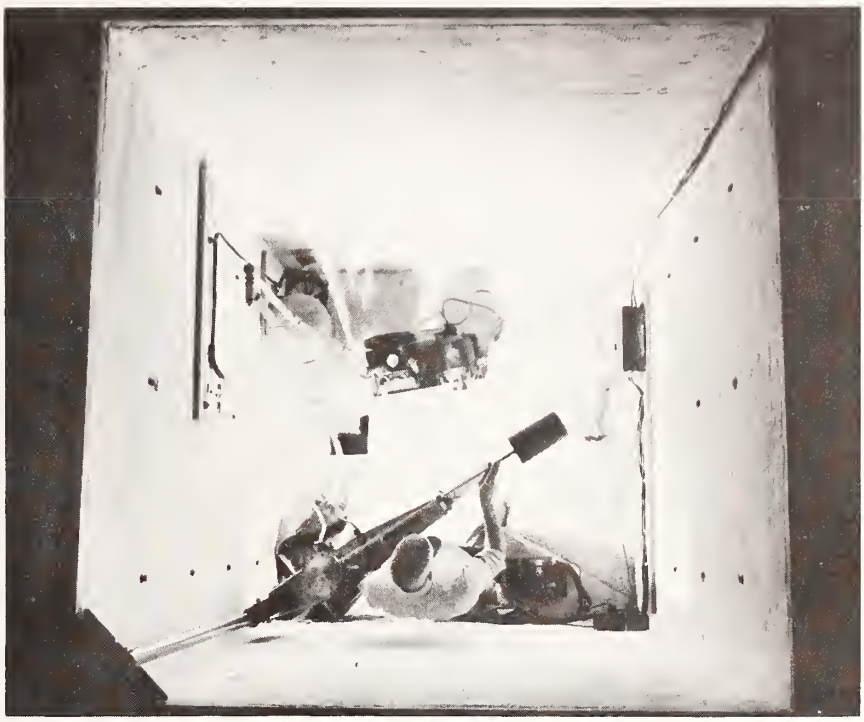

Photo No. 34. Measuring pit under 1,400-kV x-ray tube (1950).

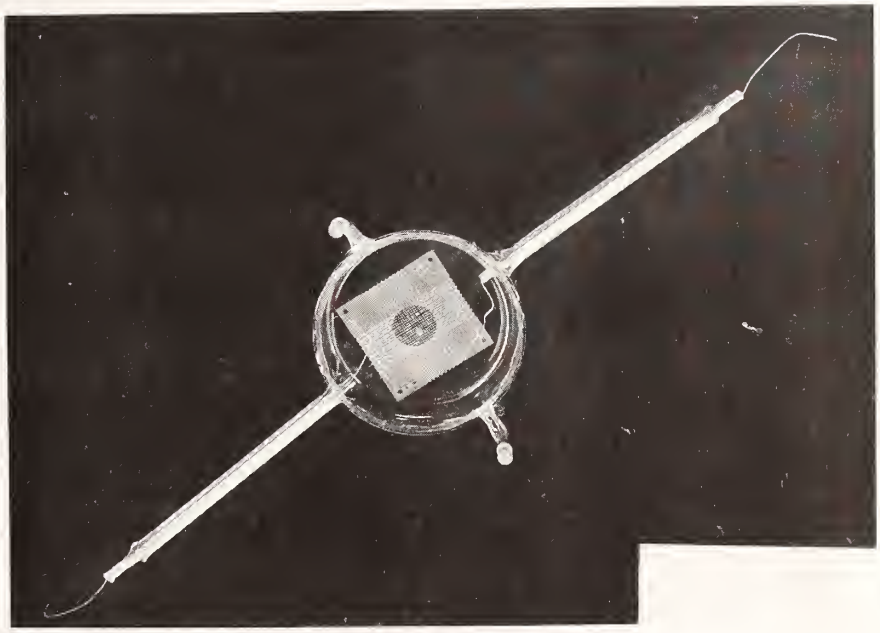

Photo No. 35. Crossed grid, liquid ionization chamber (1934).

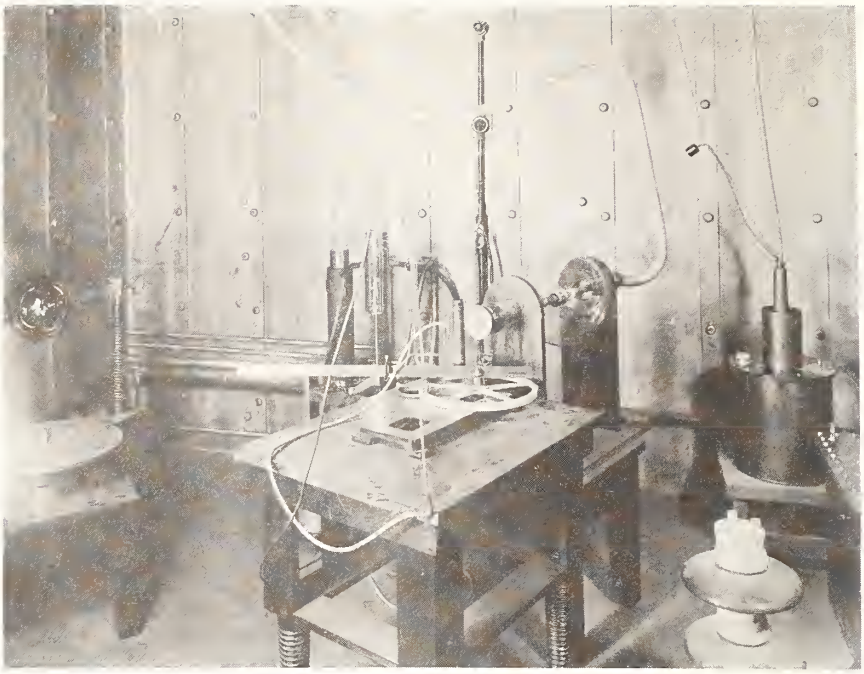

Photo No. 36. Coolidge Lenard-ray tube operated on pumps with disc-type electron collector (1928).

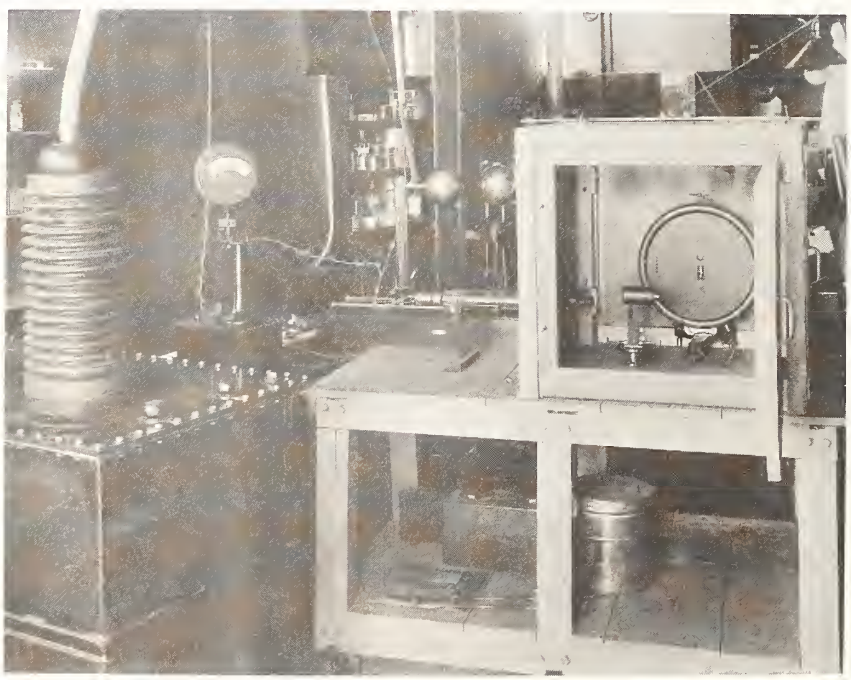

Photo No. 37. Slack sealed-off Lenard-ray tube with Faraday-cup electron collector (1928).

CHAPTER 11. NBS MOVES TO HIGHER X-RAY ENERGIES

The period beginning in the mid-1920's and extending for many years after was extremely active for newly developing x-ray technologies of all sorts. This section will cover some of the many developments in high-voltage engineering and their influences on the Bureau's radiation program.

Already mentioned was the fact that in 1926 the only high-voltage x-ray source at the Bureau was one of 140 kV (peak) resulting from a mechanical rectifier. By 1927, the Bureau had acquired a 300-kV (peak) mechanical rectifier, and by 1929 a source of 200 kV (constant potential).

In about 1931, a special source, utilizing a Villard circuit, was built for activating the Lenard-ray tubes. This circuit was a voltage doubling type which did not operate with a waveform smoothing filter. Hence the voltage waveform was a sine wave above ground potential. The particular NBS unit was based on a 150-kV transformer grounded at one end so that the final maximum voltage would be 300 kV above ground.

The circuit for this was very simple. The transformer output fed into a high-voltage capacitor and in turn to a rectifier tube and ground. The voltage, taken off between the rectifier and the capacitor, was led to one end of the x-ray tube or Lenard-ray tube, while the other end was at ground potential. The output potential could be either positive or negative, simply by reversing the direction of the rectifier tube. For NBS purposes, the filament of the rectifier was at ground potential, as were one side of the transformer secondary and the positive electrode of the x-ray or Lenard-ray tube. This provided a very simple voltage source that could be regulated easily and smoothly. The condensor for this rectifying unit was rated at 200 kV and was the same Meirowsky type previously obtained for the 200-kV standard constant potential x-ray set.*

*An interesting incident occurred while this high-voltage set was in routine use. A Patent Office examiner visited the X-Ray Laboratory one day to discuss a patent application involving the particular kind of circuit being used by the Bureau. He was convinced that it could not possibly work, that there was no way to rectify and double the voltage in the manner shown in the application. It was pointed out to him that this type of circuit, among a number of others, had been described by Villard around 1901 (Villard, 1901). Still skeptical, Taylor finally took him into the laboratory, showed him the actual circuit, and let him see how it exactly corresponded to the patent application. He departed, scratching his head. A few months later he informed Taylor that he would grant the patent because the Bureau of Standards had demonstrated it would work. Prior publication and prior use did not seem to bother him.

By 1933 or 1934, it was abundantly clear that x-ray energies for therapeutic and industrial purposes were pointing toward million-volt installations. There were also several installations from 600 to 1,000 kV in use or being built in the United States. Concern by the radiation staff that the Bureau of Standards must not fall behind at this time convinced the Division Chief, Dr. Skinner, to find $1,000 or $2,000 to plan and construct a suitable unit. Skinner's ability to do this was never questioned. (Bear in mind that this operation took place in about 1934 and had to be carried out at a strictly poverty level with a great deal of component scrounging from a variety of sources.) The operation hinged upon the personal effort of the three members of the x-ray group of that period (Taylor, Singer, and A. L. Charlton).

Although total program costs were generally unknown to one in the laboratory, Taylor believed that the expenditures for the 600-kV plant did not exceed the $2,000 limit set by Skinner.*

*Through all of this and in spite of the close and cooperative relationships between Skinner and Taylor, there is no knowledge that Dr. Skinner, at any time, from 1927 until his retirement in the early forties, ever visited any of the x-ray laboratory facilities. Nevertheless, he had given generous support to the work. Moreover, the

Director, Dr. Lyman Briggs, had never visited any of the facilities until his inspection of the new building in 1941.

The new installation was located in a space about 25 feet square on the third floor of the northwest corner of the Northwest Building. The ceiling was supported by 1/4-inch steel wires, approximately 14 feet above the floor, and about 6 feet below the concrete slab roof of the building. All of these details are important.

The first step in preparing the space was to provide adequate radiation shielding, which meant building a lead or concrete wall to ceiling height. Lead was too expensive and concrete construction was too conspicuous; there was no desire to draw the attention of the Bureau's administration to the operation. The radiation staff decided that a portion of the wall could be built with solid concrete blocks (as contrasted with the normal perforated concrete blocks).*

*They approached a small company in Virginia which was just starting a cement block business and proposed that, since there might be considerable demand for solid concrete block walls, it would be good experience for them to try out such a wall at the Bureau. The wall was constructed successfully, using Bureau masons and free blocks. Though the Bureau did no promotion of the block business other than to mention their use in a publication, the Cherrydale Cement Block Company near Rosslyn, Virginia, seemed to prosper. It is interesting to note that such outside help played an important role in this operation, as well as in other research and development programs at the Bureau. The term "conflict of interest" was not even known at that time; employees were assumed to be honest unless they demonstrated otherwise. This philosophy played a major role in the upcoming operations.

To meet the required 6-foot-square access opening, a reinforced concrete yoke consisting of a 3/4-inch lead door and track system was installed by the NBS Plant Division. The 3/4-inch lead sheet was supplied by a company for the differences in the value of a greater amount of scrap lead provided by the Bureau and the final amount of lead in the door.*

*At that time this was a perfectly acceptable procedure. Some years later the Bureau obtained at no cost a couple of hundred tons of shrapnel balls, surplus to the military requirements. These were exchanged for extruded lead bricks 2" x 4" x 8" by the company that bid to deliver the most bricks for the quantity of lead exchanged.

The next step was removal of the suspended ceiling to add a critical 6 feet to the height of the room. This was necessary to the success of the installation, but it was an operation that the staff dared not ask for funds or permission to do. The removal was accomplished quietly by Taylor, Singer, and Charlton, using extreme care not to attract attention. Starting in one corner of the attic crawl space, they carefully clipped the wires holding the ceiling and the supporting ribs, which were in turn suspended from the roof. As small portions of the ceiling were freed of supports and hung down, they were cut loose to avoid the concentration of too much weight on the remaining support wires and possible collapse of the entire ceiling, and to avoid any excessive noise that might attract attention (see photo No. 38).

Another concern was the floor loading of the main structural members of the building of monolithic construction, as well as the 25-foot-square floor slab. A Mechanics Division colleague suggested a simple way for checking this as the floor loading increased. A piano wire was stretched between the columns supporting the two main reinforced concrete beams and a second wire was stretched across the middle of the 25-foot-square floor slab. This operation was carried out with full cooperation of the Gage Section staff, located directly below the x-ray installation, whose attitude was professional rather than concern over possible consequence of overloading the floor above. The criterion established was that the loading must be stopped if the distance between the piano wire and the beam decreased as much as one-quarter of an inch or if cracks developed. Only a 1/8-inch deflection of the floor slab was permissible. Using gages supplied by the Gage Section, the staff did not detect any deflection at all.

The circuitry and arrangements for the high-voltage source and the x-ray tube are described in the paper "Measurement of Super Voltage X-Rays with the Free Air Ionization Chamber" (Ref. 62). However, there are some other interesting details not contained in that official publication which were important to the successful completion of the operation.

The four main 150-kV transformers required for the installation were contained in insulating cylinders between metal plates, the top one at 150-kV higher potential than the bottom plate. The transformers were built by the Kelley-Koett Company and loaned to the Bureau for 5 or 6 years (see photos No. 39 and 40). Filament transformers for the rectifiers, with the necessary extra insulation transformers for step-up of voltage, were supplied by the General Electric Company, also on a loan basis. The capacitors, arranged in four banks of three units each, were supplied by Westinghouse at a substantially reduced cost. There was also an extensive amount of homemade equipment making up the total unit.

The various components of the rectifier system were arranged on heavy wood frames supported from the floor by pedestal porcelain insulators. The frame for the fourth transformer, for example, had to be insulated for 450,000 volts above the floor, which was accomplished by means of several pedestal insulators obtained from the early Trans-Atlantic Naval Wireless Station located in Annapolis. Because that station, of the Paulsen-arc type, had operated at extremely high voltages, it required quantities of high-voltage insulators, many of which were still stored in an open field many years after the wireless station was replaced. Arrangements were made through the Navy to obtain approximately two truckloads of the surplus insulators.

Included in the insulators were many porcelain rings with an 18-inch outside diameter, 10-inch inside diameter, and 3 1/2-inch thickness, each with ground faces top and bottom. A 5-foot stack of these were used to house a 400-kV tube (see p. 169), and the remainder were "loaned" to Dr. Merle Tuve at the Terrestrial Magnetism Laboratory (TML) of the Carnegie Institution where experiments with high-energy tubes for a nuclear physics application were underway. At Tuve's Laboratory, a 24-foot stack of these insulators was cemented together as the envelope of a 2-million-volt accelerator of the belt generator type. The close collaboration between Taylor and Tuve on mutual problems was of invaluable assistance to both laboratories. (This probably played an influential role when, a few years later, Taylor was asked by Tuve to drop his x-ray work temporarily and initiate the Bureau's first proximity fuze program.)

The construction of the rectifier system was a noble experiment, and on the whole was reasonably successful. At the time of construction, one of the most expensive items appeared to be the purchase and replacement of rectifier tubes at a cost of some $250 each, and too often, these were of uncertain reliability. To overcome this, four rectifying tubes were constructed in situ, each in an 8-inch pyrex tube 40 inches long with suitable electrodes and filaments. The four tubes were mounted on brass manifolds and connected in a straight line across their base by 4-inch pyrex tubes about 4 feet long. The entire system was evacuated by two 4-inch apiezon "fine" oil diffusion pumps backed up by a "coarse pump." The system did, however, exhibit a difficulty because there was a possible mean-free-path of over 12 feet through the 4-inch connecting tubes, and a stray ion could be sharply accelerated to the point of causing electrical breakdown discharges.

The problem was eliminated by placing two brass tubular electrodes in each of the 4-inch glass connecting tubes, through which holes were drilled. This permitted the electrodes to be connected to small outside points which, through electrostatic field pick-up, allowed the rings to assume their proper potential.*

*This was the result of a number of consultations with Tuve who was experiencing similar difficulties. Tuve was always very positive in his recommended solution to a problem, and on one occasion was very defensive, even explosive, when told a couple of weeks later that his suggestion didn't work. After all, he would explain, that suggestion of 2 weeks ago is passé and certainly not what he would suggest today, now that he was wiser and more experienced.

The x-ray tube was somewhat more conventional. It consisted of four purchased sections of pyrex glass cylinders 12 inches in diameter and 18 inches long. The vacuum system consisted of four "fine" pumps backed by a coarse pump, arranged in a "sow and pig" fashion at the base of the tube. The general arrangement involved the use of toilet bends, sink traps, and many soldered joints in the metal portion of the system. The metal and glass parts of the system were joined with apiezon wax.

The techniques of that time were regarded as very advanced, but in comparison with today were very primitive indeed. One special problem concerned the soldering techniques for joining the many metal components. The instrument shop could assemble the metal parts with sweat-soldering techniques which appeared satisfactory but, unfortunately, leaked like a sieve. The instrument maker, carefully trained in soldering techniques by his German master, had to be retrained by a laboratory man who had to "convincingly demonstrate" how

only a different method would work. The technique involved the use of oxy-acetylene torches and ugly gobs of solder fillets which flowed off to feather edges around each joint. The method, however, did provide good vacuum-tight solder joints.

Considering the nature of the 600-kV installation, it worked well and enabled the Bureau to gain a needed understanding of the problems of measuring x rays in the range up to 600 kV. Voltage measurement and control was by means of sphere-gap and the high-voltage-resistor voltmeter multiplier described above (see p. 125). In addition to the continuously evacuated 600-kV x-ray tube, the system was also used for exciting a 400-kV sealed-off x-ray tube as described earlier (see p. 169). Meanwhile, the x-ray sources for medical, as well as for commercial purposes, had pushed into the 1- and 2-million-volt ranges.

1,400-Kilovolt X Rays (1940)

While the program outlined above was proceeding, the medical profession recognized the trend to higher energies in the midthirties and began to besiege the Bureau with requests and recommendations for moving to yet higher voltages. Reacting to these pressures, the Bureau agreed to consider an installation of a million or more volts, and, although unsuccessful in past years, enlisted the support of the electrical industry for a much needed high-voltage testing laboratory. Coupled with needs of the medical profession, which was considered to be less "self-interested" than industry, the project showed some signs of support.

This was taking place in about the middle of the Great Depression and at a time when the Government was looking for worthwhile public works to help industry, generally, and the building industry, in particular. Accordingly, arrangements were made for the services of some architects who were then employed on a make-work basis by the WPA (Work Projects Administration).

There were two groups at the Bureau interested in the proposed installation. They were the Electrical Division (F. M. Defandorf and F. B. Silsbee) and the X-Ray Group (Taylor and Singer), each to share equally the available above-ground space. However, the x-ray laboratory was to include extra space below ground level to simplify some of the radiation shielding problems. From the outset, the major part of the building would consist of a transformer bay approximately 130 feet long, 65 feet wide, and 65 feet high, with a suitable crane for handling and servicing the equipment. To the front of this bay would be five floors of laboratory space about 30 feet wide for work involving voltages less than those called for in the large room.

The WPA architects successfully advanced the design to the point of specifying the details of the steel structural frames, concrete shielding walls, metallic lining of the transformer bay, and other special features. It was then possible to start the process of estimating costs and obtaining funds for the construction. This was about 1938.

Funding had been finally achieved, when the responsibility for the final design and specifications was shifted to the General Services Administration. The absolute cost limitation on the $500,000 appropriated by Congress posed an immediate problem because the revised GSA specifications for structural steel almost exactly doubled the material requirements and the cost estimated by the WPA architects based on normal industrial design. GSA also specified the highest quality for such items as building hardware, doors, and electrical equipment, again at considerable cost in excess of what industry would have specified. The partial result of these changes was some scaling down in the building and laboratory requirements, as well as some sacrifice of equipment which could have been purchased under the original plan. Nevertheless, the final achievement of laboratory design and equipment was adequate for initial x-ray needs, but permitted no allowance for growth (see photo No. 41).

When the final high-voltage x-ray equipment was being scheduled for purchase, there was only approximately $40,000 remaining, whereas the original cost estimates for the equipment were about $75,000. After negotiating with two or three of the large electrical companies, it became evident that only one, the General Electric Company, had any interest in designing and supplying the kind of equipment required. Knowing the status of the budget, they bid $40,000 for the equipment which must have cost at least twice that amount. Nevertheless, the result was probably the finest and most powerful high-voltage constant-potential x-ray equipment ever produced; its peak voltage capacity (constant potential) was 1,400 kV at a current of 25 mA. Since no x-ray tube had been built for such power, the transformer-rectifier was tested by means of corona loading as proof that it met these specifications. Following is a brief discussion of the high-voltage equipment; additional details are given in articles by Taylor (Ref. 71) and E. E. Charlton (Charlton, 1940).

The high-voltage equipment was designed to give a constant potential of 1,400 kV with a ripplage of 0.08 percent per milliampere (see photos No. 42 and 43). The transformers were composed of 10 identical units of 140 kV each, stacked one on top of the other and rigidly bolted together. Each 140-kV unit contained the step-up transformers, two rectifiers, smoothing capacitors, and an insulation transformer to feed through to the next unit. The bolting sections were covered with adequately rounded corona shields and the whole stack was topped with an aluminium corona cap about 11 feet across and 4 feet high. The complete stack could be operated with the top at a positive or negative potential. Adjoining the main transformer stack was a herkolite tube with a 22-inch inside diameter that was open for the full height of the stack and contained 1,400 1-megohm wire resistors as noted earlier (see p. 125). Directly above the transformer was a Behr generating voltmeter for controlling the voltage, which could also be controlled through the voltmeter multiplier. To the side and on top of the projecting target room were two multi-section 12-inch diameter glass x-ray tubes for a total height of approximately 22 feet (see photo No. 44). Either tube could be connected to the transformer at each 140-kV step by means of telescoping aluminum tubes, thus uniformly grading the potential along the tube and minimizing the development of run-away potentials.

The control room was located on the mezzanine level at some distance from the target-room area and was shielded with 18 inches of concrete. The heavily shielded target room, upon which the x-ray tubes were constructed, served primarily as the target and measurement area where work with the standard ionization chambers was carried out and where a variety of the shielding studies were made.

In addition to the control room, the mezzanine floor included some open work space usually associated with experiments in the target room area. On the third floor was equipment of 250-kV constant potential and another shielded control room, used primarily for medium energy x-ray standardization purposes (see photo No. 45). This equipment permitted the alternate connection of a number of different tubes, such as a 200-kV beryllium window oil-cooled tube and a 250-kV fine focus tube. In addition, there was a 60-kV, 100-mA constant-potential set for use with a high-intensity beryllium window x-ray tube. Included were numerous miscellaneous devices in the 100-kV range, together with special electrical testing and radiation measuring instruments.

On the fourth floor were two or three rooms for instrument development work involving such items as special scintillation counters and associated recording instruments. The AEC Instruments Branch and the radiation laboratory's Theoretical Section were located on the fifth floor, along with a complete photographic darkroom, chemistry laboratory, and instrument shop. (This was the general, post-war arrangement. Originally, Mohler's laboratory occupied a portion of the fifth floor space; then U. Fano, and finally the AEC Instruments Branch.)

Betatron Laboratory (1946)

In the fall of 1945, following the defeat of Germany and Japan, Taylor, who had spent 2 years in Europe, was winding down his activities as Chief of Operations Research in the Continental Air Command. Meanwhile, he began to catch up with developments in the radiation field during his almost complete detachment over a period of some 5 years. One of the first things coming to his attention among the recent developments in high-energy x-ray physics was the invention of the betatron.

During the war years, Dr. D. W. Kerst, working at the University of Illinois and at the General Electric Company in Schnectady, had developed the so-called betatron principle for producing high-energy radiations. With the conclusion of the war, it became obvious that machines of this general type would find medical and industrial applications. It was therefore necessary for NBS to cope with the problems of radiation measurements at these very high energies.

In a discussion between Taylor and the Director, Dr. Briggs, it was decided that the Bureau would seek funds for a new betatron laboratory together with funds for two machines, one operating up to 50 MeV and the other up to 100 MeV. Though the latter had already been built by the General Electric Company, the lower energy unit seemed to be the more likely one to be used in therapeutic applications. The Bureau was given permission to seek the funds as a part of a deficiency appropriation, whose purpose was to pull many Government agencies back into the mainstream of peacetime activities.

Cost estimates were obtained for the purchase of the two machines. Meanwhile, plans were going forward for the construction of a new laboratory to be located almost entirely

underground, for shielding purposes, at the east end of the high-voltage laboratory which had been completed just 5 years before.

Then followed the usual Government procedure for handling an appropriation item. First, cost estimates were made by those who best understood what the needs were--in this case, Singer and Taylor. Their estimates included the building construction, the purchase of the equipment, personnel cost, and an additional 10 percent for contingencies. During the next step, the Director, who naturally assumed that his budget planners had overestimated the cost, chopped the funds back by some percentage to come up with a rounded figure. This figure passed through the Commerce Department and the Bureau of the Budget, and arrived at the Appropriations Committee without any further cuts. In late fall of 1945, Taylor was called upon to justify the request, even though he was still employed by the Air Force. In due course, he and the new Director, Dr. Edward Condon, appeared before the Senate Subcommittee dealing with the request.*

*The time allowed for the hearings was 30 minutes. Among the evidence that Taylor had taken with him was a copy of Radiology, containing some colored photographs of people who had been seriously "burned" by the careless use of a 1-million-volt x-ray installation at the Huntington Hospital in Boston. After about 5 minutes of routine presentation, Taylor opened the journal to the colored illustrations, passed it to the Chairman, and stated that this was the kind of situation the Bureau hoped could be avoided as a result of its proposed studies. The Chairman glanced at the illustrations, then thumbed through the journal, finally coming upon a radiograph of a somewhat knarled hand. He held up the journal and asked, "Doctor, is this a case of arthritis?" Even after looking at the title of the illustrations, Taylor was not sure, but said that he expected arthritis might look like that. "Doctor, my sister has the worst kind of arthritis you ever saw, she is in such misery ---." While speaking he had passed the journal on to the man next to him who had an Aunt who had arthritis, the next member had a brother with it, and so on, until it was evident that the entire committee had somebody in the family with arthritis. In the meantime, the 30 minutes had slipped away and Taylor was squirming because he had not yet gotten in his well-planned pitch. Suddenly, the Chairman turned to him and said, "Doctor, I think this is one of the finest programs we have listened to in many a year. I am sure that our committee will endorse this and we will give you all the funds you asked for." And they did--for the building and equipment.

Planning moved into high gear. Building plans progressed far enough for reliable cost estimates to be made. Bids were obtained on the two machines. However, when the appropriation finally became available, the funds requested for building and machinery were granted, but Congress had "temporarily" struck out the personnel funds with the instruction that they be requested when the installation was complete. What the Bureau had intended, of course, was to bring in some of the personnel who would use the equipment so they could contribute to the proper planning and use of the new installation.

As a consequence of the shortage of personnel funds, other ongoing limited programs had to be robbed, first of manpower and later of the funds necessary to bring in a person with betatron experience. The denial of personnel funds by Congress thus proved to be a great source of future difficulty. It also led to some degree of ill feeling on the part of those who were "robbed," and a degree of "irresponsibility" on the part of those who benefited. The rifts and difficulties never healed.

When bids on the new building were finally obtained, they were found to be within the means of the available funds. However, just as ground was about to be broken, the President declared a freeze on all Federal construction. After a number of weeks, the ban was partially lifted during which time the Government had removed the industrial and construction price controls in effect during the war years. This necessitated the renegotiation of the contract which, following this single action, was inflated by approximately 30 percent. Plans for the proposed underground laboratory had to be ruled out and replaced with an above-ground structure that was much less expensive, smaller, and more restricted as far as use was concerned. However, there was no other recourse, so plans went ahead on that basis.

Shortly after the order was placed for the 50-million-volt and 100-million-volt betatrons, Dr. H. W. Koch of the University of Illinois, a physicist who had worked with Kerst, was brought in to head up the Betatron Section, to recruit staff, and to plan specific research programs.

Meanwhile, with developments in the high-energy accelerator field advancing rapidly, the Bureau learned that, without having to scrap the basic magnet of the accelerator system, it would be possible to change the acceleration principle from that of a betatron to that of a synchrotron, and by this means to increase the energy from 100 to 180 MeV. Accordingly, the contract was modified to include a 50-MeV betatron and a 180-MeV synchrotron (see photos No. 46 and 47).

By the late 1950's, while the betatron programs were well underway, new accelerator devices, such as the linear accelerator (Linac), began to show promise of special high output and operating characteristics highly desirable for nuclear physics research.

Also, by the late 1950's, plans were being completed for the modernization and relocation of the National Bureau of Standards at its new Gaithersburg, Maryland, site. The plans included a 50-MeV "Linac" as part of an extensive radiation installation.

At the time of the final move to Gaithersburg in 1965, the 50-MeV betatron was abandoned. Original plans were to also abandon the 180-MeV synchrotron, but by the time the laboratory move was to begin, the staff of the Atomic Physics Division discovered that it would serve as an invaluable tool to produce synchrotron light, whose characteristics, though unrelated to those of ionizing radiation, would be of substantial value in the optical region. As a result, the laboratory building was extended to accommodate the synchrotron. Though it is still in use at the time of this writing (1980), there is talk of abandoning the Linac and moving to other programs. (As it turned out, neither the synchrotron nor the linear accelerator were ever employed for any significant amount of biomedically oriented applications. Even the 50-MeV betatron was used for such purposes to a negligible extent.)

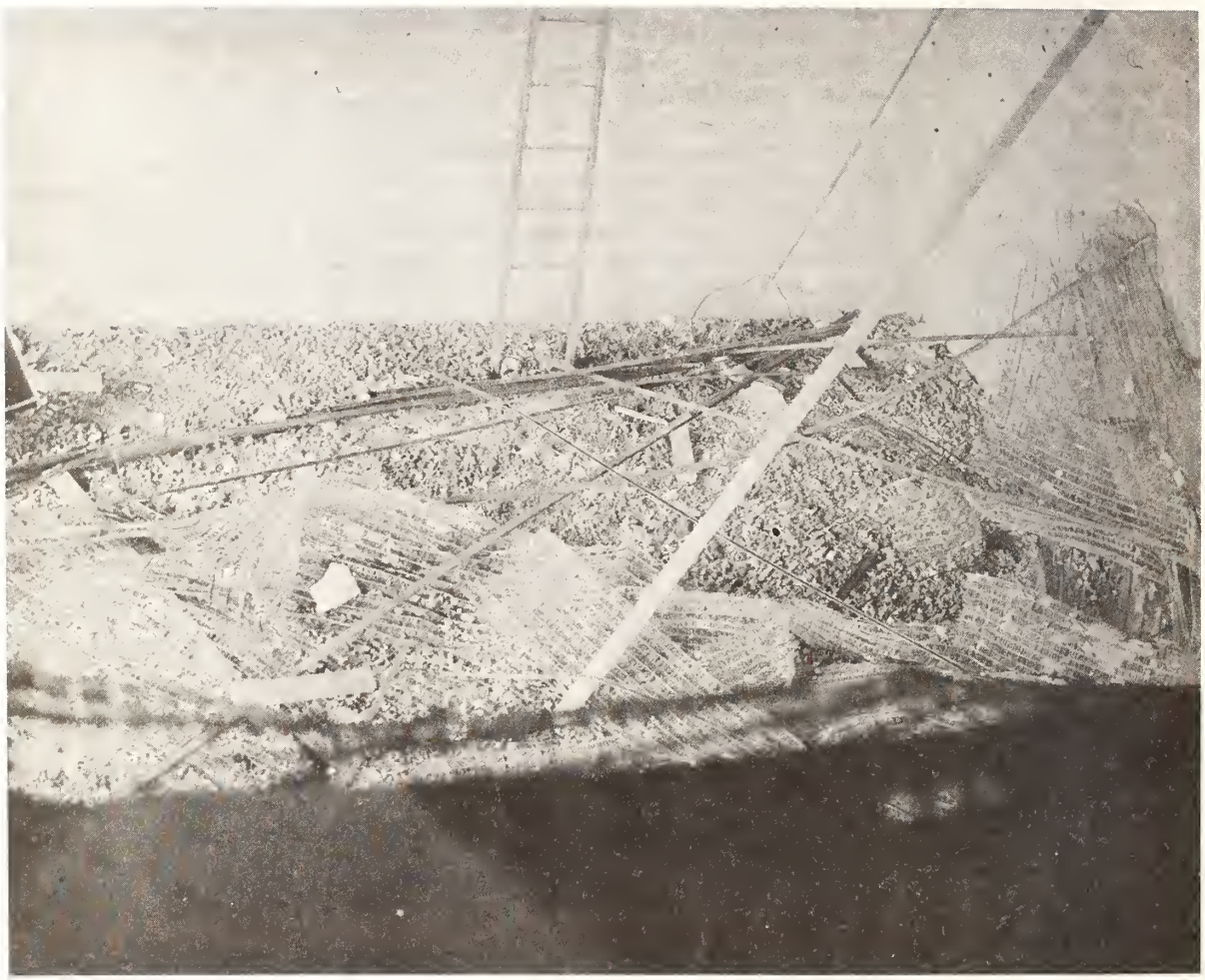

Photo No. 38. The ceiling removed from x-ray laboratory in Northwest Building, allowing installation of equipment shown in next two photos (1935).

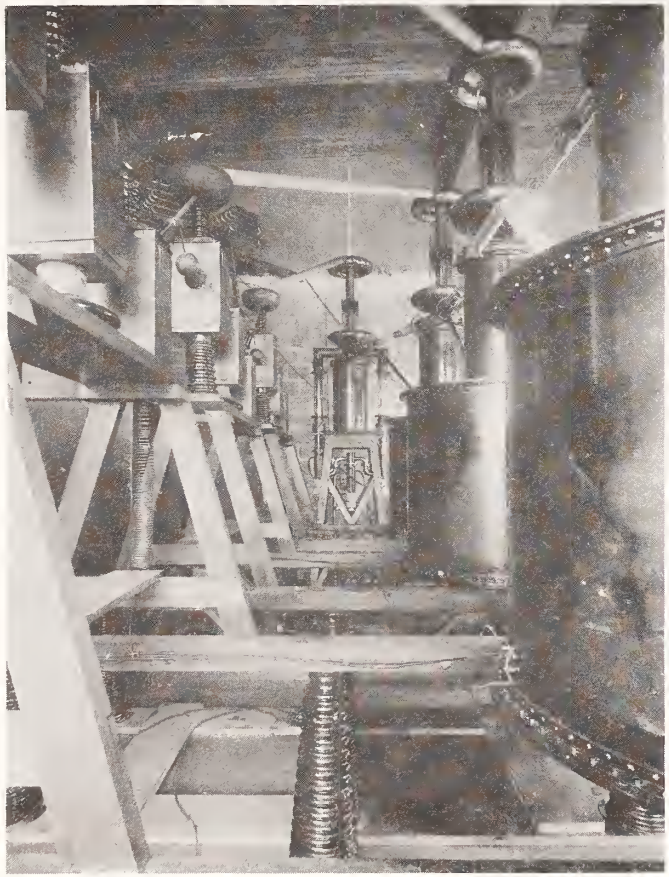

Photo No. 39. 600-kV rectifier installation showing pumped rectifiers (1935).

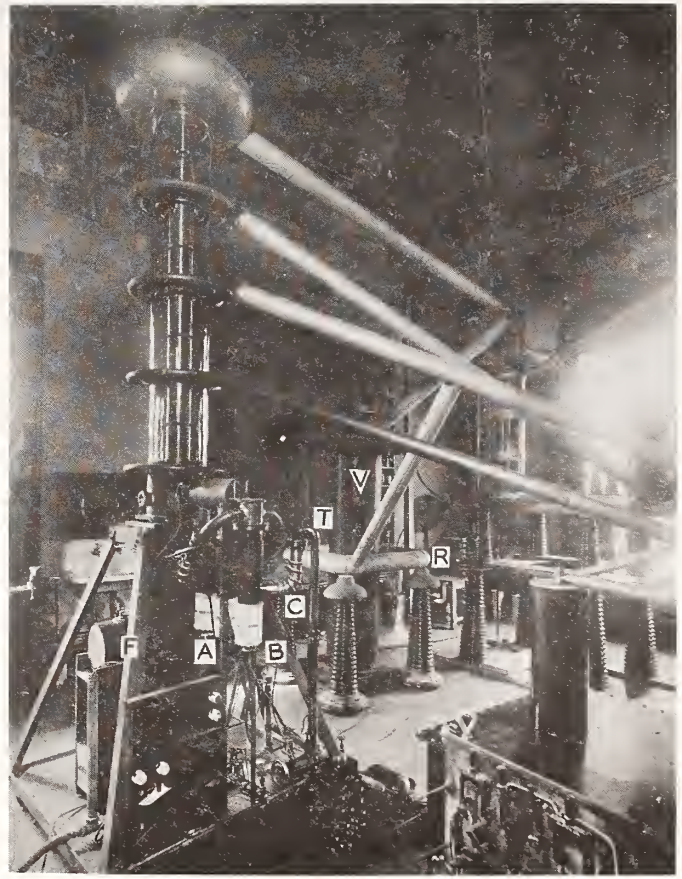

Photo No. 40. 600-kV x-ray generator with connections between tube and rectifier units (1935).

Photo No. 41. New High-Voltage and X-Ray Laboratory (1940).

Photo No. 42. 1,400-kV d-c generator connected to one of two x-ray tubes. Small column left of generator contains voltmeter-multiplier resistors (1940).

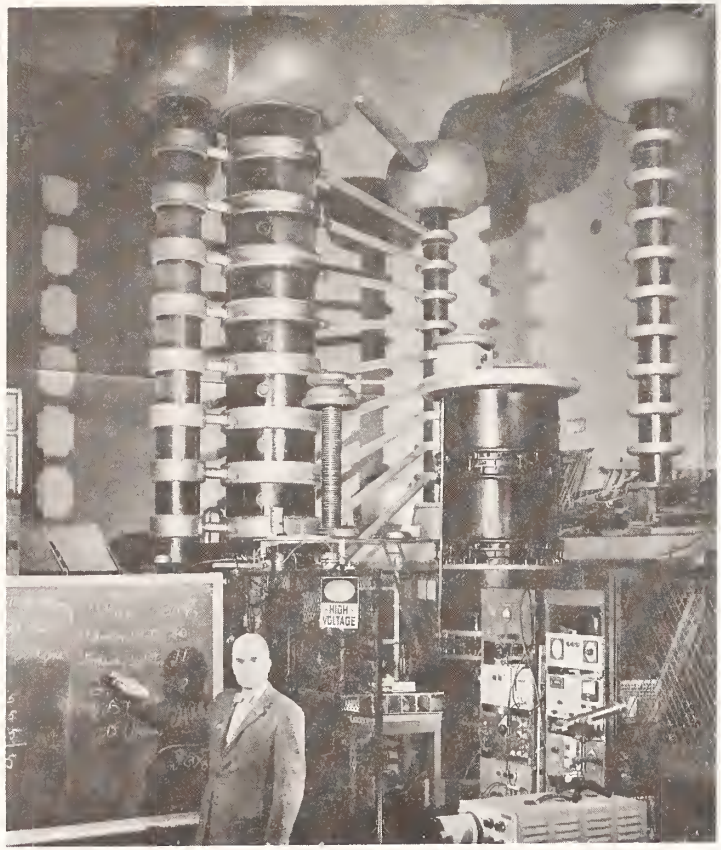

Photo No. 43. 1,400-kV d-c generator with 560-kV, in high-intensity x-ray generator in foreground (1958).

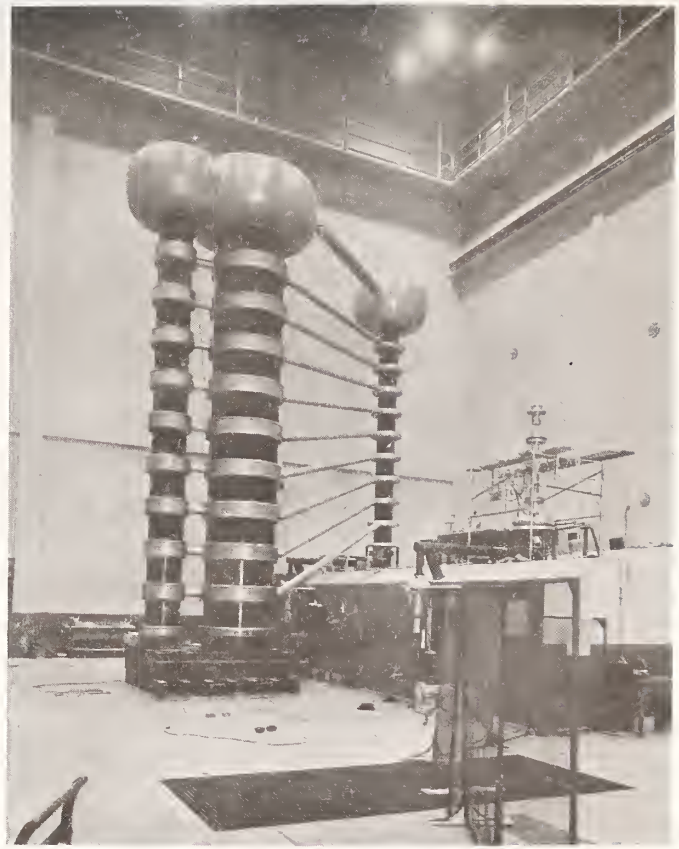

Photo No. 44. 1,400-kV d-c generator with one x-ray tube in place and the second tube (right) in initial construction stage (1940).

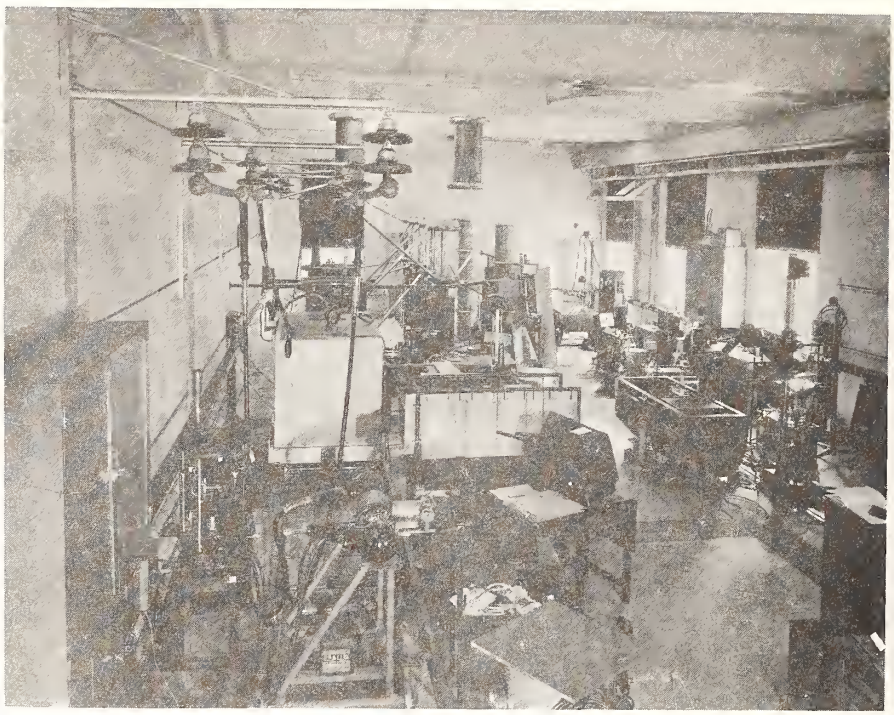

Photo No. 45. 250-kV laboratory on third floor of the High-Voltage and X-Ray Laboratory (1948).

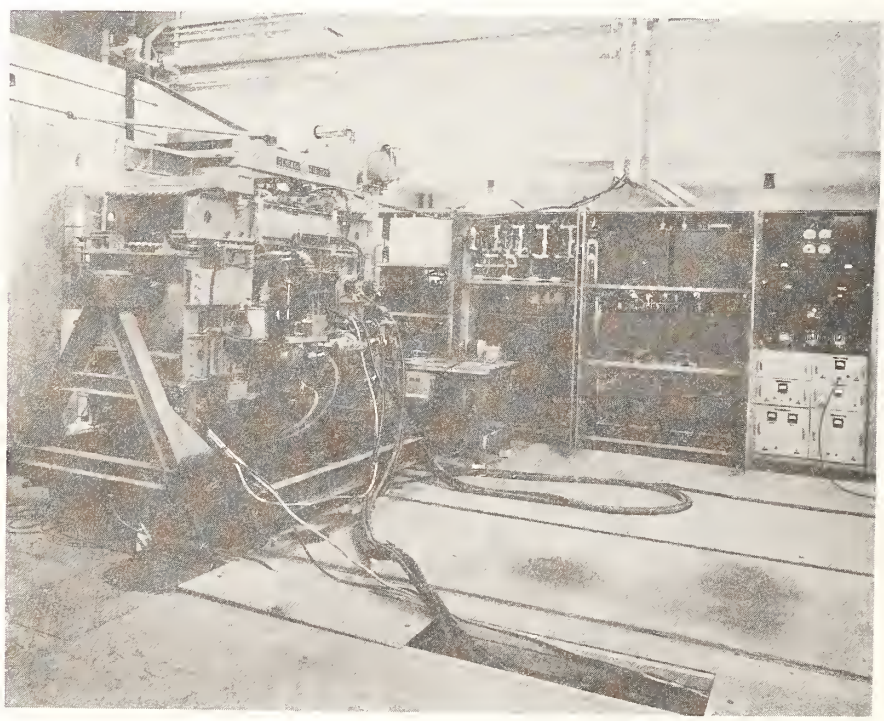

Photo No. 46. 50-MeV betatron (1950).

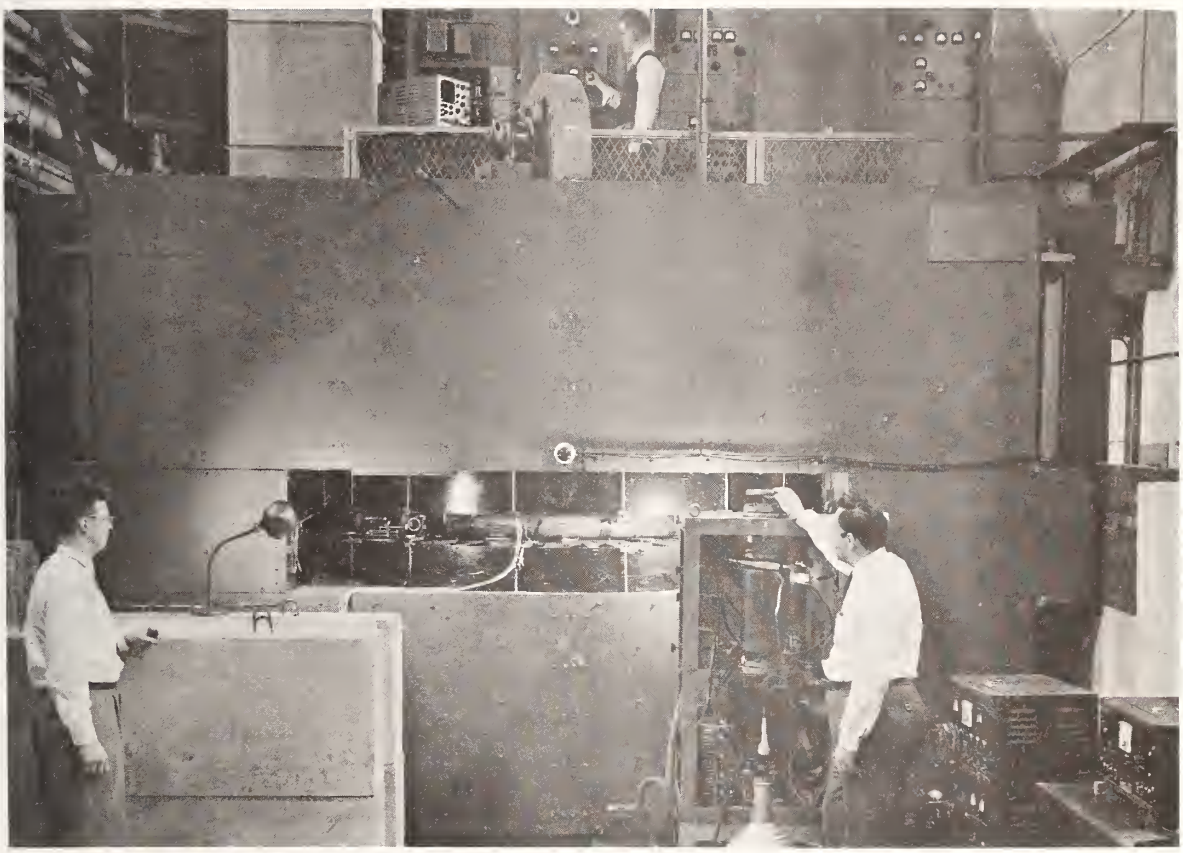

Photo No. 47. 180-MeV synchrotron (1953).

CHAPTER 12. SAFETY AND STANDARDS COMMITTEE, AMERICAN ROENTGEN RAY SOCIETY

The standards committees of the American Roentgen Ray Society (ARRS) and the Radiological Society of North American (RSNA) carried out joint operations beginning in 1936. A substantial portion of ARRS interests were therefore covered earlier along with those of RSNA. A few additional items concerning the ARRS are discussed below.

With the establishment of the Registry of X-Ray Physicists by the Standardization Committee of the Radiological Society of North America, it was recognized that any such effort should have the cooperation of the other radiological organizations, for example, the ARRS and the American Radium Society (ARS). The question was put to the President of the ARRS who, with the immediate backing of his Board, agreed to collaborate in the program. A similar approach to the ARS brought an equivalent response.

Following this the ARRS, realizing that there was substantial overlap of membership between their Committee on Safety and Standards and the RSNA Committee on Standardization, felt that there was logic in combining the two operations. At the Society's meeting in September 1936, it was decided that this could be accomplished by appointing Taylor Chairman of the Safety and Standards Committee of the Society.

The initial membership of the ARRS committee included Dr. K. W. Stenstrom, Dr. E. C. Ernst, Dr. R. R. Newell, and Dr. Douglas Quick. All had been members of the original RSNA committee and, except for Quick, were also current members. The new committee was really more of a formality than a new element in the Society's committee structure. By a common chairmanship and overlapping membership, a meeting of either committee essentially served both societies. At that time, the Standardization Committee of the RSNA had a number of extensive projects underway. Reports on these, together with reports on some earlier meetings, were submitted to the Roentgen Ray Society in the name of its Committee on Safety and Standards. This the members understood and accepted. Essentially, this also meant the endorsement of the two principal radiological societies. Furthermore, since it was customary to hold committee meetings during the course of the national society meetings, it now meant, for all practical purposes, that the joint committee could conveniently hold two meetings a year.

The charter adopted by the ARRS Committee on Safety and Standards was as follows:

> "The Committee on Safety and Standards shall formulate recommendations for regulating safe operation of roentgen apparatus, the safe application of radioactive substances--it shall keep in active contact with the Bureau of Standards of the United States and if possible with similar bodies elsewhere and similar committees in other Societies."

In September 1937, the report of the Committee on Safety and Standards to the ARRS was in essence the same as the earlier report from the RSNA (see p. 159). The principal difference was the omission of the listing of physicists (RSNA-1936).

The 1939 report by the Safety and Standards Committee closely followed the report of the RSNA, adopted a short time earlier (RSNA, 1940). (Also see p. 223.) Of particular interest was the inclusion of two paragraphs in the formal report to the Roentgen Ray Society that were not included in the published version. These follow:

> "Regarding the relationship of the National Bureau of Standards to this Committee, it was pointed out that since the National Bureau of Standards looks to the Committee for its recommendations regarding x-ray standards, it would like to suggest that the committee be recognized as the final body regarding the adoption of standards and units in this country. To provide a starting point for the Bureau's activity in the x-ray field, it looks to this committee for an indication of the primary needs of the radiologists. Based upon the consequent investigations of the National Bureau of Standards, it can make its own recommendations to the Committee.
>
> "This policy has a direct bearing on the interrelationship of the National Bureau of Standards, the Council of Physiotherapy of the AMA, and this Committee in regard to the

questions of x-ray equipment standardization proposed by the AMA last fall. The National Bureau of Standards would offer as a suggestion that this Committee have an advisory capacity to the Council of Physiotherapy in dealing with this matter."

There was a third paragraph which appeared in the published report and which should be repeated here.

"Regarding the AMA proposals for x-ray equipment standardization and performance standardization, the Committee wishes to go on record as being positively opposed to the recognition of any laboratory as a sole agent of establishing such standards. It feels that any such establishment of standards should come through some suitably representative body which can be in a free position to judge the merits of any related investigations carried out either by its request or by some independently operating laboratory."

These paragraphs are interesting because they represent the view of some of the Committee members, and because they indicate the members' feelings on the inter-relationships between the Government and private organizations. The reason why the first two of the paragraphs were not included in the published version of the report does not appear to have been recorded.

In October 1938, Dr. Eugene Pendergrass, Secretary of the ARRS, received an invitation from the American Standards Association (ASA) to name a representative to their sectional committee on "Standardization in the Field of Photography--Z38." The letter of invitation included the following outline of the committee's program and scope.

"The formulation of definitions, dimensional standards, and recommended practices in the field of photography, and the establishment of methods for testing, rating, and classifying the performance characteristics of materials and devices used in photography, including its industrial applications, but excluding cinematography."

Though the outline appeared to indicate that major attention would be devoted to what would normally be described as photography, the matter was referred to the Committee on Safety and Standards. After discussions with the ASA, the committee learned that the scope would include some aspects of radiographic procedures. Accordingly, Dr. F. O. Coe, a radiologist, was designated as the representative of the ARRS. Shortly thereafter, Coe also became a member of the Committee on Safety and Standards. Coe's first meeting with the ASA Committee came on July 18, 1940. The following is part of the report he submitted to the ARRS:

August 2, 1940

"The meeting was attended by a large representation of the various subcommittees. Dr. L. A. Jones occupied the chair.

"The reports of eight of the nine subcommittees were presented for consideration. These were fully discussed and some progress was made in solution of the problems involved. No final action on committee reports was taken. The committees as a whole are making very satisfactory progress in the standardization of photographic material, which is of vital interest both to doctors using photographic material as such, and indirectly to all radiologists.

"At the conclusion of the stated program your representative asked:

"1. That some action be taken in regard to a speed rating for x-ray films, both with and without intensifying screens:
"2. That the speed of intensifying screens be investigated, especially in regard to the aging of intensifying screens;
"3. That information be furnished in regard to the burning time and ignition time of heavy base films such as x-ray films, both before and after exposure;
"4. That the toxicity of stored exposed radiographs, on burning, should be investigated.

"These suggestions were referred to the appropriate subcommittees.

"Your representative would appreciate inquiries from any member of the American Roentgen Ray Society in regard to suggestions which it would like to have investigated, falling in the general subject of standardization of photographic materials."

On May 28, 1941, Taylor received a new listing of committee members from ARRS, consisting of Ernst, Glasser, Newell, and Taft. Advisory members (and their expertise) were J. L. Weatherwax (X-Ray and Radium Protection), F. O. Coe (American Standards Association), Edith Quimby, and Douglas Quick (Use, Rental and Sale of Radium). The ARRS letter noted that Taft would be assuming the responsibilities of Dr. Bernard B. Widmann. (The author found no previous mention of Widmann.)

Dr. Coe, in his August 26, 1941, report to the Committee on Safety and Standards, gave the following brief outline of the ASA work up to the time of their last meeting in July, 1941:

August 26, 1941

"During the past year I have attended regularly the meetings of the American Standards Association in New York, being on Mr. Nicholson's Committee No. 3.

"During the year we have endeavored to set up standards for safety films. This necessarily is a very slow process, as there is a wide variation in the requirements according to the German National Standard, the British, and those accepted by the Underwriters Laboratories. The standard that we are trying to make is largely concerned with the (1) ignitability, (2) rate of burning, and (3) toxicity of the gases evolved on burning.

"Without going into detail the greatest difficulty has been in getting a standard of nitrogen content of so-called safety films. As a representative of your Society I have been especially concerned with these elements because of the National Defense Program, which has resulted in the use of such large quantities of films, that their storage becomes a national problem.

"I have proposed that at the time when it can be taken up we will consider such subjects as a standard for speed of X-ray films, limits of variation in size of films, and standardization of intensifying screens and fluoroscopic screens. This is all within the scope of the ASA Z-38.

"I will conclude by saying that some definite progress has been made, but no accepted standards have yet been proposed."

This was the last report of record submitted by Dr. Coe. It was forwarded to the Secretary of the ARRS for information and no further committee actions were taken.

As noted above Dr. Douglas Quick had been added to the Committee on Safety and Standards to deal primarily with matters concerning the use, rental and sale of radium. On October 2, 1940, the following short report was accepted by the Committee at its annual meeting and forwarded to the Secretary:

October 2, 1940

"This Committee accepts the report of the advisory member on Use, Rental and Sale of Radium and begs to make recommendations as follows:

"1. That the membership be advised of the reaffirmed ruling of the A.M.A. on unethical practice, and that the ruling be called to the attention of the Board of Censors. The giving of consultation on the use of radium, by mail or otherwise, without examination of the patient, is regarded as unethical practice.

"2. That the American Radium Society be informally advised of the fact that certain of their members are violating the code of ethical practice as defined by the House of Delegates of the A.M.A.

"3. That this Society call the attention of the Advertising Department of the A.M.A. to the fact that they are providing advertising space in the Journal of the A.M.A. and

exhibit space at the last Annual Convention to certain individuals and corporations who are operating contrary to the code of ethics as defined by the House of Delegates of the A.M.A. in 1938 and reaffirmed at the annual meeting in St. Louis in 1939."

This was followed by a further report on September 2, 1941:

"Allow me to acknowledge your letter of August 22 in which you ask for a report to reach you by September 5 and for the enclosures in the form of a long detailed letter with abstract and former proceedings of the A.M.A. from Olin West. The data from Dr. West is not new in any sense. These we have collected and filed right along as the various matters have come up. The unfortunate point is that Dr. West either misses the point of the whole argument or very cleverly beats about the bush. No one questions the honest rental of radium. By that, in plain English, I mean the rental of radium by those who are qualified to use it. The trouble rests with those who are not qualified to use it and yet for financial gain wish to rent it if someone will just guide their hand by a little advice. The other part of the iniquitous combination is with the company, institution or individual, who is willing to rent the radium preparation, give the advice, without the proper firsthand examination of the patient. We have called attention to this one feature so often I do not see where we can make further progress since those who do have some measure of authority at least are prone to shut one eye to the dangerous practice. As far as (the) report (goes) therefore, I think that I, as an advisory member of the Committee on Safety and Standards, have nothing more to offer than to again call attention to the report offered last year and under date of October 2, 1940."

Notes on the Committee's October 2, 1940 meeting were prepared by Taylor but not published. They are as follows:

MISCELLANEOUS NOTES ON MEETING OF COMMITTEE ON
SAFETY AND STANDARDS
AMERICAN ROENTGEN RAY SOCIETY

October 2, 1940

A report of the subcommittee to work with the American Standards Association, of which Dr. F. O. Coe is chairman, was read in Dr. Coe's absence and accepted by the committee for inclusion in its meetings (see p. 201).

A report of the subcommittee on the use and rental of radium was presented by Dr. Douglas Quick of New York and showed a great deal of effort on his part. A short report on this work had been previously presented informally to the executive council without any recommended action from the committee. However, upon hearing the report, the committee decided that the information was sufficiently complete to warrant taking specific action. Accordingly, a resolution was passed by the committee recommending prompt action by the society in this connection (copy of this is not available at present but will be obtained and mailed to you at some later date). A more detailed report on the phase of our discussion will be sent to you from Dr. Quick.

INHERENT FILTRATION

The question of inherent filtration has been repeatedly referred to this committee. Two aspects will be considered, (1) From the dermatological viewpoint, inherent filtration is important since very large short-time skin doses are frequently applied and small changes in radiation quality are important. Since it is frequently difficult to measure quantity directly in most dermatological treatments, the dermatologist feels an urgent need for knowledge of the inherent filtration of his x-ray tube. From the manufacturers point of view, inherent filtration is important in a competitive sense since it apparently influences the radiation output from his x-ray tube. The manufacturer is not interested in x-ray quality nearly so much as x-ray output and if this attitude is to be changed an educational campaign will have to be undertaken. It was suggested that the term "inherent filtration" as such be avoided and that instead the half value layer of the radiation be given in all cases. A relatively simple table of half value layers for various voltage and filter combinations could be used as a guide both in regulating treatment conditions and in arriving at some idea of the

"inherent filtration" of the x-ray tube for the manufacturer. For deep therapy tubes it was suggested that the half value layer of the unfiltered beam be measured and stated by the manufacturer. It is then a relatively simple matter to see how much filter must be added to bring the radiation to the desired half value layer. This information could be readily provided by the manufacturer with each type of tube or possibly with each individual tube.

RADIATION DISTRIBUTION

A paper by Lillian Jacobson on the distribution of radiation within a phantom was brought to the attention of the committee. In this paper relatively large field variations were shown under conditions normally encountered in deep therapy with modern x-ray tubes. The committee finds that it was very important to have field uniformity and where such uniformity was impossible to at least have information on the degree of nonuniformity. It was suggested that the manufacturers be requested to supply such information with all new machines and to make a particular test of the question with each change and re-alignment of x-ray tube in a shielded container. It was also recommended that the registered physicists be informed as to the dangers of field nonuniformity with the request that they be on the lookout for such in the installations which they calibrate. It will, of course, be necessary to designate tolerances in this connection since there will always be a certain amount of grading at the edge of the fields. The committee was of the opinion that the fields used by Miss Jacobson were unduly large. It was agreed that Dr. Weatherwax look for such irregularities in the number of standard x-ray installations at his disposal. His investigation is to be confined for the present to 20 x 20 fields at a distance of 50 cm from the target. It was suggested that he get in touch with Miss Jacobson with the idea of securing her cooperation in this work. In order to better acquaint the committee with the importance of this problem, it was suggested that the manufacturers be requested to send the committee such information as they may have on distribution measurements.

REGISTRY OF X-RAY PHYSICISTS

The following applications were received and action taken as noted:

Scott W. Smith	-	Referred to Board
J. Cramer Hudson	-	Approved
Charles Robb	-	Approved
T. R. Folsom	-	Approved
C. A. duPont	-	Referred to Board
Frank E. Hoecker	-	Referred to Board
Robert A. Patterson	-	Referred to Dr. Failla
James F. Marvin	-	No action
A. W. Zimmerman	-	No action

It was suggested that closer relations be maintained between the Standardization Committee and the registered x-ray physicists. This will serve both to assist the physicists in keeping abreast of newer developments and to assist the committee in keeping them informed on field problems. It was suggested that a letter be sent to all registered physicists in which they will be asked to make protective surveys on all installations which they are calibrating for the first time. It was also suggested that the registered physicists be asked to send in to the Board sample copies of their calibration reports. These will be used to make up a uniform report form. Such a form should be of assistance both to the radiologists and to the physicist. It will assist the physicist in that (1) he will not be so likely to overlook some small detail, (2) it will assist in comparison of test data made by other physicists, and (3) it will present evidence to the radiologist as to what is desirable in the way of a calibration of his equipment without at the same time putting the physicist in the embarrassing position of appearing to try to sell the doctor a lot of service (which the doctor may not feel is necessary).

It was agreed to call to the attention of radiologists the availability of this expert consulting service in order that they might better avail themselves of the listing of x-ray physicists. In this connection frequent publications of the list of physicists will be made. It was decided to determine and set up a list of fair charges

and services. This will greatly assist many physicists who do not know how to properly charge for their services. The question of uniform reports and fair charges is to be worked out by Doctors Glasser and Weatherwax and a preliminary report presented at the Cleveland meeting (question by chairman--my notes are not clear as to whether this information on charges and services was to be made available to the radiologists as well as the physicists or whether it was just to be presented to the physicists alone. Will you please give me your recollections on this point).

The committee reaffirmed its stand on the use of the roentgen for expressing radium dosage. The committee is of the opinion that all radium dosages should be expressed in roentgens at the point delivered.

The above presents the minutes of the meeting as written up from my rather brief notes. It will be appreciated if each member will study these and let me know as soon as possible of any errors of omission or commission. When these comments have been received, the report will be written up with care and such parts of it as may be desirable submitted to the journals for publication.

Lauriston S. Taylor, Chairman

This represents the last of the ARRS Committee on Safety and Standards. It seems to have quietly disappeared during the war years.

CHAPTER 13. CLINICAL IRRADIATION CONSIDERATIONS

While all efforts of the different committees of the several radiological organizations were directed to the problems of improving the medical applications of radiation, many aspects were more scientific and academic than others. One true value of the academic approaches lay in their applications to the day-to-day problems of radiologists whose daily pressures compelled practical solutions. This chapter will discuss a few attempts to provide such solutions to some clinical needs. Other aspects of the clinical problems will be dealt with in those chapters in which the subject matter seems more appropriate.

Clinical Treatment Charts

It would be obvious that the proper administration of radiation to a patient should be accompanied by a plan and a complete description of the procedure--physical and medical factors, measurements, and so on. From the outset, these were items on the Standardization Committee agenda, but the first significant action began in about 1933-34. Perhaps the main difficulty lay in the fact that clinics already had recordkeeping systems that were usually tailored to their particular operational practices, and they were reluctant to disrupt or change their procedures just to conform to a standard practice. What was not adequately appreciated was the fact that improved standardization facilitated the research analysis of radiation therapy results, aided in those situations where patients were transferred to other institutions, and provided the clinic with legal protection. Standardization was especially important if the published reports of different clinics were to be meaningful.

As the problem of proper radiation treatment description continued to persist, Taylor continued to urge the Editor of Radiology, Dr. Leon Menville, to remedy the situation in so far as publications were concerned. On the subject of dosimetry alone, he wrote to Dr. Menville as follows:

January 3, 1935

"Referring further to your letter of December 21st, I would like to point out that the "dosage factor" question which you propose treating editorially, has been treated in some detail by both our National and International Units Committees and has been contained in two recent publications (Radiology XXII, p. 289; and XXXII, p. 580). Now the matter which worries me is, that Blank's list is not in accord with the committee's findings and moreover, is not complete in itself--lacking five extra factors. I am taking the liberty of revising the list in accord with the published reports. The order is of no significance:

"(1) Number of roentgens per area (measured in free air).
"(2) Dosage rate (rate of applying the radiation, r/min).
"(3) Equivalent voltage.
"(4) Filter - material and thickness (also include approximate tube wall thickness).
"(5) Distance - skin to target.
"(6) Interval between irradiations.
"(7) Size and number of ports of entry.

"In case these suggestions are agreeable to you, I would like very much to see the note published."

L.S.T.

In connection with problems of clinical dosimetry, it has already been noted that the Standardization Committee made a series of efforts to develop a standardized recordkeeping form for permanent retention, and covering all radiotherapeutic procedures. An example submitted to the Committee was that used by Dr. U. V. Portmann of the Cleveland Clinic Foundation. A substantial quantity of these forms were circulated for comment by the radiologists of that period. Following is the format supplied.

CLEVELAND CLINIC FOUNDATION

RECORD OF ROENTGENTHERAPY

No. _____ Name _____ Address _____

Diagnosis _____ Referred By _____

Previous Treatment _____

Date	FIELD	Size	KV.	FILTER	ewl	½ v. l.	F.S.D.	% D.D.	Ma.	Min.	MaM.	r. P.M. Air	r. P.M. Field	r. Field Dose	% Opp. Field	r. Opp. Field	Total Field Dose	Tumor Depth cm.	% Tumor Dose	r. Tumor Dose	Total Tumor Dose
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22

*See explanation of the various columns on the next page.

Cleveland Clinic Foundation
Record of Roentgentherapy
August 1935

(The following is an explanation of the various columns)

1) Date.
2) Field: the region of the body treated.
3) Size: dimensions, that is, 10 x 10 cm.
4) Kv. peak.
5) Filter.
6) ewl: effective wave length.
7) 1/2 v.l.: one-half value layer - copper or aluminium.
8) F.S.D.: focal skin distance.
9) % D.D.: percent depth dose
(Note - the columns from "Size" to "%D.D." inclusive, as shown in the dark lines are fixed conditions and factors which need not be measured or calculated for each treatment. This is also true of the next dark-lined column.
10) Ma.: milliampere.
11) Min.: minutes
12) MaM.: milliampere minutes.
13) r. P.M. air: r. per minute measured in air.
(Note - this heavy line column from "Ma." to "r.P.M.air" inclusive are fixed conditions which apply to intensity output).
14) r.P.M.field: r. per minute as measured on the field, including backscattering (this arrangement permits accurate observation of a fixed condition of r. P.M. measured in air as contrasted with the r. P.M. measured on the field.
15) r. Field Dose: number of r. applied to the particular field at one dose or on that particular day of treatment.
16) % Opp.Field: percentage of the dose which reaches the opposite field or opposite skin area.
17) r. Opp.Field: number of r which reach the opposite field.
18) Total field dose: the summation of the dose which is applied to a particular field from day to day plus the amount reaching this field from the opposite side of the body. (Note -a- if 250 r. given on the first day, 100 r. reaches the opposite field, then on the second day this opposite field is treated, the total dose on field one would equal 250 plus 100 x 300 r.) (Note -b- the heavy line column "r. P.M." field to "Total field dose" inclusive applies to the intensity of radiation administered to the skin areas.
19) Tumor Depth cm. is an estimate of the depth in cm. of a tumor or region of the body which is under treatment.
20) % Tumor Dose is an indication of the percentage of the dose which reaches a tumor from the dosage given to the skin.
21) r. Tumor Dose is the actual number of r. given to the tumor as calculated from the percentage which reaches it.
22) Total Tumor Dose is the total or additional of all of the intensity which reaches the tumor and is expressed in r. and is the summation of the dose which reaches it from each crossfired skin field. (Note - the opposite side of the sheet is blank for diagrams and drawings.)

Ed. note: (Note that several of the above items appear to be a bit archaic in view of current understanding of clinical dosimetry.)

Reactions to the proposed chart varied widely as indicated by the two samples given below--one from a physicist, the other from a radiologist.

Dr. G. C. Laurence, a physicist in charge of x-ray standardization programs of the Canadian National Research Council Laboratories, cited his experience with Canadian endeavors to achieve standardized treatment charts. He emphasized the great variety of records used by various institutions and their complexity. He made the following recommendations:

September 10, 1935

I am not clear whether it is intended that every column of the record should be filled out. If so I think there are too many. If, however, it is intended that only a certain number of these are to be filled out and that inclusion of the extra columns is intended to provide some caption in the way in which information is given (for example, with respect to quality of the radiation) I think the record form is excellent and the only change I might possibly suggest is that provision be made for giving the length and breadth of the tumour as well as its thickness. Would it not be better also under percentage tumour dose and r tumour dose to give here the values at the point in the tumour where they are minimum? By so doing one has then a record of the two tolerance limits of the treatment, viz. skin dose which determines the maximum permissible dose, and minimum tumour dose which determines whether the treatment is likely to be adequate. This is intended rather as an inquiry than a suggestion because it is outside the physicist's sphere.

In Canada the Associate Committee on Radiology of the National Research Council of Canada has a small Committee consisting of Dr. W. A. Jones, Mr. J. D. Leitch and myself, which has been looking into this subject and consulting many of the radiologists in Canada. We began by collecting a great variety of treatment records from various sources and attempted to criticise them but soon found that although this procedure was valuable as a source of suggestion we did not get very far with it and we finally settled down to a discussion of what we felt a record chart should contain. The following suggestions are my own and not necessarily the views of this committee, although of course I have been considerably influenced by them.

The first thing that was strongly emphasized to us was that the radiologist has little time for making records and therefore the briefer they are the better. It is well worth catering to this point of view because I think the making of records deserves every encouragement.

I. I suggest that we distinguish three classes of information on the records as follows - (a) information which is of value to other radiologists or for statistical summaries; (b) information on instrumental adjustments, or from which (a) is derived, which is of interest only to the particular clinic and of no interest outside it; (c) information which is derived from (a) or expresses (a) in different terms.

II. I suggest that this Committee consider first (a)-type information, agree upon such details which are necessary for inclusion in this class, reducing them to the minimum possible and recommending that every record chart make provision for these entries.

III. I suggest that the Committee do not lay down too mandatory instructions regarding (b) and (c) but it may make suggestions and draw up a suggested complete card.

IV. Information (a) is of two kinds of equal importance: (a') - description of the tumour, and (a'') - description of the radiation. Both classes of information are necessary to determine dose. I suggest that (a') include (1) location on body of tumour; (2) kind of tumour (pathological classification); (3) dimensions of tumour; and (4) greatest depth of tumour below surface of skin. (a'') information may include (1) date of treatment; (2) size of port; (3) focal skin distance; (4) roentgens in free air; (5) quality of radiation. This information is to be given for each port of entry when there are more than one. Note that from these data all information likely to be of interest in another institution may be derived. The roentgen in free air is to be measured at the skin distance from the cathode; the quality of the radiation is to be expressed in one of the two alternative ways--first, copper half-value layer, and second, by giving the kilovolts peak, the filtration, and stating whether the power supply is spark or kenetron rectified or constant potential.

V. Under the head (b)-type information, may be included such alternative methods of quoting (a)-type information as for example, equivalent wave lengths of the radiation, etc. I think under this head might be included a column entitled Technique Number,

because some radiologists make a practice of summarizing for brevity several details of a treatment which they frequently repeat under a Technique Number. Under head (c)-type information, can be included any such details as total dose for all ports, minimum tumour dose, skin dose with scattering, etc.

I am making the above suggestions in detail merely as a basis of discussion for I realize that the opinion of the physician is much better than that of the physicist on these questions.

G.C.L.

Pertinent comments of a radiologist came from Dr. R. R. Newell, an individual who not only had a good understanding of physics but one who was abounding in sheer common sense. Early in the 1930's, Newell became active with the Standardization and the Protection Committees and remained so throughout his life. He was one of the most important contributors from the medical profession to the work of the committees. Indeed, his advice and opinions were of great value to the author during his active career. Following are the comments made by Dr. Newell:

September 16, 1935

I take it that these record forms are for clinical use. I have the following criticisms to make:

Field - There should be a sketch of each field to scale or with annotations of distances from major landmarks. More room is needed in this column to permit precise designation.

Size - O.K. We write 10 x 10 or 13, and could write 110 cm^2 if area were trapezium or triangle so that the room allowed is probably sufficient.

KV. - should be abbreviated kv without capital or period. If Taylor can (im)prove upon his work, then this column ought to be kvCP equivalent.

Filter - Too much room and might be 3 columns, Sn, Cu and Al. It is perfectly safe to write in these columns 1/5 or 1/2 or 2 letting it be understood that they are measured in mm.

ewl - Might as well leave out. This ceases to be of useful significance in the supervoltage range--even at 400 kv.

1/2 v.l. - Leave in only as long as International Committee keeps it the official designation of quality. It should however be written H.V.L. copper.

F.S.D. - Having been sensitized by N.R.A. and the rest of the alphabet, I should prefer "distance" or "dist.", but that's a matter of taste.

% D.D. - leave out. This is a matter for a work sheet. It isn't a measurement, but is computed from the rest of the record.

Ma and Min - both O.K. and necessary.

MaM. - leave out, quite superfluous.

r.P.M. Air and r.P.M. Field - both superfluous, as they can be calculated from the total r and Min.

r. Field Dose - Could well be shortened to r without the period. We need always the record of roentgens measured in air at the distance of the field. This is what this column should record.

My next proposition is not yet generally accepted, but here it is and I'm sure it will be accepted more and more generally:

Backscattering varies with size of field, thickness of part, and quality of ray. It is very difficult to measure accurately. It is probably more accurate to lift it from tables based on phantom measurements than it is to do clinical measurements on each patient.

Now the argument for recording the field dose, i.e., an estimate at least of the tissue dose at the surface is that this value includes the correction for area (i.e., the influence of backscatter). Physically this argument is good but clinically we see that it covers only a part of the influence of area. I mean we have to make allowance for area over and above the part it plays in backscattering. We can and do deliver a larger tissue dose to small volumes than to large volumes. We cannot say that if 4000 roentgens measured at the skin including backscattering is a proper dose to an area of 2 cm^2, then we can safely give the same dose measured the same way to a 200 cm^2 area.

What I'm driving at is that there is no use recording the surface dose as augmented by the effect of area, when we have to go ahead and make a further allowance for area

treated. And it is disadvantageous to have our records give augmented dose when the raw dose (in air) is a more accurate measurement.

% Opp. Field and r. Opp. Field - I'd leave these out. They are calculated, as a rule, and can be got from the rest of the record. If one is in a given case keeping such close track of totals, then an extra line of the record can be used each day to add in the influence on an opposite field of what is going clear through.

All these suggestions boil down to a recommendation to reduce columns 12 to 18 inclusive to just one column, namely r or roentgens, explicitly (or implicitly) measured in air at the given distance (of skin from focus).

Tumor depth cm. - I'd leave this out and expect a careful worker to chart the position and size of the tumor.

% Tumor dose - Superfluous as a tabular entry.

r. Tumor dose - I'd suggest substituting a heading, "Tissue dose at center of tumor" or abbreviated "Tumor tissue dose". Someday we'll have a good unit of tissue dose and we ought until that time to permit workers to record it in any units they wish--"erythema doses", "standard doses", "Cancer doses", "r." or "ergs per cm^3".

Now about total dose. I find I've less and less use for calculated accumulated dose on the basis of 7% per day extinction. I do wish to have before me the total dose given each field during a given course of fractional doses. I would therefore add after the column "r" a column "total" or "total this course". And if you wish retain your last column "Total Tumor Dose".

I would move the column "Field" over next after the "total" column and next before the "tumor tissue dose" column.

I have found very useful a column for medication, so as to have before me a note as to how roentgen sickness and skin reaction are being palliated.

R.R.N.

By the end of 1936, Portmann's effort to reach reasonable agreement on a standard treatment chart finally bore fruit and was approved by the Standardization Committee of RSNA. After submission to the Board of the RSNA, the chart was printed in quantity for general use as an official recommendation by the RSNA. It is not known to what extent and for how long the chart was used. Following is the Committee report:

December 1936

To the Members of the Standardization Committee of the
Radiological Society of North America:

Enclosed are the printer's proofs of the two forms for the Standard Chart for Roentgenotherapy and also a copy of the proposed explanation which will be published with them in Radiology.

Your attention is directed to some minor changes which a subcommittee made after the general Committee Meeting in Cincinnati: (1) Under "Calibrated Physical Factors", the columns have been rearranged in logical order so that those factors having to do with quality are in one group and sequence and those having to do with quantity follow. These groups are separated by heavier vertical lines. This order also conforms to the natural order in the "Daily Record of Treatment". (2) Instead of "Tube" infiltration, provision has been made for recording this under "Inherent", with a column for "Total" infiltration.

The printing is being done through Dr. Donald S. Childs as secretary of the Society.

U. V. Portmann, M.D.

Form 1

CALIBRATED PHYSICAL FACTORS

TECH	DESCRIPTION OF APPARATUS	KV.	FILTRATION			HVL. MM.	MOV.	BEAM SIZE	DIST. CM.	R/M AIR	
			INHERENT	ADDED	TOTAL						

DAILY RECORD OF TREATMENT

DAY	DATE	TECH	REGION TREATED	FIELD SIZE	TSD CM.	MIN.	R. AIR	QUANTITY ON SKIN					QUANTITY ON DISEASE				
								R/M	FIELD R.	EXIT R.		TOTAL R.	%	R/M	DAY R.		TOTAL R.

Form 2

STANDARD CHART FOR ROENTGENOTHERAPY

NAME: No.

ADDRESS:

DIAGNOSIS:

PREVIOUS TREATMENT:

CLINICAL NOTES:

PATHOLOGY, LOCATION, EXTENT:

BODY MEASUREMENTS:

DISEASE MEASUREMENTS:

PLANS FOR TREATMENT:

*See explanation of charts on the following pages.

EXPLANATION OF CHARTS

1. The Standardization Committee of the Radiological Society of North America deems it advisable to recommend a standard form or chart for recording the factors used in administering treatments with roentgen rays. The general acceptance and usage of this standard form should encourage accuracy in planning treatments and create mutual understanding about the procedures employed by different individuals, thus ensuring uniformity in recording the technical factors, especially in publications when technics are described. After almost two years of study, the Committee has designed a chart form which should be acceptable. It contains all essential data about treatment factors in proper order and convenient spacing.

2. Two forms have been designed to be printed on the standard 8½ x 11 inch page usually used for institutional records and in private offices. The page may be folded in half so that the record will be 5½ x 8½ inches to conform to another standard size sometimes preferred. Each form will be obtainable separately or printed together on opposite sides of one sheet of paper or card. The printed matter may be centered on the page or offset to allow sufficient margin at the left, right, top or bottom for binding or for printing additional data, the names of the institutions, or individuals, etc. Thus, these forms can be adapted to almost any sort of record or filing system.

3. Form No. 1 was designed for those who prefer to keep clinical data and the record of treatments on one page in which case Form No. 2 would be printed on the opposite side of Form No. 1. It may not be necessary to use Form No. 1 in institutions or offices where clinical data is kept in detail or where other accepted record systems are used but it should be a convenient form for private practitioners. The headings for the horizontal lines on Form No. 1 are self-explanatory. It should be noted that provision has been made for recording measurements of the body and disease areas which necessarily must be made when the plans for treating a patient are outlined, in order to predetermine the quantity of radiation to be administered to different regions. The bottom half of the page is left blank for drawing, stamping or printing anatomical diagrams. It is suggested that rubber stamp anatomical outlines will be useful for this purpose. They may be obtained from the American Medical Association at reasonable prices, in many varieties and sizes which illustrate different regions of the body. The portals of entry or fields of radiation and the disease areas can be sketched into these outlines quite easily and accurately.

4. Form No. 2 should be used by all radiologists to comply with the recommendations of the Standardization Committee. This form provides spaces for recording all essential physical data and their use in giving treatments. When this form is used alone, there will be sufficient space at the top or margin in which to have printed any headings desired in addition to lines for the name of the patient, number, and diagnosis. The back of the page may be used for anatomical diagrams, for recording measurements of the body and disease areas, or other data which may be desired.

5. On Form No. 2 the "Calibrated Physical Factors" of technic are arranged in one group and the data concerning their application to the patient in another group under the heading, "Daily Record of Treatment." This arrangement obviates the necessity of writing each day those physical factors which are not changed during the course of treatment of a patient. Usually a radiologist will employ several technics for the treatment of different types of diseases. The differences between technics depend largely upon variations in voltages and filters or more than one apparatus may be used. A physicist who is registered by the Standardization Committee should standardize and calibrate each apparatus for every technic that the radiologist uses. A permanent record of these calibrations should be kept for ready reference in book form or charts. For convenience, each different technic or set of physical factors should be given individual identification by designation with a letter or number. When the radiologist has decided which technic he will employ in the treatment of a patient whose record is being compiled, he will write its letter or number designation and all of the "Calibrated Physical Factors" which pertain to it in the appropriate spaces provided on the form as will be explained.

6. Under the heading, "Calibrated Physical Factors," the first column is headed "Tech" (technic). In the first space below should be written the letter or number chosen to designate the technic which will be employed in the treatment of the patient, and the physical factors which apply to this technic will be written along the first horizontal line in the proper spaces. When this is done, there will be no necessity for rewriting these factors each day that a treatment is given. It might happen that two or even three different technics would be used in treating a patient. To meet this contingency, three horizontal lines have been provided. If a second or third technic is used, the letter or number designating each should be written in the "tech" column, and the physical factors which apply to them should also be written along the horizontal lines in the proper spaces.

7. "Description of Apparatus": Under this heading a brief description of the apparatus should be given, including especially the type of current used — whether pulsating or constant potential — and also the type of tube — whether air suspended or oil immersed.

8. The physical factors which have to do with the quality of the radiation are grouped together as headings for the next columns between heavy vertical lines.

9. "kv." (kilovolts). In choosing a technic it is natural first to decide upon the voltage which will be used. Voltage should be designated as "Peak" or "Constant" according to the type of apparatus used.

10. "Filtration": Filtration naturally follows kv. as a factor influencing quality. Under this heading are columns and spaces for writing in the thickness in millimeters of all filters which may be used for the technic employed.

11. "Inherent": Under this heading should be written the copper equivalent thickness of any filtration which is inherent in the tube or tube holder. This applies especially to metal or oil immersed tubes.

12. "Added": The thickness of any filter which is added to any inherent filter should be written in this column. When large ionization chambers are used in the beam of radiation, the copper equivalent thickness should be included as a part of the "Added" filtration.

13. "Total": This column is provided to summarize the total amount of filtration and should be expressed in millimeters of the metals used for the technic.

14. "h. v. l., m.m.": According to international agreement, the quality of a roentgen ray beam should be expressed by half-value layers in millimeter thicknesses of aluminum for low voltages and in copper for high voltage. The correct designation of the quality should be written by appropriate abbreviations in the spaces of this column.

15. The physical factors which have to do with the quantity of radiation are grouped together as headings for the next columns between heavy vertical lines.

16. "ma" is used to indicate the milliamperage used for the technic.

17. "Beam Size": Under this heading is noted the linear size in cm. of the beam through a diaphragm or cone used in making the calibrations for the technic. This size may vary during the treatment; therefore in the "Daily Record of Treatment" provision has been made under a heading "Field Size" for indicating any change. Due allowance and corrections should be made for changes in the size of the beam.

18. "Dist. Cm." (distance in centimeters). The distance in centimeters from the tube focus to the ionization chamber. This distance may be varied in giving the treatment; therefore, in the "Daily Record of Treatment" provision has been made for indicating any change under a heading "f.s.d. cm." Due allowance and correction should be made for changes in distance according to the inverse square law.

19. "r/m air": (roentgens per minute in air). This is the output in roentgens per minute of an apparatus according to the calibrated physical factors used for the technic.

20. A column has been left blank in which to write other data. The Standardization Committee suggest that the "roentgen value" for the technic be recorded in this column. This factor is determined under the following standard conditions: A standard distance of 100 cm. shall be used from the tube focus to a standardized thimble ionization chamber placed in the center of a beam which is delineated by a 10 x 10 cm. diaphragm placed 20 cm. above the chamber (or 80 cm. from the tube focus). Under these standard conditions, the r/m measured in air (back-scattering excluded) gives the "roentgen value" for the other technical factors used.

21. Under the heading "Daily Record of Treatment" are columns, lines, and spaces for recording the data concerning the application of the technic used throughout the course of treatment.

22. "Day": This column is provided for recording the sequence of days or the order in which different regions of the body and fields may be irradiated. The total number of days over which the course of treatment extends will be indicated.

23. "Date": This column is used for recording the date on which any treatment is given. It is suggested that the year number be written immediately under "date" at the head of the column and the month and day numbers in the spaces below.

24. "Tech" (Technic): In this column should be written the letter or number designation of the technic used on the day a treatment is given. It refers to the technic described above under the heading "Calibrated Physical Factors." Example: A theoretical case of a patient with an epithelioma of the lip. The radiologist decides that first he will treat the lesion with a technic which he has chosen to designate by the letter "A", and later to treat the cervical gland-bearing area by another technic which he has designated "C". He will have written in the description of the physical factors for each of these technics under the heading "Calibrated Physical Factors" as previously explained. Then on the days that "A" technic is used, he will write that letter in the "tech" column in the "Daily Record of Treatment" and likewise he will write "C" in this column on the day he uses this other technic. This obviates the necessity for rewriting the full details of each technic daily.

25. "Region Treated": The region of the body treated may be indicated by appropriate descriptive names, abbreviations, initials or numbers as preferred and should refer to anatomical diagrams which have been made showing the fields and disease areas treated as previously suggested.

26. "Field Size": The size of the field irradiated should be given in linear dimensions in cm. of the area on the skin which is irradiated and should not be expressed in square cm. The size of the field may differ from the beam size used in making the calibrations for the technic and therefore the r/m air also will be different. Consequently, due allowance and corrections should be made for changes in the quantity (r/m air) which will result. Note: When oil immersed tubes are used, it will be necessary to make direct and individual measurements for each change in field dimensions which differs from the "beam size" used in calibrations because of the scattering from the oil in the container.

27. "f.s.d. cm.": (Tube focus, skin distance): The focus skin distance used in treatment may differ from that used in making the calibrations for the technic and therefore the r/m air also will be different. When the distance is varied, due allowance and corrections should be made for the changes in quantity (r/m air) which will result according to the inverse square law. Note: When oil immersed tubes are used, the inverse square law cannot be applied; therefore it will be necessary to make direct and individual measurements for each change in distance which differs from the distance used in calibrations because of the amount of back-scattering from the oil in the container.

28. "min." (minutes of treatment) represents the number of minutes used for the treatment in order to give the predetermined quantity on the skin.

29. "r air" (roentgens in air) indicates the number of roentgens in air that will be given in the period of time (minutes) over which the treatment is given according to the physical factors for the technic employed and after corrections have been made for any changes in the size of the field or distance.

30. Under the group heading "Quantity on the Skin" are columns for recording the quantity of radiation in roentgens which affect the skin according to the technical factors used. All the calibrations or calculations of quantity which have been made previously have been based upon measurements made in air. When a treatment is given according to the conditions which have been recorded, a certain amount of back-scattering will affect the skin and this must be taken into consideration. The Standardization Committee is fully aware that the roentgen may not be a true measure for tissue effects according to strict physical criteria and the definition of this unit; however, in practice it is very convenient and satisfactory to express and think of tissue dosage in roentgens. Therefore, in designing this chart, provision has been made for recording in roentgens the quantity of radia-

tion which affects the tissues including direct and back-scattered radiation. The quantity on the skin which includes direct and back-scattered radiation may be measured with standardized thimble ionization chambers placed on the skin in the field or estimated on the basis of charts or curves which show the percentage of back-scattering according to the quantity of direct radiation under the technical conditions used for the treatment.

31. "r/m" (roentgens per minute on the skin). This is to express the rate at which the skin is affected. It may be measured directly during the treatment with a standardized thimble ionization chamber placed directly on the skin in the field of radiation or estimated on the basis of the treatment time in minutes divided into the number of "r air" and in addition allowance and correction must be made for the amount of back-scattering. Example: If the treatment time is 20 minutes to give 200 r air and it is estimated that there is 40 per cent back-scattering, the r/m skin will be calculated as follows: (200 r + 40% of 200 r) ÷ 20 min = r/m skin. 200 r + 80 r = 280 ÷ 20 = 14 r/m on skin.

32. "Field r" (Field roentgens on the skin). This is the total number of roentgens which affect the skin during the treatment. This may be measured with a standardized thimble ionization chamber placed directly on the skin in the field of radiation or estimated on the basis of the "r air" plus the estimated amount of back-scattering radiation. Example: According to the physical factors of the technic, 200 r air are to be given and, after taking into consideration the field size and volume of tissue, it may be estimated that there is 40 per cent back-scattering. Then the "Field r" will be 200 r + 40% of 200 r = 280 r.

33. "Exit r" (Exit roentgens on the skin). This column shows the amount of radiation in roentgens that affects an area on the skin of the side of the body opposite from that being treated. This quantity must be taken into consideration when this opposite area is to be used as a field in the course of treatment and must be added to the amount which is received subsequently. Usually the exit r will be estimated from isodose charts, curves or tables which show depth absorption measurements that are applicable to the technical conditions used for the treatment. Example: A pelvis is to be treated through the suprapubic and sacral fields which are 20 x 20 cm. square and the pelvis is 20 cm. in the anterior-posterior diameter. The suprapubic field is to be given 250 r on the first day. Under the technical conditions employed, the absorption curves might show that 15 per cent of the amount on the surface reached a depth of 20 cm. Therefore, from the suprapubic field, the sacral field would receive 15 per cent of 250 r or 37.5 r as the "exit r." In recording the "exit r" the number should be written on the horizontal line used to record the data concerning the region of the body to which it applies. Thus, in this example, it was the sacral field which received the "exit r" and the amount which it received should be recorded on the horizontal line which will apply to the sacral area. The suprapubic field will not receive exit radiation until the sacral field is treated, then the "field r" and "exit r" in each area will be added together to give the "total r" as will be explained.

34. A blank column has been provided in which to record other data about the quantity on the skin which may be desired. In designing those forms, no provision has been made for indicating per diem tissue losses or for keeping a record according to "saturation" technics. Those who prefer to keep their records according to this plan may use this blank column for recording the estimated percentage per diem losses or may keep any other charts which they prefer in addition to this standard form.

35. "Total r": (Total roentgens on the skin). The total quantity of radiation in roentgens which affects the skin of any field will be the "field r" (including direct and back-scattered radiation) plus any "exit r" which it may receive from another field. Example: As in the previous example, a suprapubic area is given 250 r on the first day and this number will be recorded in both the "field r" and "total r" columns because this area has not yet received any "exit r." However, the sacral field receives 37.5 (15% or 250 r) from the suprapubic field and therefore 37.5 exit r will be recorded on the horizontal line which will apply to the data concerning the sacral field when it is treated. On the second day the sacral field also is given 250 field r. Therefore, the "total r" which the sacral area will have received will be 37.5 r + 250 r = 287.5 r on this second day. The opposite suprapubic field also will have received the same amount on the second day.

36. The objects of recording the quantity on the skin according to this plan are (1) to insure taking into account all of the radiation which any area receives, (2) to be able at any time to ascertain the amount that any area has received to date, and (3) to be able to determine without any other calculations when a pre determined or skin tolerance quantity has been given. These objectives are reached quite easily by summing up from time to time the total amount which already has been given or by keeping a running total of the amount of radiation which has been given to each field at preceding treatments in the "total r" column. There are two methods for keeping the running totals.

First method: As each field is treated, the record is kept in the sequence in which treatment is given and the amount which has been administered previously is added to that given on the day of treatment. By keeping this running total for each field from day to day, there is no necessity for adding the total r at another time because the amount already given on each area will be indicated at all times and when the predetermined quantity or skin tolerance is reached the treatment may be discontinued.

The second method: When planning the course of treatment for the patient, the radiologist decides how many fields he will use and the amount which he expects to give on each. Then he may allow a certain number of horizontal lines in sequence for each field and either keep a running total or add the "total r" for each field from time to time until the predetermined amount is reached.

36. The heading, "Quantity on disease" gives a record of the amount of radiation that is applied in the disease tissues.

37. "%" (per cent): This is the percentage of the surface radiation that reaches the center of the disease area as estimated on the basis of absorption isodose curves, charts or tables. Example: It may be found that 50 per cent of the surface dose reaches to the depth of the center of a disease area. The number 50 will be written in the "%" column.

38. "r/m" (roentgens per minute) shows the rate in roentgens at which the disease is treated and is comparable to the "r/m" rate on the skin. Example: If 10 r/m might be given on the skin and 50 per cent reached the depth of a disease area, then (50% or 10) 5.0 r/m would be the rate on the disease. There are indications that in the future it may be desirable to treat different types of disease at different rates, and therefore records should be kept which will be desirable for reference in this regard.

39. "Day r" (Day roentgens) is the estimated quantity of radiation in roentgens that the center of the disease area receives on the day of treatment. This is comparable to the "field r" in the "quantity on skin" heading. The number of roentgens is estimated according to the percentage of the skin dose which reaches the depth of the disease. Example: If 250 roentgens are given on a skin field and 50 per cent reaches the disease, the "daily r" to this area will be 125 roentgens.

40. "Total r" (total roentgens) shows the total quantity of radiation in roentgens which reaches a disease area through all the portals which may be used. This may be recorded as a running total in the same manner as the "total r" on the skin was recorded. Example: If on the first day of treatment, 250 roentgens are given on the skin and the disease area receives 50 per cent of this or 125 roentgens as recorded in the "Daily r" column, then on the second day the same treatment and amounts are given through another portal, the disease area will have received a "total r" of (125 r + 125) or 250 r. In this manner it is easy to record the estimates of how much radiation was applied to a particular disease area. From clinical observations of the results obtained, conclusions may be reached about the efficaciousness of this amount of treatment and what quantity of radiation is necessary to eliminate different types of disease.

41. A blank column is left for recording any other data desired. It may be ruled and headed according to the wishes of the radiologist, being used for remarks or for names or initials of radiologists or technicians.

While the Standardization Committee grappled more with the difficult fundamental physical problems in radiology, it tried not to lose sight of the normal clinical problems of a pragmatic nature. This section will deal with a small sampling of such problems. They will be treated more or less as they appeared in the Committee records because it is impractical to follow all the threads of development over the period of about a decade.

In connection with the Bureau's study of radiation quality by matching complete absorption curves (Ref. 38), it was recognized that, while the method might be considered ideal, it was not practical in most clinical operations (see p. 129). While the matter was under discussion by the Standardization Committee, a letter of November 23, 1935, was received by Taylor from Dr. Newell, from which the following is extracted:

"However, there is good reason for trying to set up a method as precise as is practical for the most careful physical measurements that are clinically possible. Copy of absorption curves is undoubtedly the present choice. Until we have something better I am in agreement with your proposition to designate quality in terms of a constant potential and a filter which will yield the same absorption curve.

"It is most inconvenient to compare curves however. The measurements have to be brought to the same scale as the published curves. Moreover, there is as much as 1% error in printed or drawn curves on account of the moisture changes in the paper. I am proposing to substitute for publication of standard curves the publication of standard logarithmic tables. My idea is to take the common logarithm of the transmission through successive millimeters (or 1/2 millimeter) of copper (or tin) and then take the differences in these logarithms all the way down the list. This gives, in effect, a measure of the slope of the absorption curve at millimeter intervals. I have been surprised how delicate this test is. It shows up the tiniest errors in measurement. I might remark that I have been astounded at the great precision of your own published measurements. Nevertheless, I found it necessary to smooth your results (1934) in order to make a demonstration set of the tables of logarithmic differences from 100 kv to 180 kv.

"I hope to have ready to show you a specimen table of logarithmic differences of my own supervoltage apparatus. Inasmuch as I have, however, no certain method of measuring the voltage on it, I can use this only to try to persuade you to do the same thing for known constant potentials."

R.R.N.

Further considerations of the radiation quality questions led Newell, in 1938, to make some more specific proposals relating to the ranges for which half-value layers would be used as an index of radiation quality. His proposals were as follows:

Concerning General Communication #12
June 16, 1938

I very much welcome this opportunity to communicate again on our recommendations at Chicago, 1937. I have studied these all over again to see how well they match my present knowledge and belief. I have the following suggestions:

Section II
Quality as h.v.l. in:

	Cellophane or cellone	up to	h.v.l.	2mm,	above	which
Use	Aluminum	"	"	"	2mm,	"	"
"	Copper	"	"	"	2mm,	"	"
"	Tin	"	"	"	2mm,	"	"
"	Lead	ad lib					

Or may use for all qualities the extinction coefficient $(\tau + \sigma_a + \sigma_0)$ in water.

Statement of h.v.l. is clinically sufficient. If more precision be insisted upon, then the additional statement of kv, filter, or second h.v.l. will any of them bring it to the limit of precision absorption measurements. One need not specify more than one of these. To ask for a complete absorption curve is to be persnickety without increasing precision.

Note that first and second h.v.l. in water is not acceptable as a more precise designation of quality.

I think also the international committee should demand that any lead filter should be followed by at least 1/2 mm of copper, and that any copper filter be followed by at least 1 mm of aluminum. In fact, I think we are sufficiently informed to set forth standard filters:

Aluminum up to 1 mm,
which may be preceded by Copper up to 1/2 mm,
" " " " " Tin " " 1 1/2 mm,
" " " " " Lead ad lib.

Provided that for convenience copper up to 1 mm and aluminum up to 3 mm can be considered standard. Any other filters are nonstandard and must be reported in detail.

For therapy there is no reason longer for yielding to a clinical prejudice on the part of this or that worker that zinc or silver might be found superior.

Section III

(b) I would make this read: Exposure (expressed in roentgens measured in air without backscatter) for each field at each irradiation. The reason for this is that "exposure in roentgens" has a better chance of remaining undisturbed by future advancement in theory and practice of dosimetry than has "tissue dose at the surface."

Note that this suggestion leaves (a) unchanged. For the summarized picture of the whole course of treatment we want, of course, tissue dose in the tumor and tissue dose for each field. These tissue doses can only be summed by including the scattered x-ray.

I am strongly of the opinion that tissue dose (Do etc. of Sec. B, 4.) ought to have a separately named unit. Roentgen ought to be reserved for the unit of primary irradiation without backscatter, i.e. to record what was applied, without taking account of the patient's reaction.

I am actually making a clinical trial of this, with intention of keeping our thinking straight. In our therapy clinic we have engaged ourselves to speak of primary irradiation (roentgens in air) as r, and of tissue dose as rho.

R.R.N.

Upon further consideration of his remarks, Newell submitted these additional comments on the quality ranges for therapeutic x rays:

December 31, 1938

Before the year is out, I hasten to reduce my thoughts to paper concerning designation of the ranges of x-ray quality. The idea brought out at committee meeting was to stick to hvl's and say nothing about voltages. If the division points could be made a fair fit to present clinical practice, that would let us use a simple qualitative designation of the quality range we are talking about whenever we don't wish to specify the hvl precisely.

I have set my suggestions out in a table:

QUALITY RANGES OF THERAPEUTIC X-RAY

	Wave length region	Half-value layers Pb	Sn	Cu	Al
Lead range "Plumbic"	0.05	1	4		
Tin range "Stannic"	0.08		1.5	3	
Upper Copper range "Upper cupric"	0.18			0.7	
Lower copper range "Lower cupric"	0.28			0.2	5
Soft x-ray	0.7				0.5
Grenz ray					

The statute would read:

"Quality of roentgen ray is to be designated by the half value layer in a suitable substance. For the hardest qualities, use lead. If hvl is less than one millimeter Pb, use tin. If hvl is less than 1.5 millimeters Sn, use copper. If hvl is less than 0.2 millimeters Cu, use aluminum. If hvl is less than 0.5 millimeters Al, use cellone, or cellophane."

By making the divisions thus, we see only the upper supervolts will use Pb hvl's. The 300 and 300 kv fellows will use tin. The 200 and 200 kv installations can hardly get out of the copper range.

Inasmuch as small differences in quality are unimportant, it would seem sensible to adjust one's filter to the thickness giving a round number for the hvl value of the transmitted ray. One expects to say "200 kv filtered to hvl, 2 mm Cu". The fact that one uses an odd value, say 0.2 mm Sn + 0.6 mm Cu filter added to get this, needs not be reported, so gives no inconvenience. In using round numbers, one ought to make sure that they are accurate within 20 per cent.*

R.R.N.

*Ed. Note: See also page 223 for Committee recommendations on filtration.

Newell's proposals for quality specifications brought reactions from E. H. Quimby and L. Marinelli, as exemplified by the following:

January 16, 1940

"First, we can see no advantage in using both copper and iron as standards.* There

*Ed. Note: Iron was not mentioned in Newell's proposal.

is a difference of only three in atomic number between them, and a difference of only about 10% in density. Moreover iron rusts and is altogether an unsatisfactory material.

"Second, using six ranges seems unnecessary; five would be plenty. It is perfectly satisfactory to use copper down to a hvl of 0.25 mm., and by that time the aluminum equivalent is about 5 mm. which is also satisfactory.

"Third, to divide ranges at hvl's is--at the present time, at least--a more difficult concept for the average radiologist than to divide them at voltages. To us a reasonable division seems;

above 800 kv - lead
300 - 800 - tin
140 - 300 - cooper
50 - 140 - aluminum
below 50 - celluloid or cellophane.

This is a convenient division, since it means that, for the most part, the change comes as we go from one type of apparatus to another.
Namely

below 50 - Grenz rays or something of the sort;
50 - 140, the various low voltage units;
140 - 300, ordinary deep therapy;
300 - 800, a sort of low supervoltage range, in a good many cases with sealed off tubes. This is the most mixed of the groups;
above 800, supervoltage; every installation an individual at present.

Of course these descriptions of the classifications may change, but the breaks are still reasonable.

"Fourth, the names are bad, for they do not really convey any descriptive idea to most people. If we are going to use the substances for names, we might at least use the English, and not the Latin form. We don't like the idea, although we admit that there is no system at present and we have no better suggestion. I think it is of dubious value, however, to try to force something as artificial as this.

"Fifth, in the next to the last paragraph of the report, he states that there is no need to state the filtration, but he neglects to state that it is extremely necessary to state the voltage. Furthermore the idea of adjusting the filter so that the h.v.l.

comes out an even number sounds pleasant, but may not be practically very easy of accomplishment. Metals come in sheets of definite thicknesses, which are very much more likely not to give whole number hvl's in most cases."

E.H.Q. and L.M.

When, for purposes of reproducing results, it is necessary to accurately specify the total filtration in an x-ray beam, a method described by Taylor and Singer could be applied (Ref. 38) using a series of standard absorption curves to determine the filtration built into a tube (inherent filtration). To do this, it was desirable to use absorption curves made with the same general voltage waveform. This was demonstrated in a communication from R. R. Machlett, a manufacturer of x-ray tubes. His communication follows:

SUBJECT: Determination of "Inherent Filtration"

In giving consideration to the proposal of standardizing on a method of determining and specifying the filtration effect (commonly spoken of as "inherent filtration") of the fixed parts of an X-Ray tube which are in the path of the utilized X-Ray beam, such as the tube wall and, in the case of oil-immersed shockproof tubes, the film of oil and the X-Ray window material, we have made the following survey of methods already in use.

It has been our practice to determine the filtration value of the glass and other materials in our tubes in terms of equivalent thickness of aluminum by comparing the X-Ray transmission of the materials with that of aluminum until the equivalent thickness has been found. We have taken steps to minimize all possible errors in this method, and believe our results are accurate to any required degree. Then by controlling the thickness of materials used during manufacture to proper limits, we are enabled to specify the inherent filtration of our tubes with complete confidence.

However, this method has the disadvantage of not being adaptable to the measurements on finished tubes, in which the thickness or nature of the materials involved may not be accurately known, and hence can not be considered entirely suitable for the purposes for which a standardized method is required.

A method is proposed by Taylor and Singer, in Bureau of Standards Research Paper RP666, which is adaptable to measurements on finished tubes of unknown construction, and hence meets the requirements in this respect. This method consists of obtaining an absorption curve of the radiation from the tube at one or more voltages within the voltage range involved, and "matching" these curves with "standard" absorption curves for a similar voltage range. The inherent filtration is indicated by the amount of horizontal shift in the co-ordinate axes to make the curves match. Proposed standard curves are given in this publication which are made with constant potential, in copper up to 180 KV and in aluminum up to 110 KV.

Certain difficulties have been encountered in applying the above method, using the curves given as a standard. In some cases results have been obtained that seem to be certainly inaccurate, when checked against determinations made by our own method as described in a previous paragraph.

In considering possible reasons for these apparent inaccuracies, it is noted that the absorption curves obtained for the tubes being tested were taken with pulsating potential, whereas the "standard" curves were taken with constant potential. However, the Bureau of Standards paper demonstrates that the differences in absorption curves due to voltage wave-form are automatically self-compensating in the inherent filtration determination. Also, our curves are taken with a thimble-chamber type of r-meter, whereas the standard curves were, presumably, taken with a standard ionization chamber. We understand that there are certain errors in the thimble-chamber instruments at voltages below 100 KV, which might possibly account for the observed inconsistencies. Incidentally, these inconsistencies are not only in the indicated values of inherent filtration, but also in the indicated constant potential equivalent of the pulsating potential wave form employed. In some cases the indicated equivalent constant potential is as high as, or higher than the measured peak value of the pulsating voltage used.

Braestrup has made some inherent filtration determinations for us by a method which gave results checking our own very closely. This method is similar to that described in the Bureau of Standards paper, with the exception that "standard" curves for the comparison were obtained from a tube of Pyrex glass of known wall thickness, to which an accurate value of inherent filtration could be assigned, using the same generator and

same r-meter with which the tubes being studied were tested. Thus any errors due to voltage wave-form and instrument inaccuracy were eliminated.

On the basis of these experiences, we can suggest the following procedure for arriving at a standardized method of inherent filtration determination for a voltage range up to 100 KV.

Provide a "standard" tube with as low inherent filtration as possible, for which is suggested a Pyrex tube with a special blown Pyrex window as thin as is practicable for 100 KV operation. Obtain "standard" absorption curves with this tube, employing pulsating potential from a type of generator which is universally available, for instance a 4-valve full-wave generator, and using a Victoreen thimble-chamber r-meter, of specific chamber size, a type that is commonly available everywhere. Then if these same conditions are present when taking the absorption curves on the tubes being tested, any errors due to wave-form or chamber characteristics should be eliminated.

The thin-window tube is suggested so as to reduce to a minimum any error in the assumed equivalent inherent filtration of the standard. It would also be advisable to investigate further the extent to which wave-form will influence the accuracy of the results, since it is known that the length of cables employed with shockproof tubes affects the wave-form and the quality of output to a very marked extent. Unless it is found that this factor has little bearing on the inherent filtration determination by the standard absorption curve method, it would be necessary to specify rather definitely the test conditions so as to insure a wave-form sufficiently close to the one on which the standard was based to eliminate error from this source. These conditions would involve cable dimensions as well as fundamental wave-form characteristics of the generator.

These preliminary considerations seem to indicate that such a method would give results with a reasonable degree of accuracy, and would meet the requirements of universal applicability. We suggest a further investigation of its possibilities.

R.R.M.

The meeting of the Standardization Committee in December 1936 brought to a head a number of problems and issues that had been under discussion for several years. Of particular interest to the National Bureau of Standards was one related to the Bureau's proposal that a number of laboratories meeting specific requirements should be designated as suitable for calibration of clinical instruments. This proposal was adopted.

Also, there were further certifications for physicists meeting the requirements laid down earlier. The full report of the Committee (RSNA, 1937) is given on page 147.

It seems that much of the year of 1939 was taken up by detailed discussion of the 1938 Report and Recommendations by the Standardization Committee. While this was a slow process, it nevertheless drew out a great many points of interest, some of which will be included below.

The following report was finally agreed upon and published in 1940:

[Reprinted from RADIOLOGY, Vol. 35, No. 1, Pages 105–108, July, 1940]

REPORT OF STANDARDIZATION COMMITTEE

1. *The Specification of Quality.*—It was suggested that, for clinical purposes, quality may be expressed somewhat loosely since small quality differences are not important. In cases in which purely technical considerations are involved the quality should be measured carefully and in accordance with our earlier recommendations.

It is strongly suggested that the materials used in quality measurements be limited to the following: cellophane, aluminum, copper, tin, and lead (in accordance with the recommendations of the I.C.R.U.). At the same time, the filter materials used in clinical practice should be limited to the above materials. Filters of zinc, silver, iron, etc., should not be used. Particular thicknesses of filter for clinical use should not be specified by the Committee. Provision must be made for filtering out radiation that may be transmitted through the heavier filters in the region of their characteristic absorption limits.

The limits defining the regions in which different filter materials are used for both treatment purposes and quality measurements were set forth by the I.C.R.U., but the Committee is of the opinion that the method of specifying the limits is open to objection. The I.C.R.U. proposals give these limits in terms of the tube voltage without regard to the thickness of the filters involved. This may arbitrarily necessitate the use of filters which are too thin to permit of proper handling and protection from damage. Moreover, with much of the present-day equipment, the tube voltage cannot be measured directly and, even if known, may be misleading since the quality of the radiation is so strongly dependent upon whether the filament is biased or whether the wave form is constant, pulsating, etc.

It is recommended, therefore, that the limits of the application of any particular filter be specified in terms of the half value of the radiation and hence, indirectly, in terms of the filter material. In any case, the lower limits to the filtration should not be taken below those thicknesses for a particular material, where it can be safely handled without possible damage, or below which its thickness can be made and measured within a maximum error of ±2 per cent.

Accordingly, the following limits and filter materials are recommended:

For qualities measured in h.v.l.—

Use Cellophane up to h.v.l. = 2 mm., above which
Use Aluminum up to h.v.l = 2 mm., above which
Use Copper up to h.v.l. = 2 mm., above which
Use Tin up to h.v.l. = 2 mm., above which
Use Lead, for all h.v.l.

For treatment, any of the above filter materials may be used, when properly followed by filter material of lower atomic number, as follows:

Primary Treatment Filter	*Secondary Filter*
Lead	1.5 mm. tin + 0.25 mm. copper +1 mm. aluminum
Tin	0.25 mm. copper +1 mm. aluminum
Copper	1 mm. aluminum
Aluminum	None
Cellophane	None

The secondary filters shall be added between the primary filter and the patient in the order shown, *i.e.*, in the order of decreasing atomic number. In other words, the use of any filter of heavier atomic number than aluminum requires the addition of a minimum thickness of the next specified filter material, between it and the patient.

2. *Scope of the Committee's Activities.*—The question was raised by a member of our Committee as to whether it was within the Committee's province to deal with any matters other than those relating directly to standards

and units. Present at the discussion were several members of the Committee who have served actively since its inception about twelve years ago, including its originator and first chairman. It was brought out that our originally intended function was not only to foster the adoption of units and the establishment of standards but to consider and make reports on all physical problems relating to the application of x-rays and assist in the education of the radiologist on the use of physical methods in his clinical problems.

It was decided that future reports of the Committee be in three separate categories:

(*a*) The "shall" category, which represents adopted things, things which the Committee believes are established beyond all reasonable doubt;

(*b*) The "should" category, representing recommended procedures, methods, etc., which, though not obligatory, appear to be most desirable at the time; and

(*c*) The "permissible" category, pertaining to things not harmful, but upon which the Committee does not wish to take stronger action.

Regarding the American Medical Association proposals for x-ray equipment standardization and performance standardization, the Committee wishes to go on record as being positively opposed to the recognition of any laboratory as the sole agent of establishing such standards. It feels that any such establishment of standards should come through some suitably representative body which can be in a free position to judge the merits of any related investigations carried out either by its request or by some independently operating laboratory.

3. *Use of the Term "Roentgen."*—The Committee approved the use of the "roentgen" as a unit for the specification of x-rays traversing a region in any material, *e.g.*, air or tissues, etc., at any point. It would recommend, however, that it should be clearly indicated whether the specified number of roentgens refers to total, primary, or secondary radiation. The interpretation of a stated quantity of roentgens in any material is based, according to present usage, on the principle that equal numbers of ergs per square centimeter of x-radiation of a particular spectral distribution are specified by the same number of roentgens regardless of the material traversed.

4. *Depth Dose and Back-scattering Data.*—A proposal was made that the Committee adopt some specific data regarding depth dose and back-scattering as a function of voltage, field size, filter, etc. It was decided to collect and review critically the available data and prepare therefrom suitable tables and charts for use by the radiologist, without formally adopting them. A sub-committee, consisting of Dr. Laurence and Dr. Quimby, was appointed to carry out this task. This work has been largely completed and is to be published in the Journal at an early date. Reprints of this report will be obtainable through the Secretary, Dr. Donald S. Childs, in accordance with instructions given at the end of the report.

5. *Timers for X-ray Treatment.*—The Committee's attention was directed to the fact that in many cases, timers for therapy were not sufficiently accurate for the shorter treatment intervals. The following tentative recommendations were agreed upon:

(*a*) Timers shall be accurate and permit of setting within 2 per cent of any part of the scale for which they will be used, down to intervals of two minutes.

(*b*) For intervals below two minutes, single-cycle timers shall be used.

(*c*) Present use of timers not meeting the above requirements is to be condemned.

(d) Any starting and stopping devices shall be positive in their action.

(*e*) Timers shall be checked frequently.

(*f*) Short, high dosage rate treatments should be hand-timed in addition to any electrical or mechanical timing.

Use of recording timers was suggested to avoid "blunders" in recording treatment factors.

It was also suggested that the Time Section of the National Bureau of Standards be approached to assist in the testing of timers. (This has been done and has met with favorable response.)

A sub-committee under Dr. Weatherwax was appointed to follow the question through.

6. *Need for Standards in Diagnostic Field* (Dr. Henny).—It can be shown that a properly exposed roentgenogram which has great photographic contrast and the highest degree of shadow sharpness will contain more information of the part under investigation than a roentgenogram which is under- or over-exposed and has a lower degree of contrast. Because of personal idiosyncrasies many roentgenologists will prefer a roentgenogram lighter or darker than this "ideal" standard. It appears, however, that more and more roentgenologists are

becoming satisfied with such an "ideal" roentgenogram. It is probable that the individual likes and dislikes of the roentgenologist, as far as the characteristics of such a roentgenogram are concerned, could be taken care of by varying the intensity of the viewing light. Such being the case, the aim of standards in the diagnostic field would be the production of roentgenograms (*a*) having the greatest amount of detail in the shadows, and (*b*) showing photographic densities which are comparable with corresponding roentgenograms of the patient made in the same or in different roentgen-ray departments at other times.

The unmeasurable variability of different patients and the required degree of photographic latitude must be taken into consideration in planning a standard roentgenogram of a given part. These factors, together with the limitations of the roentgenographic equipment, require in many cases that a compromise be made. The particular roentgenographic factors for each examination can be arrived at with relative ease although it will be impossible to satisfy everyone. A simple factor such as tube–film distance, for example, varies over quite a wide range in different roentgen-ray departments and roentgenologists are prone to resist changes in technic.

The object of standardizing diagnostic roentgenography must be to obtain roentgenograms of standard and superior characteristics, regardless of the roentgenographic or processing equipment employed. Since so many factors (focal spot size, voltage wave form, tube current, intensifying screens, film developing characteristics, etc.) go into making each roentgenogram, it is impractical as well as expensive to standardize each one of these. By making a roentgenogram according to predetermined and proven technical factors of an aluminum stepladder of known specifications, and by processing the film under "standard" conditions in fresh solutions which are available on the market, a standard stepladder film would be available. A roentgenologist wishing to check his diagnostic machine would make roentgenograms of an identical aluminum stepladder using technical factors as nearly like those employed in making the standard as he could. After processing the film according to his routine method, he would cut it so that the ladder step shadows could be placed side by side with those of the standard over an appropriate mask on a viewing box. A close inspection would tell which was darker (faster) and which had the greater contrast. He would then have an idea as to where he stood in regard to the standard. X-ray technical factors, and possible intensifying screens and developing solution, would have to be changed until the test film was equal to the standard. It would still be necessary to measure the x-ray tube focal spot size and to test for intensifying screen grain and contact. The focal spot size may be determined from a pinhole picture and the intensifying screens are tested by a roentgenogram of a fine wire (No. 26) mesh which is placed directly on top of the cassette. From these findings, together with the tube–film distance used in roentgenography, the geometrical unsharpness can be estimated and it may be found whether or not this is within some arbitrarily set standard. Use of a small test aluminum ladder placed on the corner of the cassette beside the patient has been suggested. The shadows of this ladder, as shown on the film, may be compared from time to time with those on films taken previously to determine the status (as far as speed and contrast are concerned) of any roentgenogram. If the roentgenogram is sent to another roentgenologist, the latter, when making a roentgenogram of the same patient, may learn to judge the technical factors to be used by comparing the ladder shadows with those of a duplicate ladder on his own films. He would then obtain a roentgenogram of essentially the same density and contrast as those of the other films, and all the clinical roentgenograms could be directly compared.

7. *Listing of X-ray Physicists.*—The following names have been added to our lists.

December, 1938.

L. B. Leppard, Department of Health, Ottawa.
S. S. Sidhu, University of Pittsburgh.
J. E. Morgan, Duke University.
B. R. Stephenson, Buffalo City Hospital.

December, 1939.

Dallet B. O'Neill, Philadelphia.
S. Reid Warren, Philadelphia.

The name of J. D. Leitch, Canada, is removed from the lists without prejudice. He is now associated with a commercial concern.

The Committee agreed upon the following:

(*a*) All radiologists should seek calibration advice from a listed physicist before undertaking their own calibration work.

(*b*) The radiologist should be encouraged to get advice from a listed physicist regarding the

calibration and protection of any new equipment in advance of its purchase and installation.

(*c*) The Committee reaffirms its stand against the dosage calibration of x-ray equipment by manufacturers or their regularly employed agents. Any calibration by a manufacturer (for specification purposes) shall be considered as provisional, and shall be checked as soon as possible by a listed physicist.

(*d*) All dosage calibration shall include a statement or estimate of the inherent filtration of the tube and housing.

8. The following is the complete listing of x-ray physicists:

Blatz, I. H., Dept. of Hospitals, 414 E. 26th St., New York City.
Braestrup, C. B., Dept. of Hospitals, City of New York.
Cole, K. S., College of Physicians and Surgeons, New York City.
Corrigan, K. E., Harper Hospital, Detroit, Mich.
Demers, J., University of Montreal, Montreal, Canada.
Exner, F. M., Crocker Institute, New York City.
Failla, G., Memorial Hospital, New York City.
Glasser, Otto, Cleveland Clinic, Cleveland, Ohio.
Harrington, E. L., Univ. of Saskatchewan, Saskatoon, Canada.
Henderson, G. H., Dalhousie University, Halifax, Nova Scotia.
Henny, G., Temple University Hospital, Philadelphia.
Jacobson, L., Montefiore Hospital, New York City.
Laurence, G. C., National Research Council of Canada, Ottawa.
Lauritsen, C. C., California Institute of Technology, Pasadena, Calif.
Landauer, R., 1317 Judson Ave., Highland Park, Ill.
Leppard, L. B., Department of Health, Ottawa, Canada.
MacDonald, P. A., Cancer Institute, Winnepeg, Canada.
Marinelli, L. D., Memorial Hospital, New York City.
Morgan, J. S., Duke University, Durham, N. C.
Mutscheller, A., Post-graduate Hospital, New York City.
Nurnburger, C. E., Redmon, Ill.
Omberg, A. C., Vanderbilt University Hospital, Nashville, Tenn.
O'Neill, Dallet B., Moore School X-ray Laboratory, Philadelphia.
Pugh, R. E., 1944 Summit Avenue, Pasadena, Calif.
Quimby, Edith H., Memorial Hospital, New York City.
Reinhard, M. C., State Institute for Study of Malignant Disease, Buffalo, N. Y.
Robertson, J. K., Queens Univ., Kingston, Ont.
Rose, J. E., Marine Hospital, Baltimore.
Rovner, L., Ryerson Laboratory, University of Chicago, Chicago, Ill.
Schwarzschild, M. M., Beth Israel Hospital, New York City.
Shrum, G. M., University of British Columbia, Vancouver, B. C.
Sidhu, S. S., University of Pittsburgh, Pittsburgh, Pa.
Singer, G., National Bureau of Standards, Washington, D. C.
Stenstrom, W., University of Minnesota, Minneapolis, Minn.
Stephenson, B. R., Buffalo City Hospital, Buffalo, N. Y.
Taylor, L. S., National Bureau of Standards, Washington, D. C.
Ulrey, C. T., Bloomfield, N. J.
Vigness, Irwin, University of Minnesota, Minneapolis, Minn.
Warren, S. Reid, Moore School X-ray Laboratory, Philadelphia.
Weatherwax, J. L., Philadelphia General Hospital, Philadelphia.
Williams, M. M. D., Mayo Clinic, Rochester, Minn.

Submitted by the Committee.
Lauriston S. Taylor, *Chairman*
U. V. Portmann, *Sub-chairman*
Edwin C. Ernst
W. Edward Chamberlain
Robert R. Newell
Arthur W. Erskine
Robert B. Taft
Richard Dresser
Otto Glasser
Edith H. Quimby
W. Stenstrom
J. L. Weatherwax
George C. Laurence
George C. Henny
Kenneth E. Corrigan

Meanwhile, Drs. Quimby and Laurence had been assigned the task of preparing what would be designated as Standardization Committee Technical Bulletin No. 1. This turned out to be a monumental task (see p. 231). It was indeed one of the definitive reports of its time on the measurement of radiation dose in roentgen therapy. Much of it is still regarded as applicable although our present day techniques have far outstripped those which were available in 1940 (RSNA, 1946).

A suggestion that the Standardization Committee prepare a suitable glossary of terms was examined seriously and initiated. Unfortunately, like many subsequent starts, the effort was found to be such a thankless task that it was never carried through to fruition. Meanwhile, glossaries were prepared by the American Standards Association, the Institute of Electrical Engineers, and others, but these were slanted towards special interests other than radiology. While these organizations showed little hesitancy in defining terms for use in radiology, they had little contact with the radiological profession and seemed unconcerned to make any. The last attempt to develop a suitable glossary was undertaken at the National Bureau of Standards in the early 1960's, but, as in other cases, the participants lost interest and the project collapsed.

From time to time, questions were raised about the required accuracy of the various factors used to describe therapy treatment conditions. Responding specifically to a letter to the editor in Radiology, Newell prepared a statement to be carried as an editorial in the journal. The statement which was circulated to the members of the Standardization Committee for comment was never published; it follows:

PRECISION IN DOSIMETRY
R. R. Newell
(January 24, 1940)

We have all heard of the patient who took a swig from her bottle of digitalis tincture whenever she felt she needed it. It is quite usual for physicians to direct the patient to measure out his medicine with a teaspoon. Yet we expect the pharmacist to weight the ingredients of the prescription precisely.

The Standardization Committee of the R.S.N.A. is preparing a handbook on dosimetry which will contain tables of surface (skin) dose and depth (tumor) dose. These will be on the basis of 100 r applied and will be given to the nearest whole number. The tables are founded on careful measurements on phantoms by physicists who presumably can work more precisely than a doctor making measurements on patients. Does this mean that roentgen therapists work and records can now be accurate to one percent? His dosemeter is "guaranteed" accurate to one or two percent. The several national physical bureaus agree within a half percent.

The fact is that the error in clinical dosimetry is a composite of many errors, some of which are much larger than the errors of commercial dosemeters. The following would be our guess concerning everyday work by ordinarily careful radiologists.

Time: Measured to half a minute in ten minutes--an error of 5% in dosage.

Distance: Measured to one or two centimeters in fifty--an error of 4% or 8% in dosage.

Voltage varies several percent from moment to moment, but probably the average would run only a volt or two away from the intended setting--an error of 1 percent in voltage, i.e. 2 percent in output.

These three can easily be made more precise. Moreover it is not even difficult, nor very time consuming to bring their error down to the one percent expected from the dosemeter.

But in biologic reaction to radiation, we are much less sure. Precision with drosophila eggs can be brought within 5% by averaging hundreds of eggs. But the clinician is working on a single individual and this individual may differ a good deal from the average. We often meet deviations of ten percent, and occasionally see "idiosyncrasies" amounting to 40 or 50 percent above "normal" sensitivity to x-ray.

The erythema produced by 400 r can hardly be distinguished from that produced by 500 r. In fractionated dosage most of us feel we can see smaller differences, maybe 10 percent. We could expect to see a break in the progress of development and fading of erythema on changing from 200 r to 175 r, applied alternate days (12% reduction).

The uncertainties of dosimetry therefore lie for all practical purposes in the clinical field. And the radiologist who wishes to work more precisely will redouble his effort in this field, for it is only here that halving the error will appreciably reduce the total resultant error. It would be a great misfortune if the precision of the

tables in our projected handbook should operate to mechanize the application of radiation. It would be a step backward if it should lessen the radiologist's daily inquiring observation of the patient and his reactions. It will result in disaster if the radiologist in narrowing his attention to catch a few roentgens should slip into a blunder amounting to several erythema doses.

In treating diseases where x-ray is much used, we would suppose treatment could be planned with a precision of 10 percent. The application of the planned treatment should certainly be careful enough to insure against errors of more than half this, i.e. 5 percent. If error of 5 percent is the permitted resultant from four component errors (time, distance, roentgen measurements, part), then the permissible error in any one must be kept below 2 1/2 percent.

$$(\text{Resultant error})^2 = (E_1)^2 + (E_2)^2 + (E_3)^2 + (E_4)^2*$$

*The theorem applies to probable error where the several components are of equal importance. But we believe it gives a good enough idea in our case, where we'd prefer to talk about maximum error, and where we can't say which of our measurements is most likely to be inaccurate.

Notice that improvement in a single measurement doesn't do much good unless the other measurements are improved at the same time.

The conclusion is that the physicists can't do the radiologist's dosimetry for him, they can only provide him with the tools. In using them he has to watch everything, but should not forget above all to watch his patient.

NOTE by L. S. Taylor: Just because there may be a large biologic uncertainty, there is no excuse for tolerating sloppy physical measurements where a little effort will yield satisfactory measurements. This will lead eventually to complete degradation in the whole therapy technique.

DEFINITIONS

Radiation - energy streaming from the source. In x-ray therapy it is measured according to its ability to ionize air.

Irradiation - The application of radiation to a body (patient). Primarily an act, it has also its quantitative aspect. Compare the similar optical term "illumination".

Exposure - primary irradiation (measured in air), (secondaries from filter and cone etc. would be included for they can't always be avoided).

Surface dose (skin dose) - irradiation in the skin at the surface (exposure plus backscatter).

Tissue dose (tumor dose) - irradiation (sum of primary and scattered) at a given place within the patient.

Depth dose - ratio of tissue dose at 10 cm (or a specified) depth to the surface dose.

Ionization - the production of charged particles (electrons, atoms, groups of atoms). The ions produced measure the energy absorbed from the radiation. One roentgen of soft x-ray equals one roentgen of hard x-ray, therefore, not in total radiant energy streaming through, but rather in ability to produce the same ionization in substances composed of light atoms.

R.R.N.

Further examples of the confusion developing over the use of new terms were demonstrated in another communication by Newell in March 1940. In this he was commenting on the draft of the Standardization Committee Technical Bulletin No. 1. Because Newell was undoubtedly better versed in physics than other radiologists of that time, his comments provided a strong indication of the problems facing the Standardization Committee. They follow:

To Members of the Standardization Committee:
(March 1940)

I suspect it is at least in part due to my pleading that the subcommittee has adopted the word "exposure". It is therefore most distressing to find that I have sold the idea all right, only I sold the wrong one!

The subcommittee has set the word "exposure" equal to "dose" and preferred to it. I don't see any disadvantage to the word "dose". To be sure I don't like the decision of the international committee to make tissue dose measurable in terms of irradiation instead of in terms of ionization. But, being compelled to accept that decision, I see no drawback to using the word "dose" for the concept.

What I am interested in is to get different words for these two things, namely the primary beam (roentgens measured in air) and the skin dose (and tissue dose), roentgens at a point in or on the patient, scattered and primary added together.

Apparently only a physician-radiologist (and never a physicist) is able to see the advantage of having terms to distinguish the quantitative application of x-ray from the volume effect of the x-ray--roentgens in air from tissue dose. Maybe the reason is that physicists don't have to read doctors' published reports and private letters summarizing treatment given and using words which fail to tell whether he means that he applied 2000 r (in air) to each of 4 fields, or measured 2000 r in the center of each of the fields, or (believe it or not) that he measured 500 r in the center of each field and has added all 4 fields together.

It's no use saying he ought to make his meaning clear. The fact is he so often doesn't--seems unable to. I feel sure that the possession of two separate terms, one always used for roentgens in air and the other always used for total radiation in skin or depth would help the clarity of the average doctor's writing (and thinking).

I propose, therefore, that our committee use the word "exposure" strictly and only for primary irradiation (roentgens in the beam, contaminated by scattered ray from filters and cones, because that's unavoidable, but without including scattered x-ray from the patient).

And we would then leave "dose" as at present defined by the international committee--total radiation at the point considered. We would use "dose" in our report only in the compounds "dose meter", "skin dose", "surface dose", "exit dose", depth dose", "tumor dose", "tissue dose". In this report the word "dose" would not ever stand alone.

I have gone through the report and made the necessary changes to conform with this dichotomy of terms. This corrected (in this regard only) copy I have sent to Mrs. Quimby. This argument I am sending to all members of the Standards Committee. (See Bulletin No. 1, p. 231.)

R.R.N.

Newell's comments on the draft Bulletin were considered and many were incorporated into the final copy. However, he had brought up some basic issues which continued to be discussed for a number of years--even into 1980.

In responding to Newell, Laurence tended to blame much of the difficulty on the confusion between dose and absorption. His response follows:

April 8, 1940

I have just been reading your memo, on the Bulletin. I agree with most of your suggestions; in a few cases I would like to alter them slightly--about which more later. At present I am concerned chiefly about your proposals regarding the use of the word "exposure".

The question is a very important one and I am anxious to express my point of view to you and to the rest of the committee. The easiest way to do this is for me to send them copies of this letter, which I am sure you do not mind.

Confusion of dose and absorption (or sometimes ionization density) is very common. It is responsible for much sloppy thinking, inaccurate and often meaningless description of treatment and dosage, and general misunderstanding of the whole subject. It is forever turning up and causing difficulties. Hence in describing how to measure dose, our first concern is that our readers know what dose is, and what dose is not.

Much of the cause of this confusion is in the word "dose" itself. It suggests that it refers to what the tissue gets, takes in, absorbs.

Actually it refers to what is traversing the tissue. "Flux" might be a better name for it, but objections have been made to that word. "Exposure" is particularly suitable. The region in the tissue, or in the air, is exposed to the stated number of roentgens. The word does not suggest what happens to the radiation in the region exposed.

To introduce the word gradually we suggest it as an alternative to "dose". The radiologist or physicist may continue to say "dose" if he prefers, until the other word is more familiar.

I have misgivings about adopting the proposal that "dose" should be reserved for radiation traversing skin and other tissue, and "exposure" for radiation traversing air. The very contrast in the use of the terms in this way emphasizes the misleading suggestion of the term "dose". It would tend to foster the confusion about which I complain. Moreover it would suggest that 500 r in free air is a different kind of physical magnitude than 500 r in tissue. This is not. The differences are in the material and in consequence what this material does to the radiation.*

*Ed. Note: While "material" might modify exposure readings, as we now understand them, all of the ions are produced by electrons generated in air-like material surrounding the cavity.

It is doubtful that your proposal would achieve the end you desire. Is the man who talks of a "dose" of 1000 r., who neglects to say "free air dose", "skin dose" or "tumour dose", likely to use with discrimination the two terms which you propose should replace these expressions? Is not such a careless person more likely to use the two words indifferently with the final practical consequence for him that they become synonymous? In other words, is not the use proposed in the present draft of the bulletin likely to prevail in spite of intentions otherwise?

Carelessness in the use of terms cannot be corrected by changing the terms. It can only be corrected, if at all, by direct appeal. Thinking over those who might make such an appeal--in the pages of Radiology for example--I believe that you could do it most effectively. How about it?

G.C.L.

Having discussed some of the problems associated with the preparation of Bulletin No. 1, it is included here in its final form.

[Reprinted from RADIOLOGY, Vol. 35, No. 2, Pages 138–159, August, 1940.]

THE RADIOLOGICAL SOCIETY OF NORTH AMERICA STANDARDIZATION COMMITTEE, TECHNICAL BULLETIN NO. 1*[1]

By EDITH H. QUIMBY *and* GEORGE C. LAURENCE, Members of Subcommittee

THE MEASUREMENT OF DOSE IN ROENTGEN THERAPY

CHAPTER I.—THE MEANING OF DOSE, EXPOSURE, AND OTHER PHYSICAL TERMS USED

AS the art of radiology has developed, the significance of many of its technical terms has been altered, modified, or broadened to suit new methods or to conform with new knowledge. The meanings of the physical terms have now become fairly well established; nevertheless, the earlier changes have resulted in a vagueness in the use of these words that is responsible for many of the difficulties encountered in measuring x-rays. The terms that are used in this bulletin are discussed below with attention to some of the misunderstandings that frequently lead to difficulties.

The *exposure* or *dose*[2] is a measure of a property of the x-rays at a particular place. This place may be situated in air, in tissue or other material, or even in vacuum. The exposure is expressed as a number of roentgens.

The exposure is a property of the radiation only. The number of roentgens is a partial description of the x-rays at the place where it is measured or calculated. It tells nothing about what happens to the radiation or about the effects which it produces. It does not indicate whether the radiation is absorbed or passes through the region without absorption. It tells nothing about the material which it penetrates. It applies only to the radiation itself.

The average *exposure rate*, *dosage rate*, or *intensity* is obtained by dividing the exposure by the time required to deliver it. Hence the exposure is the product of the average exposure rate and the length of time it continues. The exposure rate is usually expressed in roentgens per minute.

The exposure may be measured with an instrument called *a dosemeter*, *exposure meter*, or *roentgen meter*. The dosemeter is intended to measure the exposure only. The reading obtained with the instrument should not be considered as a measure of the absorption of the radiation or of the ionization produced by the radiation. The absorption and ionization necessary to operate the instrument are not the effects that the dosemeter has been calibrated to measure. Exposure, absorption, and ionization are three very different physical effects and should be distinguished clearly.

Furthermore, the difference in exposure at two places, one of which is farther from the x-ray tube, is not a measure of the energy absorbed between the two places. The difference is due to three causes: divergence of the radiation, true absorption, and the presence of scattered radiation.

The *divergence* of the radiation refers to the increase in cross-section of the x-ray beam as the distance from its source increases. This increase in size of the x-ray beam implies that the radiation is more widely distributed and, therefore, less intense. The intensities (exposure rates) at points along the central axis of the x-ray beam are inversely proportional to the squares of the distances from the focal spot of the x-ray tube *if there is negligible absorption between the points and negligible scattering from objects in the beam.*

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* The Editor wishes to state that extra copies of this Bulletin may be obtained from the Secretary, D. S. Childs, M.D., 607 Medical Arts Bldg., Syracuse, N. Y., at a small cost.

[1] Approved at the Twenty-fifth Annual Meeting, at Atlanta, Dec. 11–15, 1939.

[2] The two terms "exposure" and "dose" are used interchangeably in this text. "Exposure" is preferred because it calls attention to the fact that it describes the radiation to which the place is exposed. "Dose," by unfortunate association of ideas, may suggest incorrectly that it refers to the radiant energy that is absorbed. (See third paragraph of the text.) The older term, "quantity," which is unsatisfactory, is avoided.

Example: At 25 cm. from the focal spot the exposure is 150 r.
Hence at 50 cm. from the focal spot the exposure is

$$150\ \text{r} \times \frac{25\ \text{cm.} \times 25\ \text{cm.}}{50\ \text{cm.} \times 50\ \text{cm.}} =$$

$$150\ \text{r} \times 1/2 \times 1/2 = 37.5\ \text{r.}$$

This rule is called the *inverse square law.* It applies only to radiation received directly, without scattering or absorption, from a small source such as the focal spot of the x-ray tube. Hence it should not be used for accurate calculation of the exposure in cases in which there is appreciable scattered radiation, *e.g.*, near the filters, cooling oil, and the aperture of the treatment cone if the inner surface of its walls is exposed, near walls and furniture, or when the patient is in the x-ray beam. Since scattered radiation is difficult to avoid, the inverse square law is more useful for approximate estimates in planning treatment than for the final determination of the exposure. The inverse square law enables us to calculate the effects of divergence, but it does not take into account the two other factors that can contribute to the difference in exposure at two places: absorption and the presence of scattered radiation.

The word *absorption* is used in radiology with a restricted technical meaning which should not be confused with its more general significance or with casual colloquial usage. Technically, absorption involves two processes, *viz.* (1), deflection or change in direction of some of the photons[3] of the x-ray beam, and (2) change of quality or complete disappearance of some of the photons by transfer of their energy to the material to produce fast corpuscular rays. As the radiation travels through the material, the number of photons that have not been deflected or otherwise altered continues to decrease. This decrease is due to absorption.

The energy lost from the original beam is not entirely consumed at the place where the absorption occurs, but is transmitted elsewhere by the deflected x-rays and by the fast corpuscular rays that are produced. *Hence absorption does not refer to the amount of radiant energy that is made available for producing biological effects* at the place where the absorption occurs.

Scattering refers to the change in direction of propagation of the radiation, caused by the deflection of the rays from their original direction as mentioned above. *Scattered radiation* refers to the rays received at the place of measurement, that have been so deflected in other parts of the material and in other objects exposed to the radiation.

Primary radiation is the radiation that is received directly from the focal spot of the x-ray tube. The inverse square law applies accurately to the primary radiation, except within a short distance of the target, or when there is no absorption along the path of the radiation.

Secondary characteristic x-radiation is x-radiation which *originates* in any material that is exposed to x-rays. It is difficult to distinguish in measurement from scattered radiation and is usually of relatively low intensity. In common practice the distinction is ignored, and the term "secondary radiation"[4] is used to include both scattered and secondary characteristic radiation.

Total radiation includes all x-rays arriving at the place where it is measured. The *total exposure* is the sum of the primary radiation and the secondary (scattered plus secondary characteristic) radiation exposures.

The free air exposure is the exposure of radiation received from the x-ray apparatus in a region in air where the exposure of scattered radiation from objects external to the x-ray apparatus (*e.g.*, the patient and the walls of the room) is negligible. It is due to primary radiation and radiation

[3] The energy of a beam of x-rays may be considered to be divided into a very large number of parts called photons. Usually one photon only is involved in the interaction of the radiation with one atom of the material.

[4] Reference to the secondary radiation as "scattered radiation" is a common misuse of terms. For example "back-scatter" consists of both scattered and secondary characteristic radiation.

scattered from internal parts of the x-ray apparatus and treatment cone, *e.g.*, filters, cooling oil, etc.

The *tissue exposure* is the total exposure at a place in tissue. It is useful sometimes to consider the tissue exposure as consisting of two parts: primary and secondary. *Primary tissue exposure* is the part of the tissue exposure due to primary radiation and radiation scattered from internal parts of the x-ray apparatus. *Secondary tissue exposure* is the remaining part of the tissue exposure, and is due to scattered and secondary characteristic radiation from the tissue and other objects external to the x-ray apparatus. It should be noticed that the words "primary" and "secondary" are not entirely appropriate in these terms and may be misleading.

The *skin exposure* is the tissue exposure at a place very close to the skin surface. It is possible to determine the skin exposure approximately by measuring the exposure in air very close to the skin surface, because the exposure does not alter abruptly in passing through the surface from air to tissue but changes gradually. Thus the exposure at a point 0.1 mm. above the surface does not differ very greatly from the exposure at a point 0.1 mm. below the surface, even though the two points are in different materials. (This follows from the fact, stated earlier, that the exposure describes the radiation but not what happens to it.)

In describing a treatment, either the free air exposure or the tissue exposure may be specified (often both are desirable). The former applies to the radiation that would have been present at the point considered if the patient and other scattering and absorbing objects were removed from the x-ray beam; the latter applies under the actual conditions of the treatment with the point occupied by tissue. The two values of the exposure usually differ greatly, and it is essential to indicate clearly which is intended. Such statements as "the patient received 1,000 r" are meaningless. Ambiguity should be avoided by the use of such expressions as: "free air exposure of 1,000 r at the skin surface," "skin exposure of 1,000 r," "free air exposure of 1,000 r at 5 cm. below the skin surface," "tissue exposure of 1,000 r at 5 cm. below the skin surface," or by other precise description. *The word "exposure" should never be used without preceding it by the words "free air," "skin," or "tissue"* unless it is clear from context or explanation which is meant. *The exposure should never be specified without indicating where the exposure applies* (*e.g.*, distance below skin surface, distance from cone, or otherwise). *A treatment should never be described by stating the number of roentgens without further explanation.*

X-rays are a form of energy propagated through space. It would be useful to measure the quantity of radiant energy passing through unit area at any particular place during a treatment, but this is not easily done. Measurement of the exposure in roentgens has, therefore, been adopted as a practical substitute for the measurement of the flow of energy, and our methods of dosage specification have been based entirely on measurement of exposure instead of energy. The energy of the radiation, flowing through a region or absorbed in it, is not measured and for practical purposes need not be considered. In fact, energy is mentioned here only to emphasize that *the exposure is not a measure of the energy of the radiation.* Exposure is related to a different property of the radiation, *viz.*, its ability to ionize air under certain specified conditions. It is not possible to deduce the flow of energy or the absorption of energy from knowledge of the exposure only. The flow of energy in an exposure of one roentgen of very hard x-rays is much greater than in the same exposure of very soft x-rays. The absorption of energy with an exposure of one roentgen is much greater in a dense material like tissue than in a material like air. Moreover it is not possible to deduce the attenuation of the intensity of the radiation in penetrating a material from the exposure alone. Hard radiation is more penetrating in tissue than soft radiation regardless of the skin exposure.

When rays penetrate matter, *secondary*

corpuscular rays[5] are produced, which are electrons expelled from atoms of the matter with very high velocities by energy received from the x-rays. These secondary corpuscular rays produce nearly all of the ionization that occurs when x-rays pass through matter. The number and energy of the secondary corpuscular rays that are produced depend on the material traversed, as well as on the quality of the primary beam. This fact has two consequences of particular importance in practical radiologic measurement.

First, the walls of the thimble ionization chamber that is used to measure the exposure of x-rays must be thick enough to prevent any secondary corpuscular rays that are produced in material outside the chamber from passing through the walls; otherwise, the number of ions produced in the chamber would be affected by these secondary corpuscular rays from the outside material and the instrument would not record the exposure correctly. (The use of very thin wall chambers should not be attempted without clear understanding of the physical principles involved and of the properties of the rays.)

Second, to permit a consistent relation between the skin exposure and the reaction it produces, *the material directly over the skin must be suitably chosen*, particularly when using very hard x-rays. The skin tissue receives secondary corpuscular rays from the material next to it and hence the reaction depends partly on the nature of the material. To obtain consistent results, the material should not differ greatly in x-ray absorption properties from the tissue. Commercial bakelite and other impregnated composition materials are suitable and convenient for this purpose. If the aperture of the treatment cone is not provided with a window or cover of this material, a sheet of it should be placed over the skin. (Most treatment cones recently manufactured are so provided.) It is particularly important to protect the skin from secondary corpuscular rays from lead glass or any metal that is exposed to the x-rays. The thickness of bakelite, either in the cone window or otherwise covering the skin, required for this protection, is at least 0.3 mm. for every 100 kv. peak potential applied to the x-ray tube. However, an air space of at least 5 cm. between the skin and the lead parts of the diaphragm system, as occurs in the use of open ports without cones, provides the necessary protection if the tube potential is less than 250 kv. and the width of the treatment field at least 5 cm. (See also Chapter V.)

[5] The secondary corpuscular rays are sometimes called "secondary beta rays." The latter term is inappropriate because beta rays are of radio-active origin. (The term "corpuscular rays" is applicable to any small particles projected with high velocity, *e.g.*, alpha particles, neutrons, etc., but in x-ray therapy we are concerned only with electrons.)

CHAPTER II.—INSTRUMENTS AND APPARATUS

Portable Roentgen Meters (Dosemeters) with Thimble Ionization Chambers.—Portable dosemeters or roentgen meters with small ionization chambers, known as "thimble chambers," are calibrated to read the exposure in free air that would occur at the point occupied by the center of the chamber if the instrument were removed. They record the total exposure of radiation received from all directions. They are calibrated with the axis of the chamber placed perpendicular to the axis of the x-ray beam, and they may be less accurate if used with the chamber axis in other directions. Hence the axis of the chamber should be placed at right-angles to the direction from which the greatest intensity of x-rays is received. Small thimble chambers are preferable to larger ones for measurements in a phantom because the displacement of the phantom material by their presence is a source of error.

Portable roentgen meters should be recalibrated at regular intervals unless they are checked by exposure to the radiation from a radium needle or checked by comparison with other roentgen meters. The use of a radium source consisting of a few milligrams of radium element is the only

method of checking the calibration of such a roentgen meter without recourse to comparison with another roentgen meter or a standard ionization chamber.

It is recommended that a radium check machinist, to fit the chamber support snugly, and marks should be engraved on the needle and chamber to assist in accurate replacement.

Many roentgen meters are provided with

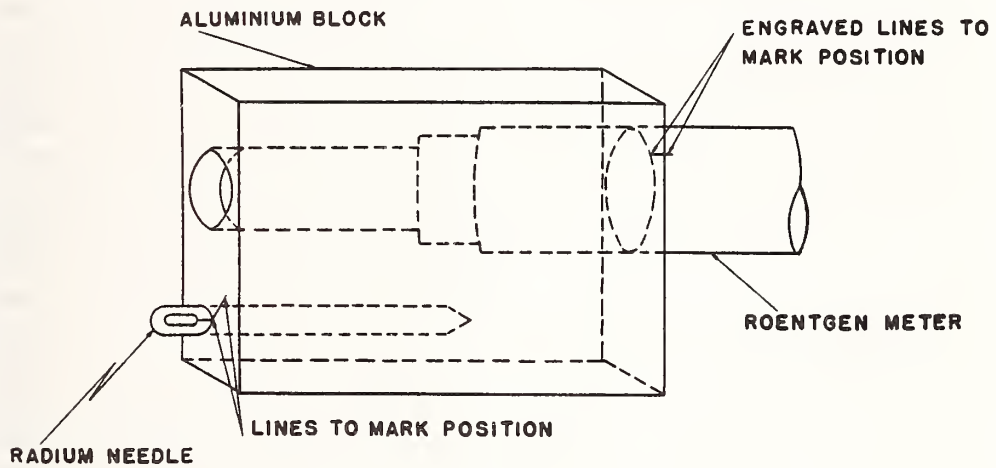

Fig. 1.

be provided for the thimble chamber roentgen meter. This may consist of a small radium needle or tube (5–10 mg.) and a metal holder to mount the radium container at a fixed distance (about 1 or 2 cm.) from the thimble chamber (see Fig. 1). When the instrument is received from the manufacturer, or has just been calibrated, the intensity of the radiation from the needle should be measured by the instrument in terms of the number of divisions of the scale traversed per minute. The instrument may be checked on later occasions by repeating this measurement. In making tests of this kind, it is necessary that the needle be replaced in the same position as accurately as possible, *e.g.*, the distance of the needle from the chamber should not vary by more than the thickness of a sheet of writing paper; the position of its ends should be set with similar accuracy; the needle should "point" in the same direction relative to the chamber, and the same sides of the needle and the chamber should be opposite each other. Hence, the metal holder should be made by a skilled

a device for testing the recording part of the instrument, *e.g.*, a switch (usually marked "test") for discharging a part of the electrical circuit, or an auxiliary ionization chamber containing a small quantity of radio-active material. These devices make it possible to verify that the electrometer or the recording part of the instrument is operating properly, but they are not a means of testing the ionization chamber. Hence, they do not provide an infallible test that the instrument continues to read correctly since last calibrated.

Use of Thimble Chambers with Extra Hard X-rays.—Satisfactory standard ionization chambers for the calibration of roentgen meters for use with extra hard x-rays were not developed until very recently, and the manufacturers have not been able to calibrate such instruments with radiation excited at potentials above 200 kv. A roentgen meter cannot be expected to read correctly with radiation that is not within the range of qualities for which it has been calibrated. (A few instruments that have been calibrated by the

manufacturers with potentials up to 200 kv. applied to the x-ray tube, have been found later to read correctly with potentials up to 400 kv., but it must not be inferred that this would be true of *any* instrument, even if made by the same manufacturer.)

A roentgen meter calibrated with 200 kv. radiation may be used for relative measurements in comparing treatments with higher kilovoltages, using the same installation under conditions that do not differ greatly, if the following precautions are observed:

1. The instrument reading is interpreted in relative arbitrary units—not in roentgens.
2. The radiation used in the different treatments being compared does not differ greatly in quality, *e.g.*, the kilovoltage is not altered by more than 20 per cent.
3. The thickness of the wall of the ionization chamber is increased by covering with a sleeve of organic material of suitable thickness. A sleeve of unit density organic material should have a thickness of 0.3 mm. for every 100 kv. in excess of 350 kv. This precaution is unnecessary if the thimble chamber is surrounded on all sides by biological tissue, phantom material, bakelite, or similar organic products, for a distance equal to at least 0.4 mm. for every 100 kv. in excess of 350 kv.
4. The electrometer part of the roentgen meter is kept out of the x-ray beam and any appreciable scattered radiation.
5. Air spaces in the instrument, likely to give rise to spurious ionization currents, are kept out of the beam or adequately shielded.

Ionization Chambers Mounted in the X-ray Tube Housing or in the Detachable Treatment Cone (Iometers, etc.).—These instruments register the intensity of the radiation emitted by the x-ray tube in arbitrary units, at an undefined position in the tube housing. They enable the radiologist to ascertain that the radiation output of the apparatus is the same during treatment as during the preliminary or subsequent intensity measurements. They are also a valuable safeguard against unforeseen changes in the performance of the x-ray apparatus that might otherwise not be observed.

The proper use of these instruments is described in Chapter VI.

Recording or Integrating Roentgen Meters.—These instruments record the total exposure (or the total dose) delivered during a treatment. The recording part of the instrument is mounted on the technician's desk, the wall, or the control panel, and is connected through a flexible cable to the proper electrode of a vacuum tube, to which is also connected a thimble ionization chamber, which remains in contact with the patient's skin during the treatment. The chamber is either attached to the window of the treatment cone, or is supported on an adjustable bracket. The former type is neater and not likely to be moved from the field accidentally. The latter type is generally more useful because it can be moved about to explore the radiation field if desired, or used in a phantom, and it is more readily calibrated. Both types may be used for the measurement of free air exposure or skin exposure by the methods described in later chapters for portable roentgen meters.

It is recommended that recording roentgen meters be calibrated more frequently than the simpler types of instrument meter, because their complicated mechanism is more likely to get out of order.

The skin directly under the supporting stem of an ionization chamber that is left in place during the treatment receives a slightly smaller exposure than the rest of the field. This partial shadow on a small part of the field is obscured by scattered radiation and is unimportant below a depth of a few centimeters.

Thin Wall Ionization Chambers.—Ionization chambers having walls so thin that they are easily penetrated by nearly all of the secondary corpuscular rays reaching them from material outside the chamber are useful for investigating these secondary corpuscular rays. Measurements made with these thin wall chambers are suitably

expressed in "electrostatic units per cubic centimeter," abbreviated "e.s.u./c.c."

Under certain conditions only, the exposure of x-rays may be deduced from measurements made with thin wall chambers. Under these conditions very accurate measurements, suitable for the compilation of tables of data, may be made. Such use should be made only with a knowledge of the properties of the secondary corpuscular rays, and is not recommended for routine clinical work.

Standard Ionization Chambers.—Standard ionization chambers are unsuitable for the measurement of dose in the ordinary hospital or clinic. The accuracy implied by the term "standard" is attained only when they are used in the measurement of almost parallel primary radiation emitted from a small source. This requirement can seldom be met with hospital equipment. The principal function of a standard ionization chamber is the calibration of thimble chambers.

Meters on the Control Panel of the X-ray Apparatus.—Technicians should be trained to watch the various instruments and meters on the control panel. They should be particularly alert to discover and correct any unusual change in the kilovoltage or milliamperage during treatment, and to reproduce the readings of these instruments that were recorded when the dosage measurements were made, or the radiation output of the apparatus was measured. This precaution is particularly important if the installation is not equipped with a monitoring device, such as an ionization chamber in the tube housing. Careful attention to the meters should not be relaxed because the apparatus has operated satisfactorily in the past—defects and failures in x-ray apparatus usually occur unexpectedly.

Treatment Cones or Applicators.—Consistent relation between the skin dose and the skin reaction cannot be expected unless the material directly over the skin and in contact with it is always the same. This purpose is fulfilled by the cover made of bakelite or similar composition material that closes the end of the treatment cone placed against the skin. If the cover is removed, a greater or lesser reaction may result, depending upon the nature of the materials inside the cone that are exposed to the x-rays, owing to the secondary corpuscular rays that the skin receives. Tables of data such as are published in this bulletin apply more accurately if the cones are provided with baffles to define the width of the x-ray beam before it reaches the cover.

Open Ports without Cones or Applicators.—To insure consistent relation between dose and reaction referred to in the preceding paragraph, when open ports are used instead of cones, the patient's skin should be separated from the edge of the port, and other parts of the x-ray apparatus exposed to the x-rays, by a distance greater than the range of most of the secondary corpuscular rays. This distance should exceed 5 cm. for the radiation produced by 200 kv. or less. This limit should be increased by 5 cm. for each additional 100 kv. applied to the x-ray tube.

Care is necessary in using open ports to insure that the exposed part of the patient's body is moved as little as possible during the treatment. A change of a few centimeters in the distance of the tissue from the target of the x-ray tube results in considerable change in the exposure received. A lateral shift of less than a centimeter may, if the field is small, result in only a portion of the correct region being irradiated.

CHAPTER III.—ACCURACY AND ERROR IN MEASUREMENT OF EXPOSURE

The busy radiologist has neither the time nor the facilities to measure exposure and dosage rate as accurately as the physicist in a standardization laboratory. He requires an accuracy that will assure him of reliable and consistent treatment. He will attempt to make these measurements as accurately as his equipment will permit without unduly increasing the labor and time required.

It is often difficult in the clinic to make measurements that can be relied on to be in error by less than 5 per cent. On the other hand, an error greater than 15 per cent can usually be avoided in the measurement of x-ray dosage for most treatments if moderate care is taken, and the directions given in this bulletin are followed.

Many causes may contribute to the error, *e.g.*, unsteady output of the x-ray tube, unavoidable differences between the conditions under which the measurement is made and the conditions under which the result of the measurement is to be used, inaccurate timing, carelessness in arranging the equipment, etc. If the total error is to be less than 15 per cent, the possible error due to each one of these causes should be considerably less than 15 per cent.

The fact must be accepted that accurate measurements require care. The attention to details stressed in the following pages may at first appear to be discouraging. To follow the recommendations faithfully will undoubtedly require more time and attention on the first occasion than the radiologist would be willing to devote to measurements in usual practice. However, it will be found that these methods will become familiar very quickly and will require very little more trouble than less careful measurements. The additional effort will be more than justified by the increase in confidence in the results obtained.

Invariably, measurements should be repeated, because inaccuracy is often revealed by disagreement in the measurements. However, close agreement of a number of measurements does not prove that they are accurate. *A large error may exist due to a cause that would not show itself by disagreement between readings*, e.g., *faulty arrangement of the apparatus, or an inaccuracy in the roentgen meter itself.*

The following directions are intended to supplement the instructions supplied by manufacturers of roentgen meters. Directions for making corrections for temperature, pressure, and altitude are not included since they are usually given in the manufacturers' instructions.

CHAPTER IV.—MEASUREMENT OF THE FREE AIR EXPOSURE

1. *The roentgen meter should be suitable for the quality of the x-rays measured.* Most roentgen meters are fairly accurate for the measurement of radiation having a copper half value layer between 0.2 mm. and 4 mm. The quality at which the true calibration deviates seriously from the scale reading varies with different meters. The radiologist should verify that the instrument is suitably calibrated before using it under the following conditions:

(*a*) Voltages above 200 kv. (constant potential),
(*b*) Half value layer less than 0.2 mm. copper or 1.5 mm. aluminum,
(*c*) Voltages below 100 kv. (peak or constant potential) with a thick wall x-ray tube and no metal filtration,
(*d*) Voltages below 120 kv. (peak or constant potential) with a thin wall x-ray tube and no metal filtration,
(*e*) Voltages below 100 kv. (peak or constant potential) with a thin wall tube and 2 mm. aluminum filtration.

2. *Scattered radiation should be avoided.* This requirement will be adequately fulfilled if there are no objects except the roentgen meter chamber in the x-ray beam between the end of the treatment cone or the port and a distance beyond the chamber equal to at least four times the width of the x-ray beam at the position of the chamber.

3. *The roentgen meter should be carefully placed in position.* The axis of the ionization chamber should be at right-angles to the direction of the x-ray beam. The instrument is intended to measure the exposure (that would obtain in the absence of the instrument) at the point occupied by the center of the thimble ionization chamber. Unless specifically stated otherwise, the exposure is measured at points on the central axis of the x-ray beam.

4. *A correction should be applied when the position of the chamber is displaced from the point where it is desired to measure the exposure rate unless this displacement is less*

than 1/50 of the distance from the target. It is often desired to measure the exposure rate at the center of the surface of the composition cover over the aperture of the cone. Since it is impossible to place the roentgen meter with the center of the chamber at the surface of the cover, the following extrapolation method may be used:

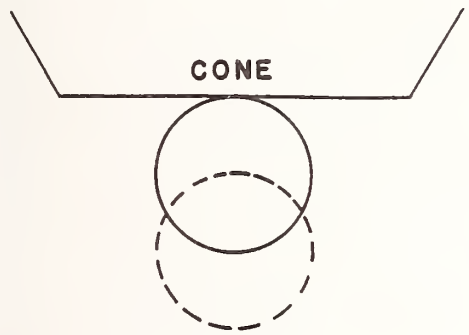

Fig. 2.

Measure the exposure rate with the chamber just below the cover (as the solid circle of Figure 2). Lower the chamber so that the distance from the surface of the cover to the center of the chamber is doubled (dotted circle in figure) and measure the exposure rate in this position. Subtract the second measurement from the first. The result is the correction for the distance from the cover to the center of the chamber. Add this correction to the first reading. (Make sure that the two positions of the chamber lie on the central axis of the x-ray beam.)

Example: First measurement, chamber touching cover—25 r/min.
Second measurement, chamber lowered—21 r/min.
Hence correction for displacement from surface of cone = 25 r/min. − 21 r/min. = 4 r/min.
And exposure rate at surface of cover = 25 r/min. + 4 r/min. = 29 r/min.
The correction may be neglected if the diameter of the chamber is less than 1/25 of the distance from the target and less than 1/10 of the diameter of the field, since the error will probably be less than 5 per cent.

5. *An accurate watch should be used for time measurements.* Do not use a "hand timer" or other time switch for timing exposures when measuring exposure rates. Hand timers are frequently quite inaccurate. Pocket watches are usually more dependable than stop watches and sufficient accuracy is attained if the second hand is read to the nearest second in timing an interval of more than one minute, or within two seconds in an interval of two minutes, etc. Moreover, all measurements intended for future reference should be made after the tube has been in operation for a short time because the output usually changes considerably during the first few minutes of operation. A suitably built-in shutter is the most satisfactory means of controlling the interval during which the roentgen meter is exposed to the x-ray.

CHAPTER V.—MEASUREMENT OF THE SKIN EXPOSURE

1. *The roentgen meter should be suitable for the quality of x-rays used.* The rays scattered from underlying tissue are softer than the rays received directly from the x-ray tube. Hence the radiation received by the skin, consisting of both primary and scattered radiation, is softer than radiation from the tube in free air. The same caution to verify that the instrument is suitably calibrated as mentioned in the first paragraph of Chapter IV should be observed, and the lower voltage limits therein stated should be raised by 10 kv.

2. *In later treatments the exposed skin should be covered with a sheet of fiber, bakelite, celluloid, wood, or other organic product not containing heavy elements, 1 to 3 mm. thick,* unless the end of the treatment applicator is permanently closed by a cover of such material. This cover acts as a filter to stop secondary corpuscular rays from the inside of the treatment cone from reaching

the skin. Heavy elements, *e.g.*, lead, lining the treatment cone, may be a source of *additional* corpuscular rays which are not recorded by a properly designed roentgen meter. Since these rays would contribute to the skin reaction they must be excluded to insure a simple and consistent relation between the x-ray exposure and the skin reaction. If an open port without a treatment cone is used, the covering over the skin may be omitted if (1) the potential applied to the tube is less than 400 kv., (2) the width of the exposed area of the skin is greater than 5 cm., and (3) the distance of the skin from metal parts or lead glass parts of the apparatus is at least 5 cm. for 200 kv. or less potential applied to the tube, 10 cm. for 300 kv., and 15 cm. for 400 kv.

3. *Error due to a difference in the position of the chamber of the roentgen meter during measurement and the skin during treatment should be avoided or corrected.* If the chamber of the roentgen meter is placed between the skin and the cover of the treatment cone, it will measure the exposure at a position above the skin a distance equal to half the diameter of the chamber. The error due to this cause is likely to exceed 10 per cent if the width of the field is less than five times the diameter of the chamber. It is usually negligible if the diameter of the chamber is less than 1/50 of the distance from the target and less than 1/15 of the width of the field. The error may be reduced by the following extrapolation method:

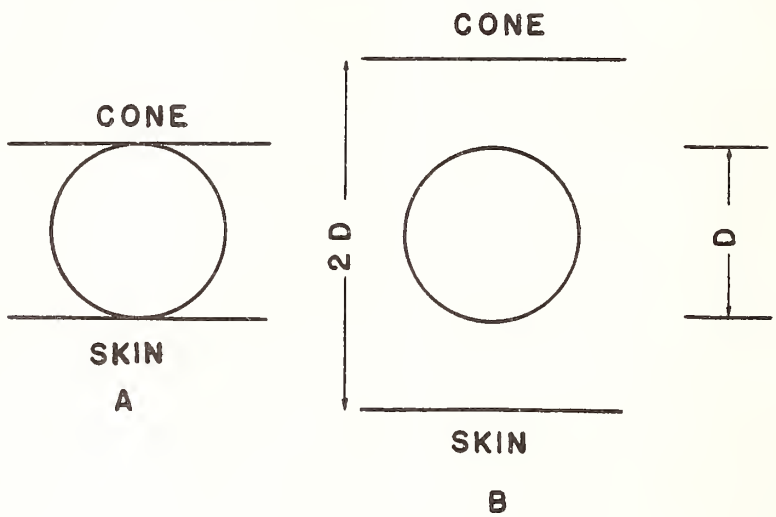

Fig. 3.

Measure the exposure rate with the chamber just touching the skin below, and the applicator cover above (*A*, Fig. 3). Make a second measurement with the center of the chamber separated from the cover above and the skin below by distances equal to the diameter of the chamber (*B*, Fig. 3). (Note that the distances from the surface of the cover to the center of the chamber, and from the surface of the skin to the center of the chamber, are just twice as great as in the first position.) Subtract the second measurement from the first; this will give the approximate correction for the space between the skin and the cone cover. Add this correction to the first reading.[6]

Example: First measurement, position *A*, 20 r/min.
Second measurement, position *B*, 17 r/min.
Hence the correction for the

[6] Care should be taken not to depress the surface of the skin, *e.g.*, by the weight of the chamber.

space is 20 r/min. − 17 r/min. = 3 r/min.
And the approximate exposure rate is 20 r/min. + 3 r/min. = 23 r/min.

A "separator" is useful for adjusting the distances for the second measurement quickly and accurately. This might be a cylindrical ring of bakelite, cardboard, or similar material having a length of twice the diameter of the thimble chamber, and a diameter nearly as large as the applicator cover. A hole in the side wall of the ring permits insertion of the chamber at the correct height. This ring should have a thin wall, so that it scatters little radiation.

If an open port without treatment cone is used, the error due to the position of the chamber may be reduced by the following extrapolation method:

Measure the exposure rate with the chamber just touching the skin (solid circle in Fig. 4). Make a second measurement with the *center* of the chamber separated from the skin by a distance equal to the diameter of the chamber (dotted circle).

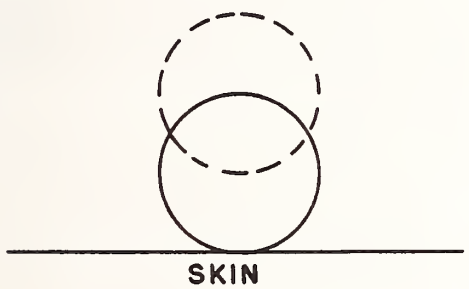

Fig. 4.

(Note that the chamber is raised a distance of half its diameter for the second measurement, and that the focus–skin distance has not been altered.) Subtract the second reading from the first; this will give the approximate correction for the size of the chamber. Add this correction to the first reading.

Example: First measurement, chamber touching; 20 r/min.
Second measurement, chamber raised 18 r/min.
Hence the correction for the size of the chamber is 20 r/min. − 18 r/min. = 2 r/min.
And the approximate skin exposure rate is 20 r/min. + 2 r/min. = 22 r/min.

4. *An accurate watch should be used for time measurements.* "Hand timers" should not be used. (See Chap. IV, par. 5.)

The Exit Skin Exposure.—The exit skin exposure is important in cross-fire treatment methods when the same region of the skin also receives direct exposure. It is always smaller than the direct exposure, and, therefore, need not be determined quite as accurately. For example, if the exit exposure is one-third as great as the direct exposure, three times as large a percentage error may be tolerated in the determination of the exit exposure.

The exit skin exposure may be measured by the methods described above for the skin exposure in direct irradiation. The instrument may be placed against the skin of the exit field. If, for this purpose, it is necessary to place the instrument between the patient's body and the treatment table top, the chamber may be protected from pressure likely to break it by a piece of wood on each side of it. Since less accuracy is required in exit exposure measurements, usually corrections for displacements, as described in Precaution 3 above, are unnecessary.

The exit irradiation depends on the thickness of material close to the skin which is encountered by the radiation after emerging through the exit field, owing to radiation scattered by this material. Care should be taken that the thickness of this material remain unaltered during the measurement and treatment.

CHAPTER VI.—USE OF "IOMETERS," AND OTHER INSTRUMENTS HAVING IONIZATION CHAMBERS PERMANENTLY INSTALLED IN THE X-RAY APPARATUS, FOR MONITOR PURPOSES

An instrument having an ionization chamber built in the x-ray tube housing or

the treatment cone is a very useful accessory. It informs the radiologist of unpredictable changes in the performance of the x-ray apparatus, which might result in failure to apply the exposure intended.

One type of these instruments has a thimble chamber fixed in the treatment cone, to the window that is pressed against the patient's skin. It is intended to record the skin exposure. Its use is, however, subject to limitations imposed by its immobility, *e.g.*, the difficulty of making correction for the distance between the skin and the center of the chamber as discussed in other chapters.

Other types of these instruments have thimble chambers installed in the tube head above the treatment cone (*e.g.*, the instrument known commercially as the "Iometer"), drum type ionization chambers in the direct beam above the treatment cone, or small ionization chambers mounted outside the direct beam to the patient. These types are monitoring devices only. They do not measure the free air exposure or the skin exposure directly. *Hence they record in arbitrary units—not in roentgens or in roentgens per minute.* Properly used, they help to prevent mistakes in kilovoltage, milliamperage, and filters. They do not, however, give protection against errors in distance.

These monitoring devices are very convenient when determining the skin exposure or the tissue exposure by the use of tables. Thus, the instrument may be "calibrated" in terms of the free air exposure *for the particular choice of conditions to be used* by determining the reading of the instrument which corresponds to a free air exposure rate of 100 r /min. (or, in the case of the recording types, that corresponds to 100 r). The reading of the instrument during treatment, then gives the free air exposure rate, or exposure, and the skin exposure can be calculated by use of the tables.

Caution: for this purpose the reading of the monitoring instrument should not be affected appreciably by the presence of the patient under the treatment cone. If its ionization chamber receives radiation scattered from the exposed tissue, a corresponding error will result. This should be tested by observing any sudden change in the reading of the instrument when the patient is removed. If a change of more than 5 per cent is observed, the instrument is not suitable for this purpose. Monitors with drum or plate type chambers are more affected by radiation scattered from the patient than the other types.

In calibrating a monitor instrument in terms of free air exposure rate, the free air exposure should be measured by the methods described in a previous chapter, using a portable dosemeter. Measurements with both instruments should be made concurrently, and, for accuracy, several readings should be taken.

The calibration applies only to the particular set of conditions under which it was made. For these conditions the reading of the instrument is proportional to the free air exposure rate at the place for which the calibration is made (*e.g.*, end of the cone). Obviously the free air exposure rate at this place will not retain the same ratio to the reading obtained with an ionization chamber located in another position if the conditions are changed. The calibration is unsatisfactory if the conditions of calibration are departed from by—

(1) Changing the treatment cone.
(2) Changing the metal of the primary filter. (The primary filter is the denser metal when more than one metal is used. The so-called "copper equivalent" of the glass wall of the x-ray tube should not be considered as a part of the filter in this connection, and in condition 3 below.)
(3) Doubling or halving (at least) the primary filter thickness.
(4) Increasing or decreasing the kilovoltage by 20 per cent or more.

Changes within these limits are permissible, without recalibration, if the walls and electrodes of the monitor are of organic material or aluminum. If of denser metals, the filters may not be changed and the kilovoltage must be kept within the limits

of the ordinary power supply fluctuations (*i.e.*, the setting of auto-transformer and rheostat or other kilovoltage control may not be changed).

CHAPTER VII.—USE OF TABLES TO DETERMINE SKIN EXPOSURE FROM MEASURED FREE AIR EXPOSURE

Table I gives the skin exposure corresponding to 100 roentgens as measured in free air, for several qualities of radiation, for a range of field sizes and body thicknesses, and for various thicknesses of cone cover. The data have been selected because they are applicable to free air exposures measured under the conditions described in Chapter IV. Many similar tables may be found in the literature but they should not be used with free air exposures measured under the conditions and methods of Chapter IV unless it is known that they are applicable.

The increase in the skin exposure as compared to the exposure measured in air depends on four factors: the area of the surface exposed, the quality of the x-rays, the composition and thickness of the cover of the treatment cone, and the depth of tissue and supporting material behind the exposed skin surface. (It is assumed that the support is wood or similar material and no metal is present.) The following precautions should be observed in using the table:

(1) *The free air exposure should be measured by the method described in Chapter IV at the center of the composition cover of the treatment cone, or (in the case of open port treatment without cone) at the same distance in front of the port as the skin is during treatment.*

(2) *If an iometer or other permanently installed ionization chamber is used to indicate the free air exposure, it should be calibrated by the method described in Chapter VI,* in terms of the free air exposure at the position occupied by the skin during treatment, *i.e.*, at the center of the cover of the treatment cone or (in the case of open port treatment) at the center of the beam at the appropriate distance from the port.

(3) *If an iometer or other permanently installed ionization chamber is used to indicate the free air exposure, a test should be made to determine whether or not this instrument receives an appreciable exposure of radiation scattered back from the tissue exposed.* This should be done by removing the patient, or phantom, and other scattering objects and observing whether or not the reading of the instrument differs from the reading obtained before they were removed. If the ionization chamber of the instrument is in the x-ray tube housing above the removable treatment cone (as in the case of the iometer), the difference in the two readings may be negligible unless the cone is short and wide. If the chamber is near the end of the treatment cone, the difference between the two readings may be large, and a correction should be made for it.

If a difference greater than 3 per cent is found, the dosage rate (or the exposure) indicated by the instrument should be corrected accordingly.

First example: Reading of iometer with patient exposed, 55 divisions. Reading of iometer with patient removed from x-ray beam, 50 divisions. Previous calibration of the iometer for the particular cone and other conditions used (see Chap. VI) showed that one division of the iometer scale corresponded to 0.400 r/min., hence the free air dosage rate to be used in Table I is 50 × 0.400 r/min. = 20.0 r/min. The reading of 55 on the iometer is not proportional to the skin exposure, since only a portion of the backscatter is effective in such a chamber. It is simply a monitor reading which must be kept

TABLE I.—SKIN DOSE IN ROENTGENS CORRESPONDING TO 100 ROENTGENS IN AIR AT SAME DISTANCE FROM FOCUS

Thickness of Underlying Tissue	Open Port				Cover on Treatment Cone 1/8 in. Bakelite				Cover on Treatment Cone 1/4 in. Bakelite			
	Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)			
	5	25	100	400	5	25	100	400	5	25	100	400
	120 kv. peak No filter H.V.L. 1.0 mm. Al or 0.035 mm. Cu											
1	104	108	111	113	104	108	111	114	104	109	112	115
2	107	112	116	119	107	113	117	119	107	113	118	121
4	110	116	122	125	110	117	123	127	111	117	124	128
6 (Max.)	111	117	124	128	111	118	126	130	112	118	127	132
	150 kv. peak 5 mm. Al filter H.V.L. 0.3 mm. Cu											
1	104	108	112	114	104	108	113	115	104	109	113	116
2	107	112	118	124	107	113	119	126	107	113	120	127
4	110	118	126	136	110	119	127	139	111	119	129	142
7	114	123	134	146	114	124	136	150	115	125	139	154
12 (Max.)	115	125	136	150	115	126	138	154	116	127	140	159
	200 kv. peak 0.5 mm. Cu filter H.V.L. 0.9 mm. Cu											
1	104	107	111	114	104	107	112	115	104	107	112	116
2	106	112	117	124	106	113	118	126	107	113	119	127
4	110	117	126	134	110	118	128	137	111	118	129	140
7	112	122	133	144	112	123	135	148	113	124	138	152
10	113	123	134	146	113	124	136	150	114	125	139	154
15 (Max.)	114	124	136	149	114	125	137	153	115	126	140	158
	200 kv. peak 2.0 mm. Cu filter H.V.L. 1.8 mm. Cu											
1	102	105	108	110	102	105	109	111	102	105	109	111
2	104	108	112	117	104	108	113	118	104	109	113	119
4	106	112	118	125	106	112	119	126	106	113	120	127
7	108	115	124	132	108	115	125	134	109	116	126	136
10	108	116	125	134	108	116	126	136	109	117	127	138
15 (Max.)	109	117	126	136	109	117	127	139	110	118	128	140

constant for the set of treatment conditions employed in the particular case under discussion.

Second example: Reading of recording dosemeter with patient exposed, 15 divisions in 60 seconds; reading with patient removed, 15 divisions in 76 seconds. Previous calibration—one division of instrument scale corresponds to 0.82 roentgen. Exposure recorded during the entire treatment—300 divisions. Hence, the total free air exposure to be used with Table I is $300 \times 60/76 \times 0.82 = 194$ r.

It should be noted that a correction determined in this way is applicable only under conditions similar to those under which it is determined, and not with other treatment cones, qualities of x-ray, etc.

The Exit Skin Exposure.—Some difficulty in determining the exit skin exposure by the use of tables results from the fact that it depends on the thickness of other material, such as the top of the treatment table, next the skin, which contributes to

the exposure by back-scatter. This difficulty may be avoided simply by increasing the thickness of the layer of material next the skin by blocks of hard wood or bags of rice and flour to a total of at least 7 cm. When this is done, the skin of the exit field may be regarded as a region in the bulk of the tissue and the exposure may be determined by the method and tables of Chapter IX.

If it is inconvenient or undesirable to place material next the skin of the exit field, approximate values of exit exposures may be obtained from Table IV. Actual exit exposures are given for 5, 10, and 15 cm. thicknesses of part, for several fields, and two qualities of radiation, for cones covered with 1/8 in. bakelite. For other sets of factors, the ratio of the exit exposure to the full thickness exposure can be estimated.

Example: Estimate the exit exposure for a 100 sq. cm. field in a part of the body 7 cm. thick, for 200 kv. x-rays, with a filter of 0.5 mm. Cu and a distance of 50 cm.

The table does not give data for 7 cm. thickness, but this can be estimated from that for 5 and 10 cm. For a 100 sq. cm. field and 5 cm. thickness the exit exposure is 44 per cent; for a full phantom the depth dose at 5 cm. with these factors (see Table II) is 57 per cent. Hence the exit exposure at this level is 44/57 of the dose at the same depth in a complete phantom, or 77 per cent of it. Similarly, for a 10 cm. thickness, the exit exposure is 22/29, or 76 per cent of the dose at the same depth in a complete phantom. Accordingly the exit exposure for a 7 cm. thickness must also be approximately 76 per cent of the dose at the same depth in a complete phantom, which (see above) is 44 per cent. Hence, 76 per cent of 44 or 33 per cent, is the exit exposure desired.

CHAPTER VIII.—MEASUREMENT OF THE TISSUE EXPOSURE

1. *The roentgen meter should be suitable for the quality of the x-rays measured.* The same precaution should be observed as for the measurement of skin exposure, as discussed in Chap. V, par. 1.

2. *The material used for a phantom should have a specific gravity of 0.95 to 1.0 and contain no chemical element of atomic number greater than 8.* The atomic numbers of the light elements are as follows: H—1, He—2, Li—3, Be—4, B—5, C—6, N—7, O—8. It should contain not more than three atoms of hydrogen for one atom of oxygen in its composition. (Theoretically, the ideal material is one in which

$$(n_1N_1^4 + n_2N_2^4 + \text{etc.})/(n_1N_1 + n_2N_2 + \text{etc.}) = \text{approximately } 400$$

wherein n_1, n_2, n_3, etc., are the relative number of atoms of the elements of atomic numbers, N_1, N_2, N_3, etc., respectively.) This requirement is satisfied by water, and by rice mixed with sufficient flour to fill the spaces between the rice kernels. Hydrocarbons, such as paraffin wax, are not suitable. Wood products that have a specific gravity between 0.95 and 1.00 are satisfactory (*e.g.*, masonite pressdwood).

3. *A roentgen meter with the smallest ionization chamber available should be used.* The air space of the ionization chamber causes less absorption than the phantom material it displaces. Therefore, more primary radiation reaches the center of the chamber and the instrument reads too high. The error due to this cause, when using a chamber 2 cm. in diameter, varies up to 40 per cent depending on depth, with radiation excited at 80 kv. To reduce the error due to the size of the chamber below 10 per cent for radiation of copper half value thickness greater than 0.3 mm., the diameter of the thimble chamber should not be greater than 1 cm.

TABLE II.—TISSUE DOSE IN ROENTGENS CORRESPONDING TO A SKIN SURFACE DOSE OF 100 ROENTGENS, AT 50 CM. FOCUS–SKIN DISTANCE

Depth (cm.)	Open Port				Cover on Treatment Cone 1/8 in. Bakelite				Cover on Treatment Cone 1/4 in. Bakelite			
	Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)			
	5	25	100	400	5	25	100	400	5	25	100	400
	120 kv. peak No filter H.V.L. 1.0 mm. Al or 0.035 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	56	61	65	68	61	66	69	73	65	68	73	75
3	26	32	38	43	30	36	41	45	33	38	44	48
5	14	18	23	27	17	20	26	30	18	21	28	32
7	8	10	15	19	10	12	17	21	11	13	18	23
10	4	5	8	12	5	6	9	14	6	7	10	16
	150 kv. peak 5 mm. Al filter H.V.L. 0.3 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	81	91	96	99	81	90	95	98	81	87	93	95
3	53	66	77	84	53	63	73	80	53	61	70	77
5	34	45	56	65	34	44	54	62	34	42	52	60
7	22	31	41	50	22	30	40	48	22	29	38	47
10	12	18	26	35	12	17	25	33	12	17	24	32
12	7	12	19	27	7	11	19	25	7	11	18	25
	200 kv. peak 0.5 mm. Cu filter H.V.L. 0.9 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	83	93	98	101	82	91	96	98	82	89	94	96
3	55	70	80	88	55	67	76	84	55	65	73	81
5	38	49	60	72	37	48	57	68	37	46	55	65
7	25	35	46	57	25	34	44	54	26	33	42	52
10	15	21	30	41	15	20	29	38	15	20	27	37
12	11	15	23	33	11	14	22	31	11	14	21	30
15	5	9	16	22	5	8	15	21	5	8	15	20
20	3	4	8	11	3	4	8	10	3	4	8	10
	200 kv. peak 2.0 mm. Cu filter H.V.L. 1.8 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	83	93	98	101	83	91	96	99	83	90	94	97
3	56	71	81	89	57	68	78	85	57	65	75	82
5	39	51	63	74	39	49	60	70	38	48	58	67
7	26	37	48	59	26	36	46	57	27	35	45	54
10	16	23	34	44	16	22	32	42	17	22	31	41
12	12	17	27	36	12	16	26	35	12	16	25	34
15	6	11	19	25	6	10	19	24	6	10	18	24
20	3	5	9	14	3	5	9	13	3	5	9	13

4. If the tissue exposure is to be evaluated as a percentage of the free air exposure, of the skin exposure, or in terms of the reading of a monitor, *the free air exposure, skin exposure, or monitor reading should be measured by the methods described in this bulletin.* The most common practice is to express tissue exposure as a percentage of the skin exposure.

5. Unless otherwise specifically stated, tissue exposures are measured at points along the central axis of the x-ray beam.

6. The tissue exposure having been determined by previous measurements with the phantom, *the following conditions should be closely reproduced* in using the data for actual treatment: *kilovoltage, filtration, focus–skin distance,* skin *area* exposed, *depth* of tissue, and other material directly below skin surface and the *density* of these materials. A new calibration should be made if the conditions are altered by—

(1) Changing the treatment cone,
(2) Changing the metal of the filter,
(3) Changing the thickness of the primary (more dense) filter by more than 20 per cent (ignore so-called

"copper equivalent" of tube wall and oil),

(4) Changing the kilovoltage by more than 15 per cent.

Tissue exposure measurements made in phantoms as described above are directly applicable for treatment in regions containing predominantly muscle or connective tissue. Owing to differences in density and atomic composition, they differ slightly from the true tissue exposure in regions containing predominantly bone, cartilage, or lung tissue. If the tissue exposure is desired in a part of the body such that the thickness of material beyond the point in question (including wood or other support behind the exposed part of the body) is less than 7 cm., measurements must be made in a phantom of proper size, or a correction must be made to values obtained in a large phantom. The latter method will be discussed in the next chapter.

7. *Measurements of time should be made with a watch.* Hand timers and other automatic switching devices should not be used. (See Chap. IV, par. 5.)

CHAPTER IX.—USE OF TABLES TO DETERMINE THE TISSUE EXPOSURE FROM SKIN EXPOSURE

Table II gives the tissue exposure corresponding to a skin surface exposure of 100 roentgens, for a wide range of qualities of radiation and of field sizes. The data have been selected because they are applicable to surface dosage measured by the method described in Chapter V. Similar tables may be found in the literature, but they should not be used with skin surface dosages measured by the methods of Chapter V unless it is known that they are applicable.

The difference between the tissue exposure and the skin exposure depends on six factors: the depth below the skin surface, the thickness and composition of the treatment applicator cover, the quality of the x-rays, the thickness of tissues and support directly below the skin, the area of the skin surface exposed, and the target–skin distance.

The skin exposure, from which the tissue exposure is to be determined by the use of tables, may be measured by the method of Chapter V, or determined from the measured exposure in free air by the tables of Chapter VII.

The following precautions should be observed in using the tables:

(1) *The skin exposure should be measured by the methods described in Chapter V, or determined from the free air exposure by the tables of Chapter VI,* and corrected to apply to the skin in contact with the applicator cover. Otherwise the data will not be applicable. The skin exposure should be determined at the center of the exposed field.

(2) *If an iometer or other type of monitor ionization chamber is used to indicate skin exposure, the monitor should be calibrated by the method described in Chapter VI.*

(3) *Correction should be made to the tissue exposure obtained from Table II if the target–skin distance differs from 50 cm.* Table II gives the tissue exposure for a target–skin distance of 50 cm. For other target–skin distances correct by multiplying the value obtained from Table II by the factors given in Table III.

Example: Using x-rays generated at 200 kv. peak, having a h.v.l. of 1.8 mm. Cu, at a distance of 70 cm., and with a field of 200 sq. cm., the cover on the treatment cone being 1/8 in. bakelite, find the roentgens delivered at a depth of 12 cm. per 100 r on the skin. This field size is not included in the tables; it is necessary to interpolate. From Table II, for this quality of radiation and this cone cover, for 100 r on surface, with 50 cm. target–skin distance, at 12 cm. depth in

100 sq. cm. field—26 r;
400 sq. cm. field—35 r.

This is a difference of 35 − 26

TABLE III.—FACTORS FOR DETERMINING DEPTH DOSES AT VARIOUS FOCUS–SKIN DISTANCES IN TERMS OF EACH DEPTH DOSE FOR 50 CM. DISTANCE AS 100 PER CENT, SKIN DOSE 100 PER CENT FOR EVERY DISTANCE

Depth (cm.)	Focus–skin Distance (cm.)					
	30	40	50	70	100	150
	Percentage of Depth Dose at 50 cm.					
0	100	100	100	100	100	100
1	97.5	99.0	100	101	102	103
2	95.5	98.0	100	102	104	105
3	93.2	97.5	100	103	106	108
5	89.2	95.5	100	106	110	114
7	85.5	94.2	100	108	114	119
10	81.2	92.3	100	110	118	126
12	78.5	91.0	100	112	122	133
15	75.5	89.8	100	115	127	140
20	70.5	87.4	100	119	136	153

= 9 r for a difference of 300 sq. cm. For a difference of 100 sq. cm., the difference in exposure would be about one-third as great, or 3 r. Hence, at 50 cm. distance the depth dose is

26 r + 3 r = 29 r per 100 r on the skin.

From Table III it is found that at 12 cm. depth the dose with a 70 cm. distance is 112 per cent of that with 50 cm. distance.

112 per cent of 29 r = 32 r per 100 r on the skin.

It should be pointed out that the inverse square law holds only for point sources of radiation. For positions close to the filter or tube holder, deviations may be considerable. Exposure data for such positions must be determined by special calibrations rather than by attempting to correct tables of values obtained at longer distances.

(4) *Errors due to insufficient depth of underlying tissue should be avoided.* The data given in Table II have been determined by the use of a very deep phantom. They are directly applicable for the determination of the exposure in very thick parts of the body, except within a few centimeters of the skin surface where the rays emerge on the side of the body remote from the x-ray tube (*i.e.*, the "exit field"). Owing to the small thickness of the material contributing to the back-scattering in this region close to the exit field, the exposure is smaller than that given in the table. This difficulty is particularly important in the treatment of thinner parts of the body. Significant errors due to this cause may be avoided either by applying corrections or by providing a layer of suitable material over the exit field.

The second alternative, that of providing a layer of material behind the exposed part of the body, is much simpler than the other and has the additional advantage of increasing the depth dose. It has, however, the disadvantage of increasing the exit exposure. Blocks of hardwood or bags of rice may be used for this purpose. The total thickness of material (including tissue, the additional material provided, the mattress and wood top of the treatment table immediately underneath) in the x-ray beam beyond the place at which the dose is to be determined, should be at least 8 cm. Under these conditions, the data given in this chapter are applicable without corrections for errors due to lack of underlying scattering material.

TABLE IV.—DEPTH AND EXIT DOSES IN PHANTOMS OF LIMITED THICKNESSES: TISSUE DOSES CORRESPONDING TO A SKIN SURFACE DOSE OF 100 ROENTGENS 200 KV. PEAK. 50 CM. FOCUS–SKIN DISTANCE. COVER ON TREATMENT CONE 1/8 IN. BAKELITE

Depth (cm.)	Irradiated Area 5 sq. cm.			Irradiated Area 25 sq. cm.			Irradiated Area 100 sq. cm.			Irradiated Area 400 sq. cm.		
	Thickness of Part (cm.)			Thickness of Part (cm.)			Thickness of Part (cm.)			Thickness of Part (cm.)		
	5	10	15	5	10	15	5	10	15	5	10	15
	0.5 mm. Cu filter H.V.L. 0.9 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	80	82	82	87	91	91	90	96	96		98	98
3	51	55	55	62	67	67	68	75	76		82	84
5	34	37	37	41	47	48	44	55	57		64	68
7		24	25		33	34		41	43		48	52
10		13	15		16	20		22	28		27	36
12			11			13			20			27
15			4			6			11			15
	2.0 mm. Cu filter H.V.L. 1.8 mm. Cu											
0	100	100	100	100	100	100	100	100	100	100	100	100
1	81	83	83	87	91	91	90	96	96		99	99
3	53	57	57	63	68	68	70	77	78		83	85
5	36	39	39	42	48	49	46	58	60		66	70
7		25	26		35	36		43	45		50	55
10		13	16		18	22		25	31		30	40
12			12			15			24			31
15			5			8			13			17

The first alternative method, that of applying corrections to avoid error due to this cause, is more difficult owing to the number of conditions which must be considered. Approximate values of exposures with small thicknesses of underlying material may be obtained from Table IV. Actual depth dose data are given for 5, 10, and 15 cm. thick phantoms. By comparing these values with those for thick phantoms, in Table II, one sees what portion of the tabulated dose is due to the back-scatter from the extra layers, and can make this calculated reduction in the case when the extra layers are not there.

Example: With x-rays generated at 200 kv., 0.5 mm. Cu filter, 50 cm. distance, 25 sq. cm. field, 1/8 in. bakelite cover, estimate the exposure at a depth of 10 cm. in a neck 12 cm. in diameter. From Table II, the exposure at this depth in a full phantom is 20 r per 100 r on the skin. To obtain an idea of the reduction necessary, find a place in Table IV for the same field and quality of radiation, where the exposure is given for a position such that about the same thickness of tissue (2 cm.) exists beyond the point of measurement. The exposure at 12 cm. in a phantom 15 cm. thick should serve. This is 13 r per 100 r on the skin. For the thick phantom the dose at the same depth is 14 r. Hence an approximation to the exposure desired should be 13/14 of 20, or 18.6 r per 100 r on the skin. This is still slightly high because the values from Table IV were for 3 cm. of tissue beyond the point, while in the problematical case there are only 2 cm. Hence the exposure can be taken as 18 r per 100 r on the skin, as a satisfactory approximation.

TABLE V.—TISSUE DOSE IN ROENTGENS CORRESPONDING TO A FREE AIR DOSE OF 100 ROENTGENS AT 50 CM. FOCUS–SKIN DISTANCE

Depth (cm.)	Open Port				Cover on Treatment Cone 1/8 in. Bakelite				Cover on Treatment Cone 1/4 in. Bakelite			
	Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)				Irradiated Area (sq. cm.)			
	5	25	100	400	5	25	100	400	5	25	100	400
	200 kv. peak 0.5 mm. Cu filter H.V.L. 0.9 mm. Cu											
0	114	124	136	149	114	125	138	153	115	126	141	158
1	95	115	133	150	95	114	132	150	94	111	133	152
3	63	87	109	131	63	84	105	129	63	82	103	128
5	43	61	82	107	43	60	77	104	43	58	77	103
7	29	43	63	85	29	42	61	83	29	42	59	82
10	17	26	41	61	17	25	40	58	17	25	39	58
12	13	20	33	50	13	18	30	48	12	18	30	48
15	6	11	22	33	6	10	21	32	6	10	21	32
20	3	5	11	15	3	5	11	15	3	5	11	16
	200 kv. peak 2.0 mm. Cu filter H.V.L. 1.8 mm. Cu											
0	109	117	126	136	109	117	127	138	110	118	128	139
1	90	109	124	137	91	107	122	137	91	106	120	135
3	61	82	102	121	62	80	90	117	63	77	96	114
5	42	60	79	101	42	50	76	97	42	57	74	93
7	28	43	60	80	28	42	58	79	29	41	57	75
10	17	27	43	60	17	26	41	58	19	26	40	57
12	13	20	34	50	13	19	33	48	13	19	32	47
15	7	13	24	34	7	12	24	34	7	12	23	35
20	3	6	11	19	3	6	11	18	3	6	11	18

CHAPTER X.—USE OF TABLES TO DETERMINE THE TISSUE EXPOSURE FROM THE FREE AIR EXPOSURE

Table V gives the tissue exposure at various depths, corresponding to an exposure *in free air* of 100 roentgens, at the position of the skin surface. It has been compiled from the same data as the tables in Chapters VII and IX. The table applies only to a limited number of conditions, *i.e.*, 50 cm. focus–skin distance, 0.9 and 1.8 mm. Cu h.v.l., covers of 0, 1/8 in. and 1/4 in. bakelite on the treatment cones, and 5, 25, 100, and 400 sq. cm. fields. These conditions are, however, frequently employed in therapy at the present time.

A comprehensive table applicable to all conditions likely to be used would be difficult to prepare and inconveniently lengthy, owing to the large number of conditions that affect the tissue exposure. If it is desired to determine the tissue exposure from the free air exposure for a set of conditions not included in Table V, this may be done in two steps, *viz.*, by (first) obtaining the skin dose from the free air exposure by the method of Chapter VII, and (second) using the skin exposure so obtained to determine the tissue exposure by the method of Chapter IX.

If a particular set of conditions that is not included in Table V is used frequently, the radiologist will find it convenient to prepare a suitable table on the same principles as Table V. For example, the data for 50 cm. focus–skin distance, 1/4 in. bakelite cover, 400 sq. cm. field, 0.9 mm. Cu h.v.l., which are given in the right-hand column of Table V, were obtained by the following method: From Table I it is seen that, for the above conditions, a free air exposure of 100 r is equivalent to a skin exposure on a large phantom of 158 r. Multiplying the depth doses in Table II by the ratio of these (1.58) gives the desired table of depth exposures as percentages of free air exposures *for the same physical factors.*

The table thus prepared is applicable to a focus–skin distance of 50 cm. For any other distance an additional step in the calculations is required; the values obtained by the above calculations must be

multiplied by the percentages given in Table III for the desired distance.

Tissue exposures determined from Table V, or from tables suited to particular conditions as suggested above, may be in error due to a shallow depth of underlying tissue. (See Chapter IX, recommendation 4.)

GLOSSARY

Absorption:[7] Attenuation of a beam of x-rays when traversing matter, by deflection of part of the rays from the original direction of propagation, and by the conversion of the energy of part of the rays into other forms of energy, such as motion of ions, etc. (The word is also used to mean these two processes that cause the attenuation.)

Absorption Curve: Curve showing percentage of radiation remaining after the beam traverses increasing thickness of a particular substance. (More appropriately called "transmission curve.")

Ångstrom (Å.): Unit of length for defining visible light and x-rays, equal to one ten-millionth of a millimeter.

Atomic Number (At. No.): Number expressing net positive charge on nucleus, or number of orbital electrons, in the neutral atom.

Atomic Weight (At. Wt.): Weight of atom, in terms of weight of oxygen as 16.

Back-scatter: Radiation scattered backward within a volume of matter and re-emerging through the area of incidence.

Calibration: Comparison of a practical measuring instrument or device with another which is known to read correctly. Calibration of x-ray roentgen meters may be done at the National Bureau of Standards (Washington, D. C.), the National Research Council of Canada, or certain officially recognized private laboratories.

Corpuscular Rays: Sub-atomic particles travelling at very high velocities; beta rays, cathode rays, protons, deuterons, alpha rays, neutrons.

Corpuscular Rays, Secondary: Electrons released at very high velocities in consequence of absorbing energy from x-rays.

Depth Dose: Dose at specified depth below the surface of the body (or phantom).

Diaphragm: Aperture, usually variable in size, through which x-rays emerge from tube holder.

Dosage Rate: *See* Exposure rate.

Dose: *See* Exposure.

Dose Meter: *See* Roentgen meter.

Dose Meter, Integrating: A dose or roentgen meter which determines the total exposure during a treatment.

Effective Wave Length: (λ Eff.) Wave length of monochromatic radiation which would be absorbed in a specified substance at the same rate as the heterogeneous beam under consideration.

Electrode: Electrically charged terminal in x-ray tube or ionization chamber.

Electrometer: An instrument for measuring differences in potential, applicable with accessories for measurement of very small currents.

Electron: Sub-atomic particle having negative charge of electricity; the natural elementary quantity of negative electricity.

Electron, Photo: Electron set into swift motion by the impact of a photon; the photon gives up all of its energy and ceases to exist.

Electron, Recoil (or Compton): Electron set into motion by impact of a photon; the photon loses only a part of its energy.

Electrostatic Unit of Charge (E.S.U.): The quantity of electricity which repels an equal and similar quantity at a distance of one centimeter in a vacuum with a force of one dyne (2,095,000 times the charge on an electron).

Electrostatic Unit of Current: The passage of one electrostatic unit of charge per second (3.33×10^{-10} amperes).

Exit Skin Dose (or Exposure): The skin dose or exposure in the exit field. *See* Skin dose.

Exposure; Dose: A physical quantity, expressed in roentgens, associated with the radiation traversing a specified point during a specified time, treatment, or act of irradiation. The number of roentgens that would be recorded by a standard ionization chamber traversed by photons identical in quality and number per unit cross sectional area with those traversing the specified point in the specified act of irradiation.

Exposure Meter: *See* Roentgen meter.

Exposure Rate: Dosage Rate: The exposure per unit time.

Extra-hard Radiation: X-rays generated with more than 250 kv. applied to the x-ray tube, and filtered by at least 0.5 mm. Cu or the equivalent. X-rays of h.v.l. more than 3 mm. Cu.

Field: The area on the skin through which a given beam of radiation passes.

Filter: Any substance interposed in a beam of radiation for the purpose of changing its quality.

Filter, Composite: A filter of three or more metals of different atomic numbers (the highest being the primary filter).

Filter, Primary: The filter employed to produce the desired effect on the primary beam.

Filter, Secondary: The material inserted between the primary filter and the patient to absorb the soft secondary radiation from the primary filter.

Filter, Thoraeus: A composite filter of tin, copper, and aluminum.

Filtration: Changing the quality of a beam of radiation (usually to make it more penetrating) by passing it through a layer of material.

Focal Skin Distance: Distance from focal spot of x-ray tube to skin of patient.

Free Air Dose: *See* Free air exposure.

Free Air Exposure: Exposure in air when the scattered radiation is negligible.

Frequency: Number of cycles or complete oscillations per second. (Denoted by Greek letter ν, nu.)

Half Value Layer (h.v.l.): Thickness of material which reduces to half the intensity of a particular beam of radiation.

Hard X-rays: X-rays of short wave length or large half value layer; penetrating x-rays.

Heterogeneous Radiation: A beam of radiation containing a mixture of many wave lengths.

Homogeneous Radiation: A beam of radiation of a single wave length.

Intensity: Dose or exposure per unit time.

Inverse Square Law: Statement of the diminution of radiation from a point source with distance from the source. The intensity of radiation at any point varies inversely as the square of the distance from the point source, provided there is negligible absorption in the intervening space.

Iometer: Trade name of a particular type of monitor ionization chamber (q.v.).

Ion: Charged particle. The fundamental part of the negative ion is an electron; of the positive ion an atom from which an orbital electron has been removed. Either of these particles may attach itself

[7] Absorption, as defined here, is often called "total absorption," as it is sometimes desirable to distinguish the parts of the absorption due to the different processes that contribute to it.

to a group of molecules, in which case the whole group becomes the ion.

Ionization: Breaking the neutral, normal atom or molecule into ions, usually by the action of radiation.

Ionization Chamber: Device for collecting ions produced in a definite region in the beam of radiation. When it is suitably attached to a proper measuring instrument, this affords a measure of the exposure or dose.

Ionization Chamber, Air Wall: A chamber with walls of material of low atomic number, having the same effective atomic number as atmospheric air.

Ionization Chamber, Extrapolation: An ionization chamber arranged so that the enclosed air volume may be altered and reduced almost to zero.

Ionization Chamber, Thimble: Small enclosed ionization chamber having a volume usually less than two cubic centimeters.

Ionization Chamber, Thin Wall: An ionization chamber having walls so thin that nearly all secondary corpuscular rays reaching them from external materials can penetrate them easily.

Ionization Chamber, Standard: An ionization chamber so designed, with due regard to the definition of the roentgen, that dose may be determined by calculation from the electric charge accumulated during irradiation.

Irradiation: Act of administering radiation.

Monitor Ionization Chamber: An ionization chamber used for checking the constancy of performance of the x-ray apparatus.

Monochromatic Radiation: Radiation of a single wave length. (Homogeneous.)

Neutron: Sub-atomic particle having no electric charge, and mass approximately equal to that of the hydrogen atom.

Percentage Depth Dose: Amount of radiation delivered at a specified depth in tissues in percentage of that delivered at the center of the skin field.

Phantom: Volume of material, usually water or some organic substance, used as a substitute for the human body in experimental studies of distribution of radiation.

Port: The aperture in the tube holder through which the beam emerges. *See* Diaphragm.

Port, Open: Omission of cones or applicators between the port and the skin field.

Photon: A single ray (*e.g.*, x- or gamma-ray). The quantity of radiant energy involved in a single process when the radiation acts on an atom or atomic particle.

Primary Radiation: Radiation received directly from the focal spot of the tube.

Quality: The property of the radiation which determines the manner in which it affects and is altered by the matter it traverses, expressed quantitatively in terms of wave length (q.v.) or half value layer (q.v.).

Quantum: Amount of energy associated with a photon; the x-ray energy reacting with or affecting a single atom or ion.

Recombination: Reunion of positive and negative ions to form neutral atoms.

Roentgen (r): Unit of dose or exposure of x-radiation. The quantity of x- or gamma-radiation such that the associated corpuscular emission per 0.001293 gram of air produces, in air, ions carrying 1 e.s.u. of quantity of electricity of either sign.

Roentgen Meter: An instrument for measuring exposure, usually by ionization methods.

Saturation Current: Current that would be obtained from an ionization chamber if all ions formed by the radiation were utilized.

Saturation Voltage: Voltage on ionization chamber necessary to obtain almost saturation current.

Scattering: Change in direction, and (usually) quality of radiation in passing through matter.

Scattered Radiation: Radiation whose direction has been changed and (usually) quality altered by collisions with atoms of material.

Secondary Radiation: Radiation generated in atoms of material by impact of electrons or photons.

Soft X-rays: X-rays of long wave length or low half value layer; x-rays which can be easily absorbed.

Supervoltage: Voltage greater than 250 kilovolts.

Skin Exposure; Skin Dose: Exposure or dose at skin surface.

Target–skin Distance: Distance from center of focal spot of target to center of irradiated area on skin.

Timer: Instrument for measuring the duration of irradiation.

Tissue Exposure; Tissue Dose: Exposure or dose at specified point in tissue.

Total Radiation: Sum of primary, scattered, and secondary radiation.

Treatment Applicator; Treatment Cone: Device inserted into port to define beam of rays at skin field and establish correct target–skin distance.

Wave Length: A quantity (with the dimensions of length) used to specify the quality of x-rays.

X-radiation, Characteristic: X-radiation of quality determined by, and characteristic of, material in which it is generated.

X-radiation, Fluorescent: Characteristic x-radiation.

X-radiation, Continuous Spectrum: X-radiation emitted by the x-ray tube which is not characteristic radiation.

X-ray Spectrum: Radiation distributed or sorted out according to quality.

X Unit: 0.001 Ångstrom.

Prepared by the Subcommittee,
EDITH H. QUIMBY and
GEORGE C. LAURENCE.

Accepted by the Committee,
LAURISTON S. TAYLOR, *Chairman,*
and U. V. PORTMANN, *Sub-chairman.*

CHAPTER 14. STANDARDIZATION IN RADIOGRAPHY

In September 1934, general communication #3 to the members of the RSNA Standardization Committee called attention to the fact that a number of letters had been received suggesting standardization in the field of roentgenography (or radiography). These suggestions ranged from the standardization of films and apparatus to the standardization of techniques. Though such problems were being worked on by several outside organizations, the question raised was whether or not the RSNA Standardization Committee should undertake such work. Communication #4, about a month later, based on pro and con replies from most members of the committee, concluded that it might not be desirable at the moment to take up the question of roentgenographic standards.

Dr. G. Failla, a physicist, stated that, while he had no professional interest in the field of roentgenography, he thought it would be a good thing for the committee to look into. Likewise, Dr. K. W. Stenstrom, then an associate professor in biophysics, thought it advisable for the Standardization Committee to extend its activity to roentgenography. Moreover, he expressed interest in the development of a small apparatus which would be placed on each film during the exposure and automatically record the exposure time and hardness of the rays. For example, a small disk with a hole in its periphery when rotated by clockwork, would show the number of impulses of exposure if an alternating current was used. Some type of penetrometer would record the hardness of the radiation.

On the other hand, Portmann, an M.D., and Glasser, a physicist, both at Cleveland Clinic, saw no need for the RSNA Committee to become involved in a roentgenographic study. Though standardization of films and apparatus was desirable, they felt that it was a matter better taken up by the manufacturers. As to the standardization of the radiographic technique, they believed that it would be impossible to come to any satisfactory agreement, since roentgenologists would want to use their own individual methods. By and large the general response of the committee membership was lukewarm and no roentgenographic standardization action was undertaken at the time. (In 1980, we are seeing an endeavor to accomplish this type of standardization essentially through the Federal Government regulatory process.) However, the questions kept arising and finally, as noted in the Committee Report of December 1936, Dr. George Henny was appointed Chairman of a Sub-Committee on Diagnostic Standardization (see p. 147). Although the problem had been discussed on many occasions by the main committee, it was complicated by so many variables that the main committee never seemed to get a real handle on how to proceed. Henny, in the meantime, had prepared a statement on "The Standardization of Roentgenographic Technique". It was an excellent report and follows:

October 26, 1936

THE STANDARDIZATION OF ROENTGENOGRAPHIC TECHNIQUE

If roentgenographic films of the same body part which vary widely in density and contrast are given to each of a large number of Roentgenologists and a vote is taken on which is the best diagnostic film, it will be found that there is quite a difference of opinion. Some men will like a light film, some a dark one. Some will like high contrast, and others contrast which is not so high. This difference of opinion is due partly to differences in training which result in likes and dislikes regarding general density and contrast, partly to differences in eyes and partly to differences in lighting conditions used in the viewing of films.

From the objective point of view, it can be shown that one of the various films mentioned above has recorded on it more information than any of the other films. This film has qualities of density and contrast which can be studied by physical means. Such a study involves the density of the areas under consideration, the contrast characteristics of the finished film and the degree of unsharpness or blurring due to focal spot size and intensifying screen. If one film can be shown to actually have recorded on it more information than any of the other films, and if the general characteristics of this film are such that it is favorable for visual inspection under

ordinary lighting conditions, then the technique used in the making of this film is the one which is best. By standardizing such technique it would be possible to get the most information on each roentgenograph and therefore the standardization would be highly desirable. Such standardization would have to take into account not only roentgenographic technique but also characteristics of the film emulsion, intensifying screen and the photographic processing in the dark room. The quality of the finished roentgenograph from the objective point of view is influenced, as mentioned above, by:

1. Sharpness of the recorded image.
2. The density.
3. The contrast.

1. Sharpness may be defined as the rate of change of density on the film across the shadow border. The higher this rate of change of density across the shadow border, the greater is the sharpness in the roentgenogram. Sharpness is affected by the following factors:

(a) The size of the x-ray tube focal spot in relation to its distance from both the film and the object.

(b) The faithfulness with which the x-ray film and the intensifying screen reproduces the actual x-ray shadow.

(c) Movement of the object during the x-ray exposure.

(a) The size of the focal spot of the x-ray tube can be directly determined by use of the pinhole camera. The distance of the film from the tube and of the object from the film can be directly measured, so the degree of blurring or unsharpness from these factors may be calculated and compared with that occurring in other x-ray departments.

(b) The faithfulness with which the x-ray film and intensifying screens reproduce the actual x-ray shadow cannot readily be measured. Apparent sharpness in a roentgenograph depends partly on the phenomena of vision and therefore a visual comparative test is of value. To make such a test a wire mesh of number 28 copper or nikrome wire is placed directly on top of the cassette and an x-ray exposure is made with a tube-film-distance of four feet. The exposure should be such that the background on the film is of average roentgenographic density. It is of great importance in comparing two such films to have the background densities equal. Blurring of the wire shadow may be caused either by the fluorescent material of the screen together with dirt deposited on its surface, or by poor contact between the screen and the emulsion of the film. The size of the tube focal spot has no appreciable effect in this case because the tube is so far from the film and the wire mesh is so close to the film. In modern x-ray films the emulsion grain is so fine as to not be a factor.

(c) Movement of the object during the x-ray exposure is mentioned for the sake of completeness. As far as standardization of technique is concerned all movement should be eliminated.

2. Density of an area of the finished film is defined as the relationship between the intensity I_0 of the light incident upon the area of the intensity I_1 of light transmitted through the area. It is expressed by the equation

$$D = \log_{10} I_0/I_1$$

The density of an area of the processed film depends upon the intensity and duration of the x-ray reaching the film (not altogether on the product of these factors), the quality or hardness of the x-ray, the response of the intensitying screens of this quality of x-ray, the characteristics of the film emulsion, and the processing of the film after exposure. The first three of these factors are regulated by the x-ray technician in terms of tube kilovoltage, current, time and distance. They constitute the x-ray exposure and will be discussed in more detail below. Intensifying screen characteristics may be compared with those of a standard screen. The film processing technique in itself is difficult to standardize. An approach to processing standardization can be made by using freshly made up solutions of given chemicals at a specified temperature and time.

3. Contrast between two areas of a roentgenogram may be defined as the difference in density between these areas. Contrast depends upon roentgenographic technique, the intensifying screen-film combination and the processing. Under roentgenographic technique must be included factors affecting the production of secondary radiation and whether or not a Potter-Bucky diaphragm is used. The contrast characteristics of film-

screen combinations can be compared by making roentgenograms with the same exposure of two identical aluminum step ladders on their respective cassettes. After simultaneous processing of these comparative films the density steps may be studied side by side on the viewing box. This will allow a qualitative comparison of contrast. For a quantitative measure of contrast the density of each step must be measured and from the data so obtained curves of density versus aluminum step thickness are plotted. Proper analysis of the curves reveals the relative contrast characteristics of the various films.

STANDARDIZATION OF X-RAY EXPOSURE

Before discussing this question, it would be to our advantage to know about how much deviation from a standard technique can be tolerated in two roentgenographs of the same part taken at different times which are to be directly compared. In chest roentgenography Weyl and Warren, after making an exhaustive study of the field, have concluded that more than 10% variation in the average density in a chest roentgenogram is objectionable to most Roentgenologists. These authors have also found that a change in x-ray tube voltage of 1.5 kilovolt causes a 10% change in film density, all other factors remaining constant. They conclude therefore that for chest roentgenography the apparatus should be calibrated and controlled exactly enough to be able to duplicate exposures to within at least 1.5 kilovolts. Likewise a change in current of 10% causes an objectionable difference in density between two roentgenographs which are to be compared, so that this factor also must be kept within that limit. What is true of the tolerance for chest roentgenographs is also probably true for other parts of the body but to a greater degree.

For voltage calibration the most widely used method employs the sphere gap of standard size polished spheres. The gap should be carefully set up away from external electrical fields (near-by conductors) and should be protected with series resistors. The pre-reading voltmeter readings are then correlated with the peak kilovoltages across the x-ray tube in the form of curves, a separate curve being drawn for each tube current. In standardizing the condenser discharge machine the voltage across the fully charged condensers must be measured throughout the range to be used and the primary voltmeter reading recorded for each voltage. The capacity of the condenser bank must be known. The total energy delivered to the x-ray tube is then independent of the time of discharge. Various x-ray machines will have voltage wave forms of different kinds and this will reflect itself in the spectral distribution of the produced x-rays. This in turn produces variations from machine to machine in density and contrast of the finished film. To standardize voltage wave form would be extremely difficult due to the large number of variable factors involved. Perhaps an equivalent over-all standardization may be made by the method to be described below which will be an aid to the final solution of the problem.*

*See memo by Taylor, p. 257.

The average tube current for long exposures is easily read with the usual moving coil milliammeter. For short exposures with high current this method cannot be employed so the ballistic milliammeter may be used. As mentioned above, a difference in tube current of 10% produces a perceptible change in density in the roentgenogram. The roentgenographic density is not proportional to milliampere seconds of exposure but depends upon exposure time and x-ray tube current. This effect is known as reciprocity-law failure and is due to the inherent characteristics of the light from intensifying screens that exposes the film and from the film itself.

The time of exposure can, for short exposures, be accurately determined by using a rotating lead disc, containing a hole in its periphery above the cassette, at the time of the exposure. The number of alternations, or 1/120ths of a second (for a 60 cycle current and full wave rectification) will then be recorded on the film. For a three phase or condenser discharge machine the rate of rotation of the disc must be accurately known. From this the rate of angular rotation can be calculated. By measuring the arc over which the record of the timing disc extends and dividing this by the rate of angular rotation, the time of exposure may be calculated to a fair degree of accuracy. For comparatively long exposures the time may be checked with a stop watch.

Having accurately calibrated the x-ray equipment for peak kilovoltage, average tube current and time, the x-ray exposure has still not been standardized when given factors

are used. This is because the wave form of the voltage applied to the tube is different for different machines and different tube windows absorb the x-rays to a varying extent. For the given machine, however, it should be possible to duplicate x-ray exposures once the machine has been properly calibrated.

Since there are so many variable factors which have gone into making up the finished roentgenograph it seems advisable to accurately standardize one of these and then to mass all of the others by measuring the densitites on the finished films. From the densities, the speed and contrast factors of the whole process become available. A possible and perhaps good standard for all roentgenographic departments would be an accurately made step ladder built up from rolled pure aluminum sheet. Such ladders could be built and checked at a central point or they could be built according to specifications, the aluminum sheet being obtained from a specified factory. A ladder made up of 1/8" rolled pure aluminum sheet in steps 3/4" wide and 3" long, having ten steps would cover most roentgenographic techniques. The minimum thickness of aluminum would be 0 or else 1/8" and the maximum thickness would be 1-1/4". Ladders similar to this have been used by Weyl and Warren, R. B. Wilsey, D. R. White and others. If a roentgenographic technique for thick parts is used which penetrates the ladder too much, then additional full length 1/8" aluminum sheets may be added below the ladder. An area of the film protected by 1/8" lead should be provided so that fog may be measured. Standard roentgenographs of this ladder at from 50 to 100 kilovolts peak without and with Potter-Bucky diaphragm may be made on an accurately controlled four valve machine with known characteristics. These films after exposure, having been developed in freshly prepared developer of known composition and at 65 deg. F. for five minutes, could be measured with a densitometer and curves drawn of density versus thickness of aluminum. These curves would then be reference standards for radiographs made of the same (or exactly similar) step ladder in different x-ray departments and with different films, screens, and even developers. The department would be able to tell for a given technique whether or not its over-all speed and contrast were above or below the reference standard and if there was an appreciable difference, appropriate changes could be made to bring the testing machine in line with the reference or standard machine. In making the standard films it is assumed that a fair degree of speed with a high degree of contrast would be obtained.

Having made standard films of a specified ladder with known techniques so that all machines can be brought as nearly as possible into roentgenographic line, it becomes necessary to determine the optimum and practical technique which shall be used to obtain the most information on the finished film as seen under ordinary viewing conditions. At this point it might be stated that the viewing factors can be standardized using a light meter to measure the average light intensity over the surface of the viewing box and the light intensity within the viewing room. The color of the viewing light must also be taken into account. Since individual's eyes vary considerably only limits could be placed on the intensity and color of the viewing light.

To determine the optimum technical factors for roentgenographs of various parts of the body it is necessary to consider the electrical capacity of the equipment. For the standard films mentioned above, a high grade four valve machine with the latest type x-ray tube could be used. The films produced would be of good quality and would be examples which most departments could duplicate. If it were not practical for a lower powered equipment to reach this quality at least allowances could be made and the best quality roentgenographs for the particular equipment might be obtained. With the "standard" roentgenographic machine, techniques for each part of the body and for varying thicknesses of these parts could be worked out to give the highest combination of contrast and sharpness in each case. With these techniques roentgenographs of the standard aluminum step ladder would be made. The density curves obtained from these, or the films themselves would be reference standards by which other roentgenographic departments could check or standardize their machines, intensifying screens, films and film processing. Once the whole process is standardized, the optimum technical factors become useable and roentgenograms with the most information recorded on them are obtained.

G.H.

At about the same time that Henny was working on his report, attention was directed to a unpublished manuscript by C. Zintheo dealing with some of the physical factors involved in radiographic standardization. Taylor circulated Zintheo's comments in a memo to the committee for consideration --the memo follows:

A survey has been undertaken under Dr. Henny to determine the needs, if any, for standardization in the field of diagnostic X-rays.

It has been pointed out in a manuscript by C. Zintheo, that the following physical factors require control and possibly some mode of standardization:

A. Those affecting the voltage through the tube:
Resistance drop of the main leads.
Resistance drop in the controls and transformer.
Transformer design, step-up ratio and capacity. (sic)
Type of rectification, whether self-rectified, mechanical or valve.
Degree of rectification: whether single or four valve; if mechanical, the percentage rectified. (sic)
Rectification losses: these vary with the design of the cross arms, if mechanical, or with the design of the rectifying tubes and the setting of the filament in value apparatus.

B. Those affecting the high tension current through the tube:
Type of stabilizer used, if any, and its adjustment. Losses of current (sic) which have been measured by the milliammeter; these will be of greater or less influence as the meter is more distant or nearer the tube:
Leakage over insulators, posts, brackets and across switches.
Corona loss from sharp points such as on switches, terminals of the aerial system, and frayed points on cord-reel wires. Corona loss from aerial conductors as influenced by the size and spacing of the tubing and of the cord-reel wires.

C. Hidden electrical factors:
Wave form of the voltage, including its general form and super-imposed regular or transient surges. Wave form of the current, including its general form and super-imposed regular or transient surges. The wave form of both the potential and the current is influenced by most of the items listed under A and some of those under D.
Type of main switch used, i.e., whether or not it always makes and breaks at zero potential, thus influencing transient surges and affecting the total exposure for rapid shots. Phase relationship (power factor) of the high tension voltage and current as varied by the design of the control and transformer, the rectifying system, as determined by geometrical arrangement of the aerial leads.

D. Tube factors:
Design of bulb and envelope as it affects the building up and distribution of scattered electrons (the grid effect). Design of the component parts of the tube as it may affect the electrical characteristics of the high tension circuit (wave form, surges and power factor). Degree of vacuum of the tube and kind of residual gas.
Slope of the target face and direction of the beam of rays with reference to the face of the anode. Degree of pitting and cracking of the target. Thickness and kind of material of the tube wall exit portal.
Filtering by a film of tungsten and/or copper on the tube wall vaporized from overloading the target. Tube design with reference to the relationship of saturation voltage at various current loads.

In addition to these there are probably other factors which have been overlooked that also play a part in varying the roentgen-ray output between different laboratories. Undoubtedly the effects of many of these sources are trivial and of no significance from a practical roentgenographic standpoint, but the decision as to which should and which should not be considered noteworthy is safest determined from actual experiment and test.

When specifying by electrical terms only a small part of these variables are controlled. Thus if one has a good milliammeter of proper type and accurate calibration mounted close to the X-ray tube it may be assured that many of the differences caused by high tension current losses are eliminated, as far as comparing that phase of electrical input to the tube with another similar installation is concerned. But even that is not of great significance, for milliamperes do not mean the same thing on all generators

from the standpoint of roentgen-ray intensity; even with all other factors theoretically the same, 20 milliamperes as measured on a self-rectified machine may be expected to yield a different radiation intensity than 20 milliamperes on a four valve circuit. Taylor (25) found in his work in the therapeutic range that effective milliamperes, as measured by a surge-protected and shielded A.C. milliammeter was a more accurate indicator of x-ray intensity at a given voltage than average milliamperes as measured by the customary D.C. meter. Also when suitable correction was made for the suppressed half of the cycle, results from half-wave generators were closely comparable to those from full wave and constant potential apparatus.

L.S.T.

However, interest in radiographic standardization continued. The hope that the American Standards Association would address the problem from the viewpoint of radiological needs dimmed with the passage of time; their interest seemed to focus primarily upon industrial problems. The Standardization Committee therefore urged Henny to continue the efforts of his subcommittee. His next step was to prepare the short note entitled "Need for Standards in the Diagnostic Field" which follows:

NEED FOR STANDARDS IN THE DIAGNOSTIC FIELD
April 10, 1939

It can be shown that a properly exposed roentgenogram which has great photographic contrast and the highest degree of shadow sharpness will contain more information of the part under investigation than a roentgenogram which is under or over exposed and has a lower degree of contrast. Because of personal idiosyncrasies many roentgenologists will prefer a roentgenogram lighter or darker than this "ideal" standard. It appears, however, that more and more roentgenologists are becoming satisfied with such an "ideal" roentgenogram. It is probable that the individual likes and dislikes of the roentgenologist as far as the characteristics of such a roentgenogram are concerned could be taken care of by varying the intensity of the viewing light. Such being the case, the aim of standards in the diagnostic field would be the production of roentgenograms, (a) having the greatest amount of detail in the shadows, and (b) showing photographic densities which are comparable with corresponding roentgenograms of the patient made in the same or in different roentgen ray departments at other times.

The unmeasurable variability of different patients and the required degree of photographic latitude must be taken into consideration in planning a standard roentgenogram of a given part. These factors, together with the limitations of the roentgenographic equipment, require in many cases that a compromise be made. The particular roentgenographic factors for each examination can be arrived at relatively easily although it will be impossible to satisfy everyone. A simple factor such as tube-film distance, for example, varies over quite a wide range in different roentgen ray departments and roentgenologists are prone to resist changes in technique.

The object of standardizing diagnostic roentgenography must be to obtain roentgenograms of standard and superior characteristics, regardless of the roentgenographic or processing equipment employed. Since so many factors (focal spot size, voltage wave form, tube current, intensitying screens, film developing characteristics, etc.) go into making each roentgenogram, it is impractical as well as expensive to standardize each one of these. By making a roentgenogram according to predetermined and proven technical factors of an aluminum step ladder of known specifications and by processing the film under "standard" conditions in fresh solutions which are available on the market, a standard step ladder film would be available. A roentgenologist wishing to check his diagnostic machine would make roentgenograms of an identical aluminum step ladder using technical factors as nearly like those employed in making the standard as he could. After processing the film according to his routine method he would cut it so that the ladder step shadows could be placed side by side with those of the standard over an appropriate mask on a viewing box. A close inspection would tell which was darker (faster) and which had the greatest contrast. He would then have an idea as to where he stood in regard to the standard. X-ray technical factors, and possibly intensifying screens and developing solution would have to be changed until the test film was equal to the standard. It would still be necessary to measure the x-ray tube focal-spot size and to test for intensifying screen grain and contact. The focal-spot size may be determined from a pin hole picture and the intensifying screens are tested by a roentgenogram of a fine wire (#26) mesh which is placed directly on top

of the cassette. From these findings, together with the tube-film distance used in roentgenography, the geometrical unsharpness can be estimated and it may be found whether or not this is within some arbitrarily set standard. S. Reid Warren has suggested the use of, and has been using a small test aluminum ladder placed on the corner of the cassette beside the patient. The shadows of this ladder, as shown on the film, may be compared from time to time with those on films taken previously to determine the status (as far as speed and contrast are concerned) of any roentgenogram. If the roentgenogram is sent to another roentgenologist, the latter, when making a roentgenogram of the same patient, may learn to judge the technical factors to be used by comparing the ladder shadows with those of a duplicate ladder on his own films. He would then obtain a roentgenogram of essentially the same density and contrast as those of the other films and all the clinical roentgenograms could be directly compared.

G.H.

Unfortunately this effort stopped as war clouds began to appear in 1939--it was never resumed.

CHAPTER 15. RELATIONSHIPS WITH THE AMERICAN COLLEGE OF RADIOLOGY (1942)

Prior to 1942, most of the physicists working with the RSNA Standardization Committee, as well as the Registered Physicists, had never heard of the American College of Radiology (ACR). The few who had heard of the ACR had the general impression that it was the business, political, and "trade association" arm of the several radiological organizations. It was not considered as having scientific or technical content.

Thus, when Newell, who had opposed the transfer of the registry of x-ray physicists to the American Board of Radiology, suddenly came up with a proposal in early 1942 that the several technical committees of the Radiological Society of North America and American Roentgen Ray Society be consolidated into a single committee of the American College of Radiology, it was quite a surprise. Because the proposal was made in 1942 when the Country was deeply involved in the war, it was impractical to poll the members of the committees for their opinions. A few reactions obtained informally indicated no great enthusiasm for the proposal. On March 10, Taylor responded to Newell, voicing some questions that had been raised. However, by 1946, when technical activities of the committees were resumed, the decision to consolidate had, in effect, already been reached by the RSNA and ACR officers.

The RSNA Board of Directors were, in part, very much opposed to the consolidation; they therefore organized some new committees within the society's structure when the transfer was made.

Following is a portion of Taylor's March 10 response to Newell's letter of February 26, 1942 (which is not available).

March 10, 1942

> With reference to your communication of February 26, I am in agreement with the principle that the committee work on standards, measurements, and protection could undoubtedly be lightened by some sort of a consolidation. It has, in fact, been lightened in this way by informal understandings by the societies that their committee memberships overlapped to a considerable degree. This has to some extent reduced the work required of the chairman. It is certainly more logical to have a single committee serve all of the interested societies.
>
> One of the greatest difficulties with committees as I view them is the apparent desire to rotate membership so as to gradually include every society member. This may be all right in principle and is certainly all right in certain committees, but in committees dealing with certain technical matters such as X-ray protection, the committees operate much more effectively when their membership is retained intact. This has certainly been demonstrated in the case of the Standardization Committee of the Radiological Society of North America where most of the members have been on the committee since its inception in 1927. The same is true of the Advisory Committee on X-ray and Radium Protection (this Committee is not, however, a committee of any of the Radiological Societies, but rather is an outside committee which includes members of the Radiological Society, A.M.A., Manufacturers, etc.) where the only membership changes have been caused by the death of a member.
>
> Should all of this committee work be taken over by the College of Radiology, I wonder how it will handle the question of physicists who, by its constitution, cannot be members of the College. I do not believe that most physicists would care to devote much time to committee work where their services were of a consultative and transitory nature.
>
> L.S.T.

At that time physicists could not be members of the College nor chair a College committee or commission. Thus, the chairman had to be a radiologist or at least a medical person. The organization finally established became known as the Commission on Radiological Units, Standards and Protection (CRUSP). Its first chairman was Newell, who was undoubtedly the best qualified radiologist available for the position.

As events developed, the commission gradually expanded in scope and membership, often with as many as 20 or more people in attendance. At that time, in as much as the College was more politically than scientifically oriented, a wide variety of questions with little or no technical content were put before CRUSP. While one-day-a-year meetings of CRUSP afforded a convenient, and often useful, opportunity to meet with people, they produced very limited concrete results of scientific or technical significance. This led to further shifting of membership to the point where there was very little basic scientific competence among the radiologist members of CRUSP.

With respect to the transfer of the Standardization Committee, the RSNA Board members moved to preserve the prestige position which the committee had brought to the society. They established a new "Physics Committee" and appointed Taylor and U. V. Portmann, M.D., as chairman and co-chairman, the same positions they had held on the Standardization Committee. During this period (late 1940's) it was becoming increasingly difficult to convince busy radiation physicists to devote time to a lameduck committee designed to replace the old and very effective Standardization Committee. The committee has continued, but its directions have changed markedly.

By 1950, the National Committee on Radiation Protection had replaced the old Advisory Committee on X-ray and Radium Protection, and had become extremely active and influential. Also, at about that time, the International Commission on Radiological Protection was reorganized, strengthened, and began to also play a dominant role in the protection field. Simultaneously, the International Commission on Radiation Units was reorganized and expanded. Thus, the three organizations, NCRP, ICRP, and ICRU, succeeded in attracting the bulk of the scientific talent, including biology, medicine, protection, and measurement. The result was a sharp decline in the close collaboration between the Government and the radiological organizations, as the cooperative efforts shifted largely to these three organizations. This soon caused dissatisfaction among the borderline scientists who were unable to be full members of any radiological society, yet were asked to participate in the scientific and technical matters of the societies. This led to the organization of the "American Association of Physicists in Medicine," a growing and useful organization having ties to the Radiological Society of North America but still not really a part of it.

Taylor completed his assignment with the Ninth Air Force and returned from Europe in the summer of 1945; in mid fall, he was able to re-direct his attention to matters of radiology. In October, he wrote to Dr. Childs, Secretary of the RSNA, that he was "reporting for duty," although he was still committed to organize an Operation Research Division for the U.S. Continental Air Command. The Air Force permitted him to spend some time with the National Bureau of Standards on various activities which were expected to be resumed.

The Committee on Safety and Standards of the ARRS, of which Taylor had been chairman, had its first post-war meeting on November 11, 1945, under the acting chairmanship of Dr. Newell. No physicist was able to attend. The discussions touched upon some problems that would evolve from the new radiation situations introduced during the war years. For example, during this period the betatron had been developed. In fact, by the time of the November 11 meeting, the National Bureau of Standards had already submitted a supplemental request for a betatron laboratory and two betatrons. (As mentioned earlier, Taylor prepared testimony and testified before the Senate Subcommittee while still with the Continental Air Command (see p. 191).) Following is the report of the Committee Chairman:

REPORT OF THE COMMITTEE ON STANDARDS
AND SAFETY OF THE ARRS OR ACR (?)

This committee met in Chicago, November 11, 1945. Doctor Newell reported that he thought the Z-54 War Committee of the American Standards Association is doing a thorough job of the Safety Code for Industrial Use of X-rays, under chairmanship of George Singer of the National Bureau of Standards. The first part has been published (Industrial Standardization: 16, 141-144, July, 1945). The subsequent parts are near completion.

The membership of the Committee is broad, comprising physicians, representatives of some radium and x-ray companies, airplane and automobile industries, insurance, and the Army and Navy, and the National Bureau of Standards.

Approved, but not carried out, was the suggestion to list the agencies and organizations affected by these hazards and this code, and to devise an ideal organization of them for the purpose of forwarding the safety program.

This committee considered carefully the matter of radon by mail to general practitioners. The commercial pumpers of radon often offer, as a regular service, the loan of instruments and also advice as to application of the radon. We consider it

hazardous to act on advice of a consultant who has not seen the patient. Therefore we recommend that the College try to get the voluntary cooperation of the companies selling radon in that they routinely avoid giving clinical advice, and routinely enclose with the radon a note of warning such as:

"Our Medical Director has not seen your patient and cannot therefore tell you how to use this radon. If you desire consultation, you should ask a specialist in your neighborhood."

The products of the cyclotron may prove to have serious hazards. At present this committee can offer no special safety code thereon; it can only urge that their use be always in close consultation with physicists and physicians who are experienced in their use. There are as yet no commercial sources of these products as far as we are aware.

The betatron presents new radiation hazards. Against the gamma rays we recommend retention of the present permissible daily dose of 0.1 r. The beta rays are outside the experience of most radiologists, and some may not be sufficiently aware that a moment's careless exposure could result in disaster. Here is a field in which physicist and physician must work together at all times and watch each other with this danger constantly in mind.

Radon ointment is being used in such weak concentrations that the beta and gamma radiations are no hazard. Escaping radon gas could contaminate photographic and electronic apparatus, therefore radon ointment should always be shipped and stored in airtight containers, and wastes containing radon should be disposed directly into sewer or incinerator.

This Committee is in favor of formal certification of physicists for standardization and safety inspection of x-ray equipment. If the American Board of Radiology can undertake this, we recommend its support. If the College has to undertake it directly, it will need the services of physicists, who, ideally, should have membership in the College.

Respectfully submitted,
W. Edward Chamberlain, M.D.
R. R. Newell, M.D. (Chairman)
Robert B. Taft, M.D.

As can be seen from the above report and the following material, confusion as to the parentage of the committee had already developed. Obviously, Newell, and perhaps others, thought it was a committee of the ACR, and yet listed as a member was K. E. Corrigan, a physicist (and acting chairman of the Standardization Committee of the RSNA). Physicists were not then permitted to be members of an ACR committee.

In January 1946, Taylor returned full time to the National Bureau of Standards and began to pick up the loose ends of earlier activities and to determine what direction the programs should take. In connection with these efforts, he wrote to Dr. Ross Golden, then President of the ARRS. Portions of his letter and Golden's reply follow:

January 31, 1946

Having recently returned to the Bureau of Standards after having been out of the country for over two years, I am endeavoring to pick up the loose ends of my work in the radiological field. In this connection I am at a loss to know what connection, if any, I now have with the committee on Safety and Standards of the American Roentgen Ray Society. I was chairman of this committee for some time after 1936. I would appreciate it very much if you would let me know what my status on this would be inasmuch as there are numerous problems arising which should be brought to this committee.

One question in particular involves the relationships between this committee and the corresponding committee in the Radiological Society of North America. Some years ago we had pointed out the desirability of having these committees overlap in membership to a considerable extent thereby eliminating unnecessary duplication of effort or the reaching of recommendations which might otherwise conflict between the two committees. This had worked out very satisfactorily, and I hope that the same arrangement may be continued in the future. This is particularly important in the matter of the Board of Registry of X-ray Physicists which at the present time is being handled through my office. Inasmuch as this activity is sponsored jointly by the two societies, it is

desirable that the arrangement of handling applications, examinations, and records be centralized at one point for the two organizations. Another problem is going to arise in the near future in the field of x-ray protection.

Here again, it is going to be most desirable that there be overlapping of committee membership in order that the same group may act for the two societies. It is likely also that this work will center up at the Bureau of Standards since much of the mechanical work is already being handled here as part of the regular program.

L.S.T.

Dr. Golden replied:

"Last year, this committee consisted of Robert B. Taft, Chairman, with Bernard P. Widmann, Robert R. Newell, Kenneth E. Corrigan and W. Edward Chamberlain as Members. If I am not mistaken, there has been some discussion of centering the work of this committee in a similar committee of the College of Radiology to avoid the overlapping of effort which you mention. Mac F. Cahal, Secretary of the American College of Radiology, could tell you exactly how matters stand. As you say, there should be some arrangement whereby duplication of effort can be avoided."

Further investigation revealed that "The Committee on Safety and Standards" was not a Roentgen Ray Society Committee as most of the earlier members had assumed, but a new one established by the American College of Radiology (see p. 261). For further details, Taylor wrote to Mr. Mac F. Cahal, Executive Secretary of the ACR.

The situation can best be described by Cahal's answer. His letter was full of surprises for Taylor, the first of which was to learn about the resolutions adopted by the RSNA and the ARRS to discharge their committees. Next was the establishment of a new committee by the ACR, consisting of only three people, all of whom were radiologists, and only one of whom (Newell) had had any experience with either radiation protection or measurements. Yet another surprise was the proposal for an amendment to the constitution of the ACR to permit nonmedical individuals to become Associate Fellows. (This was to change the existing rule that only a Fellow of the College could be chairman or a member of their committees. At that time no physicist could belong to one of their committees.) The final bit of intelligence was that the College had asked the American Board of Radiology (ABR) to begin the examination and certification of physicists. This was especially critical because Taylor, acting in his original capacity as chairman of that board, was already processing a number of applications. Moreover, all of the files of the registry were in his office.

For further details, reference may be made to Cahal's letter which follows:

February 28, 1946

Thank you for your letter of February 26.

About two years ago the American Roentgen Ray Society and the Radiological Society of North America both enacted resolutions asking that the activities of their respective committees on safety and standards be combined in a single committee of the American College of Radiology.

Acting upon this request the Board of Chancellors of the College created a new committee, called the Committee on Standards and Safety. Dr. R. R. Newell is chairman of the committee. Other members are Doctors Robert B. Taft and W. Edward Chamberlain. As might be expected, the committee has been almost wholly inactive during the period of the war. Now, the committee plans to undertake a full program again and will assume the same functions formerly carried out by the respective committees of the other two societies. Pursuant to the resolutions adopted by the A.R.R.S. and the R.S.N.A. referred to above, I presume that these societies will discharge their committees.

I am enclosing the annual report (Nov. 11, 1945) of Chairman Newell which was approved by the Board of Chancellors at its meeting on February 3, 1946.

The Board of Chancellors has approved the following proposed amendment to the constitution and by-laws of the American College of Radiology:

CONSTITUTION

Article III, Membership, Section 2, paragraph 6

"Associate Fellows shall be those who in the judgment of the Board of Chancellors shall be deemed eligible for Associate Fellowship because of noteworthy contributions and achievements in the division of physics related to radiology and who in the judgment of the Board of Chancellors will contribute to the objects of the American College of Radiology. They shall not pay dues and shall not have the right to vote or to hold office but they may act as members of committees and commissions."

BY-LAWS
Chapter I, Section 6

"Nominations for Associate Fellowship may be made to the Board of Chancellors by any Fellow and shall be in writing. The nomination shall set forth clearly the qualifications and contributions of the nominee for election to Associate Fellowship. The Executive Secretary shall make a printed list of the approved nominees with a résumé of their qualifications and shall send this by mail with a return envelope to all Fellows for a mail vote. More than ten (10) negative votes will disqualify the nominee for Associate Fellowship. Each newly elected Associate Fellow shall receive a suitable diploma setting forth his election to Associate Fellowship."

I think there is little doubt that this amendment will be adopted by the College at its annual meeting in San Francisco on June 29, 1946. Immediately thereafter I presume a class of Associate Fellows will be elected by the Board of Chancellors and that some of these will be appointed to the Committee on Standards and Safety. Only then will this committee be in a position to properly carry out its responsibilities.

You may be interested in knowing also that the College has asked the American Board of Radiology to begin the examination and certification of physicists.

It was a pleasure to hear from you, and I hope these developments meet with your approval. Undoubtedly there is a considerable amount of work awaiting action by the new Committee on Standards and Safety; it will have a full agenda of activity as soon as the necessary amendments to the by-laws are enacted and the personnel of the committee is suitably enlarged.

M.F.C.

In his next attempt to learn the status of these matters, Taylor wrote to Newell (4/6/46) requesting information on where the various committee activities stood. Referring to the fact that both the ARRS and RSNA had passed resolutions, theoretically turning over the work of safety and standards to the ACR, he noted that since physicists could not be members of the College, there appeared to be no way in which they could play any future role in radiology.

Furthermore, he pointed out that he had received a notice from Dr. Childs, Secretary of the RSNA, that a new RSNA "Standardization Committee" had been formed, an action which appeared to be completely inconsistent with their resolution to turn over this kind of work to the College of Radiology. Actually, Dr. Childs and some others were incensed at the actions of their board, and took immediate steps to insure that some kind of Standardization Committee activity would be continued in the RSNA. There was thus established the new "Physics Committee," which was discussed earlier (p. 260) and which seemed to satisfy their needs under the chairmanship, first of Taylor and then of Wyckoff. However, the committee gradually shifted to other activities under the name "Associated Sciences Committee."

In another communication with Cahal (3/5/46), Taylor again inquired about the status of the Registry of Physicists. The confused status was exemplified by part of Cahal's reply in which he said that he did not know who had suggested that the ABR undertake the examination and certification of physicists. He was unaware that there was already an existing body for this purpose, and was under the impression that the physicists themselves had made the request of the ABR. He said,

"During a meeting of the Board of Chancellors of the College last November, Dr. B. R. Kirklin mentioned that the suggestion had been made, and stated that the Board would be perfectly willing to undertake this responsibility if the parent societies desired it. Following this discussion, and acting on the belief that nearly everyone concerned would welcome this development, the Board of Chancellors adopted resolutions requesting that the American Board of Radiology proceed with the certification of physicists. I do not

know whether the other societies participating in the American Board of Radiology have or will take similar action."

Taylor also wrote to Newell, who pointed out that the idea of transferring the certification of physicists to the American Board had been discussed at one of the RSNA Standardization Committee meetings. This was indeed the case (see p. 260); however, neither the chairman nor any of the physicists were subsequently brought into the discussions. Some points of interest in Newell's letter follow:

March 14, 1946

Some years ago, in your presence, the question was brought up about turning over the work of certification of physicists to the American Board. At that time I ventured the opinion that it was being successfully handled where it was, but some of the others thought that it was not the proper set-up, being not sufficiently on a wide base.

Of late years the American College of Radiology has taken the responsibility for most of the political activities of the Radiological Society. This seemed a good move in the direction of unification. I would suppose that the two societies would not change their committees on standardization and so forth, and make those committee men representatives on the American College Commission.

The laws of the College are to be altered to permit membership of physicists. This will bring the necessary talent within the College.

The American Board of Radiologists already has physicists examining radiologists for certification, the presumption is that the secretary, Dr. Kirklin, would feel free if necessary to get the physicists needed to examine physicists for x-ray standardization, and would probably make a more formal and well crystallized job of the certification of x-ray physicists than our Committee of the Radiological Society has been doing.

At any rate that is the picture as far as I see it, and I do hope that it does not seem to you to have too many drawbacks.

R.R.N.

In the second paragraph, note Newell's reference to "political activities" on the part of the College. This confirmed the general impression that most physicists had as to the role of the ACR. On March 23, Taylor responded:

"I recall the discussion regarding the situation of physicists by the American Board some years ago and as I recall I did not have any very fundamental objection to the plan at the time nor do I at the present. I am mainly concerned, I believe, with the certainty that the physicist is kept properly in the picture. If I could always be certain that such matters would be handled with such radiologists as you and Ed, I would have nothing to worry about, but as you yourself realize you are few and far between and such programs handled by the average radiologist would be very badly mangled. I trust that you have some hand in the American College of Radiology and will exert your efforts towards having the physics part of this work properly handled by physicists. If, as has been pointed out, the laws of the college are altered to permit the membership of the physicist, the problem is from thereon not too difficult. The same general rules would apply to the committees on standardization and protection."

L.S.T.

Having obtained all the information that seemed to be available, Taylor tried to consolidate the data into a single framework. This was done in consultation with a number of the physicists who had served on the earlier committees. On July 18, 1946, he addressed a memorandum to the Board of Chancellors of the ACR and Officers of the ARRS and RSNA. Following is his memorandum:

June 18, 1946

To: Members of Board of Chancellors, American College of Radiology
Officers of the American Roentgen Ray Society and Radiological Society of North America

Subject: Participation of Physicists in Radiology

1. For a year or two prior to the war there were some informal discussions regarding the question of bringing the Registry of X-ray Physicists under the Board of Radiology. On this there were many conflicting suggestions and opinions, and no real agreements were reached--certainly not among the physicists who were then associate members of the Radiological Society of North America and the American Roentgen Ray Society.

Apparently during the war years, this question was the subject of some action by the societies and further ramifications developed beyond simply the Registry of Physicists. This took place at a time when many of the physicists associated with radiology were otherwise engaged in more pressing war activity and could not take the time and effort to pass on such jurisdictional problems. For this reason I urge your careful consideration before deciding upon these matters involving the radiologist-physicist relations.

Within the past few years steps have been taken by the Radiological Society of North America and the American Roentgen Ray Society to discontinue their committees relating to x-ray standardization, measurements and protection, and centralize their activities in the American College of Radiology. I am not in particular disagreement with this plan but feel that certain conditions must be met to insure the proper functioning of such work, and to insure complete acceptance and understanding on the part of the physicists involved. I shall give below some of my personal ideas on this subject. These have been discussed with a few of the leading physicists working in the field of radiology and we find ourselves in substantial agreement.

2. Committee Work. This might be either in the American College of Radiology or the societies. For a number of years prior to the war, the corresponding committees of the American Roentgen Ray Society and the Radiological Society of North America have had an almost 100% overlap in membership with the result that committee meetings were held at each of the annual society meetings and a fine degree of coordination and continuity resulted. I believe that during this period the committees functioned very effectively. One of the principal reasons for this was that most of the physicists involved were regular attendants of the society meetings.

The same condition might obtain under the auspices of the American College of Radiology provided the College held its meetings in conjunction with the annual meetings of the two societies. Unless the meetings are so held, I am reasonably certain that you could not get the physicists to attend the meetings of the American College of Radiology. Most of them already carry a heavy meeting and travel load and would not be in a position to undertake more.

3. Registry of X-ray Physicists. Here a more fundamental problem presents itself. The question of the registry of x-ray physicists originated in the Standardization Committee of the Radiological Society of North America because of some bad practices by manufacturers' agents. The program was subsequently adopted by both societies and has been reasonably effective in assuring a better grade of unbiased technical advice to the radiologist. The work was carried out by an examining board of four physicists and one radiologist, and the policies of the board were set up by the Standardization Committees and the board itself. All of the applications, records, etc., have been handled through my office as chairman of the Standardization Committee, and a number of new applications are being processed at present. It has been suggested that this work be carried out under the auspices of the Bureau of Standards, but I am not sure that this would be wholly satisfactory.

At the outset it was agreed that this work should be carried out under the principal direction of physicists. They alone are adequate judges of the technical qualifications of an x-ray physicist, and know his problems and his approach to the field of radiology. This is just as reasonable as the basic requirement that a radiologist pass on the qualifications of another radiologist.

I would, therefore, insist that whatever future program be set up for the registry of x-ray physicists, it be run by physicists with assistance of radiologists.

With the setup in the two societies, this was effectively accomplished. Physicists are associate members of both societies, take part in proceedings and serve on

committees. The policies affecting physicists, physical measurements, and standards are effectively in the hands of physicists--of course with the full cooperation of the radiologist. With affairs in the hands of the American College of Radiology, as proposed, it is difficult to see how this situation can continue, and unless it does I can assure you that an effective registry of x-ray physicists will cease.

4. Physicists in the American College of Radiology. It has been proposed to have a category of associate membership for physicists in the American College of Radiology to overcome this difficulty. This would no doubt be satisfactory (provided it does not mean separate meetings from the societies). The physicists are not particularly anxious to add the American College of Radiology to their list of technical organizations, however fully appreciating the aims and ideals of the college. Professionally, it just doesn't add anything one way or another, any more than for a radiologist to belong to the Optical Society. If, on the other hand, such membership can serve some real purpose, I am sure they would not object. Perhaps my own feelings would be summed up in this way: As a recognition by my radiological associates over many years--yes; as simply membership in another society--no; as a means of carrying on what we have already carried on for many years--why change?

The major interests of the physicists in radiology are problems of standards, dosage, protection, physical measurements and the registry of x-ray physicists. I think the physicist will be most productive if allowed to do this thing in the most direct way possible and with the least subterfuge.

5. Board of Radiology. I think it a fundamentally sound idea to place the registry of x-ray physicists in the hands of the Board of Radiology. They have the experience, prestige, and facilities for carrying out this work. But again I insist that matters of policy and decision be dominantly in the hands of physicists as it has been in the past. The mechanics for arriving at this can be worked out by the American College of Radiology, the radiological societies and the Board of Radiology--perhaps along the lines mentioned above. I feel strongly that there has been a close comradeship between the radiologist and the physicist for the last twenty-five years and I would hate to see any artificial barriers break this down.

L.S.T.

One of the more vociferous reactions to the communication of June 18th was that from Dr. Arthur W. Erskine, a radiologist who was one of the founders of the RSNA Standardization Committee. Dr. Erskine was one of those actively working to start the programs at the Bureau of Standards, a member of the College, and a former president of the RSNA. His comments about the background and formation of the RSNA Committee are of interest and have been covered in earlier chapters (see p. 260). He also expressed himself very clearly about the purposes and operations of the College, and for that reason, his letter is given below.

June 22, 1946

My opinions have been and are so definite on the questions raised in your letter that I hardly need much "careful consideration before deciding upon these matters." I am definitely opposed to centralizing the committees of the two major societies on standardization, measurements, and protection in the American College of Radiology. The College is not a scientific society. Its two major purposes are to protect the economic status of radiologists and to promote better radiologic education. Any invasion of the field of scientific work of the other two societies on the part of the College should be (and no doubt will be) vigorously opposed, because such a move would merely set up another competitive society and would not, in all probability, lessen the labors of these committees in their respective organizations.

Incidentally, I think your point about the meetings of the standardization committee and the other committees being held at the time of the meetings of the scientific societies is a good one. The meetings of the College are social. All the business is transacted by the Board of Chancellors and the Commissions. There has never been the least semblance of scientific work at the meetings and convocations of the College.

I don't know whether I have told you anything about the organization of the Standardization Committee of our Radiological Society of North America. The motion was made at the Atlantic City meeting in the summer of 1925 to appoint such a committee. When I appointed it, by the greatest stroke of good luck, I appointed Ernst as Chairman, and Glasser as Sub-Chairman, with complete authority to select whatever members of the Committee they wanted, medical men and physicists, and to define the scope of the activities of the Committee. There had been many Standardization Committees before,

which were always dominated by some very well known individual (for example, Professor Duane of Harvard) who had pet projects of their own. Ernst and Glasser, both modest young men, saw within two months that the job was too big for them and turned it over to the National Bureau of Standards. By that time I was a member of the Committee and knew something about the details of getting the appropriation through. It would have been very difficult had we been handicapped by having a renowned scientist as a member who had something of his own to sell. As you know, the Committee has done extremely well. The fact that its membership and that of the American Roentgen Ray Society are interlocking is a definite advantage. I do not believe it should be changed.

I think there is no objection to allowing physicists to become members of the College if they desire, although I can see very little advantage to the physicists in belonging to such a nonscientific organization.

I think it is proper that the American Board of Radiologists should qualify x-ray physicists as such. There are now enough physicists on the Board or associated with it to handle this problem properly. It could certainly not be done by medical radiologists.

A.W.E.

In September 1946, the Standardization Committee of the RSNA met during the annual meeting of the Roentgen Ray Society. Following detailed discussions of the various questions brought up in the past correspondence, the committee agreed that it should continue until such time as the College properly took over the function. Several suggestions were made by the committee. The first was that the present committee make-up, which had functioned satisfactorily for 18 years with only minor changes in membership, be carried over to the College of Radiology. Because the membership of the committee, including the chairmanship, had for the past 12 years or so, overlapped almost completely the corresponding committee of the Roentgen Ray Society, the findings of one committee held for the other. This had been a very workable and successful arrangement, and one for which the societies clearly understood that committee-membership should not rotate too freely. It was strongly felt that the College of Radiology should keep the Standardization Committee as constant as possible over long periods.

The name and expanded functions of the new committee were important considerations. Since it would be dealing with problems in the field of x rays, radioactivity, and radioactive isotopes, the following name was suggested: "Committee on Radiological Units, Standards, and Protection." The single U.S. committee was thus in line with two corresponding committees of the International Congress of Radiology.

As to the procedure for handling future committee reports to the American College of Radiology, it was suggested that such reports be reviewed simultaneously by the two societies for publication in their journals. This would be in line with the current practice of the Standardization Committee, whose reports were normally adopted after open hearings and discussions. There had been no occasion where a society had rejected a report.

It was also suggested that committee meetings be held during meetings of the national societies. Physicists who attended these meetings were normally regular attendants of the radiological society meetings, but they could rarely afford to attend the meetings of the AMA or ACR, as most physicists were in a lower income bracket than radiologists. If it were imperative that members of the committee attend some irregular meetings, the question was raised whether the College would pay their expenses. These suggestions, it was emphasized, were merely those raised during the discussions and any final decisions for a workable arrangement would naturally be left to the College.

In responding to the Standardization Committee's communication, Cahal expressed delight at the suggested name which was adopted soon thereafter. However, a matter of some concern to physicists and earlier committee members was the statement concerning review procedures of the new committee's work. The College felt that, since the committee would be an agency of the College, the Board of Chancellors and the College membership would naturally have the authority to review, amend, or suppress any report of its committee, and no other organization could have such authority in this regard. In this respect, however, Cahal did point out that the Board of Chancellors included official representatives from the several radiological organizations, and thus could be considered as representing the entire profession.

The College, having adopted the amendment to its by-laws to allow Associate Fellowships in the College (see p. 263), asked Taylor for a confidential listing of physicists who he believed would be qualified for Associate Fellowships (10-10-46). Working from the registry list of March 6, 1942, Taylor suggested 17 of the more senior and experienced physicists

working in the field. All 17 candidates were submitted to the Board of Chancellors through Dr. H. Dabney Kerr, and they became the initial physicists elected to the College. Following is the list:

REGISTRY OF X-RAY PHYSICISTS FOR ACR MEMBERS

Aebersold, P.C., University of California, Berkeley, Cal.
Braestrup, C.B., Department of Hospitals, New York City
Corrigan, K.E., Harper Hospital, Detroit, Michigan
Failla, G., Memorial Hospital, New York City
Glasser, Otto, Cleveland Clinic, Cleveland, Ohio
Henny, G., Temple University Hospital, Philadelphia, Pa.
Laurence, G.C., National Research Council of Canada, Ottawa, Canada
Marinelli, L.D., Memorial Hospital, New York City
Nurnberger, Carl E., Redmon, Ill.
Quimby, E., Memorial Hospital, New York City
Reinhard, M.D., State Institute for Study of Malignant Disease, Buffalo, N.Y.
Rose, J.E., Marine Hospital, Baltimore, Md.
Singer, G., National Bureau of Standards, Washington, D.C.
Stenstrom, W., University of Minnesota, Minneapolis, Minn.
Taylor, L.S., National Bureau of Standards, Washington, D.C.
Weatherwax, J.L., Philadelphia, Pa. Philadelphia General Hospital
Williams, M., Mayo Clinic, Rochester, Minn.

Soon afterwards the names of R.E. Zirkle of the University of Chicago and H.O. Wyckoff of NBS were added.

In spite of the general confusion and uncertainty, plans continued on the transfer of the Registry of Physicists to the American Board of Radiology. This remained, however, a continuing subject of discussions by the Standardization Committee of the RSNA. Though the committee had long since accepted the transfer, it noted that the mechanics were in the hands of the Board of Radiology, all of whom were radiologists. The committee felt that it was important to keep in touch with the Board by memorandum since face to face meetings were impractical. One such memorandum was that of October 17, 1946, which follows:

To The Members of The American Board of Radiology
October 17, 1946

As you know, there has been some discussion regarding the possibility of bringing the operations of the Registry of X-ray Physicists under the American Board of Radiology. This was brought up at the last meeting of the Standardization Committee of the Radiological Society of North America, the members of which are in general agreement with this proposal.

It is recognized, however, from informal discussion with several members of the Board of Radiology that there may be some difficulties in the Board taking over this activity under its present constitutional setup. As we understand it, the present Board includes only radiologists who are also members of one of the national societies. This provision makes it impossible for a physicist to be on the Board of Radiology at the present time. Our committee, however, feels strongly that if the Board of Radiology were to take over the functions of the Registry of X-ray Physicists, it would be highly desirable that a physicist be on the Board at a policy-making level. The reason for this is just as obvious as the necessity for having a radiologist to determine the policies dealing with the qualifications of other radiologists. A possible solution to this difficulty is outlined below and while it is agreed by the committee that this might do as a makeshift, at the same time it recognizes that the more desirable solution would be to have a physicist actually on the Board.

The following tentative suggestions were put forward by the examining board for the Registry of X-ray Physicists at its last meeting:

(1) The Board of Radiology might appoint a committee of four to be made up of three physicists and one radiologist and to be under the chairmanship of one of the physicists. The tie between this committee and the Board of Radiology might be through the radiologist member who would at the same time be a member of the Board of Radiology. If such a committee is set up, it is suggested that the physicist members at the start be made up of those who have served on the examining board since its inception some ten years ago.

(2) Applicants for the Registry of X-ray Physicists could be examined at the informal Board meetings by whatever physicists were carrying out the normal examinations for the radiologists. Thus far, all of the physicists who have carried out this work

would be considered as accredited for the purpose of examining physicists. The findings of the examiners would be submitted to the committee of four mentioned above, who would pass on the suitability of the applicants. Their recommendations would then be passed on to the Board of Radiology for its approval.

(3) It was recommended, because the bulk of the physicists likely to apply for the registry fall into a limited income group, that no fees be charged for this examination. According to past experience the number of applicants would probably not exceed more than two or three a year.

(4) An alternate suggestion regarding the examination of candidates would be to set aside a day during the society meetings of one of the radiological societies at which time the committee of four mentioned above could examine the candidates and submit their findings to the Board of Radiology.

During the discussions of this problem numerous questions arose regarding the modus operandi of the Board of Radiology and we felt somewhat incompetent to make suitable recommendations not knowing more about the Board. It was suggested therefore that in order to investigate these questions further the Board of Radiology select a group of 4 including a member of the Board of Radiology who can meet together and discuss the problem and possibly arrive at a plan of operation. It would seem to me that if the Board would care to do so, this might be done at once. It is not necessary that this group be made up of the present examining board for the Registry of X-ray Physicists although I can see no objection to doing so. For your information the present board consists of G. Failla, G. C. Laurence, Otto Glasser, R. R. Newell (G. Failla has asked to be relieved of these duties.) and L. S. Taylor and U. V. Portmann acting on the the Board ex officio as chairmen of the Committee on Standardization of X-ray Measurements. All of the records and procedures of the Board of Registry have been handled in the office of L. S. Taylor and are there now.

This communication to the members of the Board of Radiology is being made upon the suggestion of Dr. Pierson with whom the matter has been discussed informally. It was his opinion that it would be desirable to express our views to the individual members of the Board of Radiology prior to their meeting which I understand is to take place at some time in November.

If, in the meantime, you have any suggestions to offer or questions to raise, I would be glad to try to take care of them myself or if necessary circulate them to our committee in order to obtain an expression of their views.

L.S.T.

In response to a letter from Dr. Newell, who was to be the chairman of the College "Committee on Radiological Units, Standards and Protection" (CRUSP), Taylor submitted a list of proposed radiologists--Newell, Portmann, Taft, and Ernst; and physicists Failla, Glasser, Laurence, Taylor, and Quimby. Taylor pointed out that the list was small and omitted some well known radiologists and physicists because of their poor track record in attending past meetings and answering correspondence. The above listing made up the membership at the first CRUSP meeting, at which a pattern was established whereby interested non-members could be brought in for specific meetings and discussions.

Newell was very much concerned about Taylor's feelings on these matters. In a letter of February 12, 1947, he tried to explain the situation, pointing out that he and several others had recommended that Taylor chair CRUSP. (Taylor would have rejected this position, even had it been possible; the College by-laws, at that time, strictly forbade other than full members from holding any office.)

In Taylor's final response to Newell, he said,

"I think operations of the Committee under your Chairmanship will be highly successful and you can count on my fullest cooperation in every respect. As a matter of fact, I am secretly a bit glad not to have the responsibility since our workload here at the present time is already pretty severe.

"The whole idea does raise in my mind some points of perhaps academic interest. As you know, I have never been overly enthusiastic about the American College of Radiology taking over some of the functions of the radiological societies and bringing certain physicists into its fold for that purpose. I have never been able to visualize the gain in such a step but am perfectly willing to see it tried out. I think one of the very nice things about the American Roentgen Ray Society and the Radiological Society of North America was the way that they took physicists into their fold and really worked under what was, for all practical purposes, a common footing. Of course, no physicist

could ever hold office or full membership in the Societies but on the other hand surely none ever wanted to. They could and often did head up important committees. I am very much afraid that many of the people will regard the College of Radiology as a bit of unnecessary fiction and have little or no interest in its organization or operation."

L.S.T.

On July 14, 1947, Taylor received formal notice of his appointment to serve on CRUSP, with which he had already been meeting. It pointed out that the commission (somewhere it had graduated from a committee to a commission) was presently without a chairman since Dr. Newell had been compelled to resign for reasons of health. Nevertheless, a letter was received from Dr. Newell on October 22 concerning plans for a meeting to be held during the annual RSNA meetings. It was his specific suggestion to hold a 4-hour session "about the roentgen," for which he and Failla had prepared a list of some 30 attendees.

The purpose of this discussion was not clear at the time, but it did contribute to the preparations for further discussions coming up several years ahead in connection with the re-establishment of the International Commission on Radiological Units. In his letter he wrote,

"Will you and Dr. Fano introduce the subject of the roentgen for us. You could refresh our memories as to the precise nature of the unit, its shortcomings as you see them, the difficulty of its 'realization' in the multimegavolt ranges, and its applicability to beta, alpha, proton, and neutron irradiation. Then you could give us your advice, or your present state of thinking, in regard to possible solutions: (a) keeping the roentgen unchanged in nature but devising ways of measuring its acceptability? (b) set a unit for ionizing particles other than photons? (c) alter the definition or devise a new unit covering the whole field of ionizing radiations."

Enclosed with his letter was a draft invitation which he proposed to send to his list of participants. The list included Dr. L. F. Curtiss, Head of the NBS Radioactivity Section, Dr. Robley D. Evans of MIT, and Dr. E. U. Condon, NBS Director, presumably because they had proposed the introduction of a new term called the "rutherford" to replace, or to be used in addition to, the "curie" as a unit of measurement for radioactivity (Condon, 1946). (This proposal was subsequently put before the International Council of Scientific Unions in the summer of 1950. However, it was thoroughly rejected by the ICSU and bitterly opposed by the Curie family.)

On October 29, 1947, the College sent a special bulletin to the Commission on Radiological Units, Standards, and Protection, giving notice of a meeting to be held in Boston during the RSNA meeting. The bulletin noted a change in the personnel of the commission by the chairman of the commission and the chairman of the Board of Chancellors. The new commission consisted of R. R. Newell, M.D., Chairman; Robert S. Taft, M.D.; George C. Henny, M.D.; Hymer L. Friedell, M.D.; Simeon T. Cantril, M.D.; and Lauriston S. Taylor, Secretary and the only nonmedical person on the commission. The bulletin was also Taylor's first notice that he was the secretary.

In his letter of November 10 to prospective participants in the CRUSP meeting, Newell stated that he would send abstracts of the leading presentations if available prior to the meeting of November 30. In the meantime, he suggested that the participants read the following:

1. Report by National Research Council in the October Issue of Nucleonics, page 32.
2. Taylor's "Early History of X-Ray Dosimetry."
3. His own essay on the "Language of Engineers and Radiologists."

Items 2 and 3 are included in Chapter 6 on quantities and units, together with the additional material promised by Newell in his November 10 letter (see pp. 72, 74).

At this point, we return to the period of 1946-47 to pick up the last threads of the RSNA Standardization Committee. This was a less-than-normal period with rather thin record files. However, some material is covered in the section on CRUSP.

In response to Taylor's notice of October, 1945 that he was back in circulation (see p. 261), Dr. Childs, Secretary of the Radiological Society, notified him of an up-coming business meeting of the RSNA in Chicago on November 9 and 10. He also pointed out that while Taylor was out of the Country, Dr. Kenneth E. Corrigan and Dr. Portmann had been acting as co-chairmen of the Standardization Committee. But he did not mention the organization changes and shifts to the ACR during the preceding 2 or 3 years.

Following the November meeting of their Board of Directors, the RSNA re-established the Standardization Committee (or the Physics Committee?) as follows: Lauriston S. Taylor and Ursus V. Portmann, Co-Chairmen; physicians Edwin C. Ernst, W. Edward Chamberlain, Robert R. Newell, Robert B. Taft, Bernard P. Widmann, and Frederick O. Coe; and physicists Otto Glasser, Edith H. Quimby, W. Stenstrom, J. L. Weatherwax, George C. Laurence, George C. Henny, and Kenneth E. Corrigan.

Because Corrigan had held the committee together during the war years, he was asked to prepare a statement along with recommendations that should be passed on to the new committee. In October 1946, Corrigan submitted the following minutes to the members of the new Standardization Committee:

To all members of the Standardization Committee of
the Radiological Society:
October 1946

There are certain matters to which the members of this Committee are asked to give some attention at this time.

The first of these is the Annual Report, which must be written this week. Any member having material to be included in the report should send it to me immediately.

Second in sequence but not in importance is the annual meeting which will be held at the Palmer House on Sunday, September 23. Exact time and place later, but it will be necessary to hold the meeting on Sunday in order to bring its report before the executive meeting on Monday night. This appearance before the executive council has been requested by the President.

Since this is a combined meeting of the two Societies, and, since the work being done by the Standardization Committee of the Roentgen Society under the chairmanship of Dr. Taft closely parallels the work of this group, an invitation has been extended to Dr. Taft to hold a combined meeting of the committees.

The work already projected to come before this meeting consists of several very important subjects:

1. A revision and restatement of certain protection standards and standard practices: - Such as the use of "7 foot lead walls."
2. A re-consideration of the concept of a "tolerance dose": -
3. A system for inspection of X-ray and radium installations: - Should the members of these committees, and of the Societies at large, go farther than they have in offering their services to physicians and to industry? Certain activities of the Public Health Service, of local Boards of Health and of labor unions force this question upon us. There is much to be said pro and con. Please give it your consideration.
4. An educational publication: - Has been suggested several times. A concrete proposal of considerable importance has been put forward by Dr. Taft. Should the combined committees or a subcommittee issue a pamphlet or a general publication in addition to publication in the X-ray literature?

Your chairman is very well aware of the fact that many of you have done a lot of work on these problems and that several members have excellent publications on some of these points and others of equal importance which need not be detailed here but should be brought before the meeting. The fact remains that this information has not been sufficiently widespread nor its message adequately understood.

Please express your opinion as to these proposals and as to others which you may have in mind.

Don't forget to forward anything you would like to have included in the annual report - at once.

K.E.C.

An informal meeting of the RSNA Standardization Committee took place during the regular meeting of the American Roentgen Ray Society in September. It was called primarily for reorganization purposes, but the committee did lay out some program objectives. These were contained in a brief report at the RSNA business session in December 1946 as follows:

REPORT
RSNA Standardization Committee (9-18-46)

1. An informal meeting of the Standardization Committee was held on September 18. A report from the examining board of the Registry of X-ray Physicists indicated that several new applications were under consideration and that steps were being taken to bring the individuals up for examination. One individual was examined during the meetings and was certified by the Board.

It was agreed that steps would be taken to permit the inclusion in the Registry of Physicists those employed by manufacturing concerns. This was in order to provide a partial solution for the difficulty in having adequate calibration services available in certain sections of the country. It was felt that if this should be done, the standards for certification should be tightened considerably.

Discussions regarding the turning over of the Board of Registry of X-ray Physicists to the Board of Radiology were held and some suggestions to be submitted to the Board of Radiology were prepared by the Committee. This question, together with operation of the Standardization Committees under the College of Radiology, appears to have certain difficulties and since it seems to be a foregone conclusion that these changes will be made, the Committee has been endeavoring to formulate and express its own ideas on the subject.

The Committee program as outlined for the coming year or two will include the following questions:

(1) An examination of the definition of the roentgen as applying to radiations in the megavolt range and taking into consideration pair production.
(2) Consideration will be given to the problem of re-definition of the curie. The recent definition proposed by a committee of the National Research Council has met with almost universal opposition. A resolution was passed concerning the use of the new unit known as the rutherford.
(3) A study of thimble chambers for use in the voltage range from 400 kV upwards and particularly in the pair production region is essential.
(4) The problem of standardization of radioactive isotopes requires considerable study in order to insure proper protection for the patients and the workers.
(5) The problem of the measurement of high intensity radiations appears to be questionable at the present time. Further study of this question is recommended.
(6) It was proposed that a subcommittee be formed to prepare a technical bulletin No. 2 dealing with the general subject of calibration of medical equipment in the hospital.

L.S.T.

Note that most of the items were different from those suggested by the interim committee. This was not a rejection of the interim committee's proposals, but a recognition of new problems that would be facing the radiological profession.

On December 17, 1946, Dr. Childs notified the Standardization Committee of membership appointments. These were the same as before except for the addition of Dr. Robert S. Stone, a radiologist from San Francisco.

On September 3, 1947, Taylor announced a proposed meeting of the RSNA Standardization Committee during the ARRS meetings in September. The notice (General Communication No. 24) outlined a number of questions recommended for consideration as follows:

September 3, 1947
TO THE MEMBERS OF THE STANDARDIZATION COMMITTEE OF
THE RADIOLOGICAL SOCIETY OF NORTH AMERICA
General Communication No. 24

It is planned to hold a meeting of the Standardization Committee of the Radiological Society of North America at Haddon Hall during the week of September 16, probably on the morning of Friday the 19th. This meeting will be preliminary to the annual meeting which will be held as normally at the December meeting.

There are numerous questions which should be considered by the committee. The following is a list of suggested topics for discussion with the idea that further investigations into the subject may be made by members of the committee prior to the December meeting.

1. The General Problem of X-ray Measurements in the High Energy Region.
   a. Discussion of the Report by the Bureau of Standards on the "Problem of Radiation Dosimetry in the Multi-million-Volt Energy Range"
   b. The Measurement of Surface, Depth, and Exit Dosage in the Region from 1 to 50 MV
   c. The Problem of Selective Absorption and Scattering of High Energy Radiation by Bone
2. Discussion of the Integrated Body Dose as Proposed by Several English Scientists as well as Others.
3. A Discussion of the Relative Merits of Geiger Counters and Ionization Chambers for Radiation Measurements, particularly for Radiation Survey Purposes.
4. The Problems Regarding the Use of Very Intense Soft X Rays in Experimental Work, Including the Techniques Involved in the Use of Body Cavity Tubes.
5. Discussion of the Results of the Questionaire which was Sent Out to all Registered Physicists Last Spring.
6. Plans for the Presentation of Recommendations to the International Congress of Radiology at its Next Meeting.

There are no doubt additional questions which you may care to bring up. If you can send these to me in advance of the meeting, I will try to prepare an agenda for advance distribution.

L.S.T.

Following its meeting of September 18, 1947, the RSNA Standardization Committee circulated the following minutes on the 29th. These dealt with a number of subjects, but particularly with the transfer of the Registry of Physicists to the Board of Radiology and the matter of qualities and units. (These are also covered in chapters under those names.)

Report of Standardization Committee
Radiological Society of North America
September 29, 1947

The Standardization Committee of this society has been taking steps to turn over to the American Board of Radiology all activities in connection with the registry of x-ray physicists. The Board of Radiology has agreed to undertake this work and it is believed that steps have been taken which will make it possible for the Board to take over in early 1948. In the meantime a survey has been made regarding the activities and interests of the x-ray physicists registered up to 1946. It is planned to publish the results of this survey together with an account of the general activities of such physicists as our closing out action on this phase of our Committee work.

Among our present registered physicists it was found that a considerable number are engaged in work which is only of a research or consulting activity and are not in a position to perform calibration services for the radiologists. Of the 24 physicists who are in a position to undertake radiation calibration and survey work, less than half are able to devote as much as 50% of their time to this work--there are only 8 who devote 100% of their time to these activities. Judging by the large number of requests that have been received asking for advice on this subject, there is demand for much more service of this sort than can presently be supplied. It might be pointed out that both the Army and Veterans Administration require calibration and inspection services by registered physicists.

The geographical distribution shows that the coverage over the country as a whole is spotty. The northeast section centered around New York and Pennsylvania appears to be heavily covered and yet it is believed that the available services in these areas are not in excess of the demand. The whole south and southwest is essentially uncovered except as one or two individuals may be in a position to make prolonged 1000-mile tours a few times per year. (We have had a large number of calls from Army and Veterans Administration requesting services south of Ohio.) Of the west coast only California can be said to be covered, although there is one individual who will make occasional long trips into bordering states. States like Washington, Oregon and Idaho have no coverage. The coverage in Maine, New Hampshire, Vermont and Massachusetts appears to be too thin. This applies equally to all of the states south of Virginia and in the central west. It is recommended that the Board encourage people to provide consulting and calibration services in these areas even though it may not justify a man spending a 100% of his time in the area. The availability of part-time people would be of great benefit.

Sample copies of routine calibration reports were submitted by all people in group 2. These appear reasonably consistent and most of them gave all of the information which would be of value to the radiologist. Occasionally the physicists have supplied other data in their report which may be considered as unnecessary but which is no doubt justified in many cases by the specific wishes of the radiologists. These data are usually computed from the direct calibration data, mainly, for ready reference by the radiologists.

At the last meeting of the Radiological Society there was an informal gathering of about a dozen radiological physicists who happened to be in attendance. A general conference was held regarding the problems which they have encountered in their work. A great many worthwhile and interesting ideas were brought out and everybody seemed to profit by the opportunity to discuss his problems with his co-workers. The questionnaire indicated unanimous interest in holding future gatherings in conjunction with annual meetings of radiological societies. It is recommended therefore that provision be made for holding such a meeting of the societies at some specified time and place. It is further suggested that we use one of the society classrooms at such a time as it may not be otherwise used. Arrangements to do this can probably be made in advance with the secretary of the society and a notice to this effect printed in the announcement of the meetings.

As it is now, it is believed that many of the younger or lesser known physicists have tended to stay away from the society meetings feeling that they did not have sufficient interest in common with the other people attending. This is certainly not the case. In the first place, there are usually a number of radiological physicists, particularly from the older group, in attendance. Secondly, it is believed to be highly desirable for the radiological physicists to mingle with the radiologists to discuss their common problems, to make themselves known, and to give and receive such beneficial advice and information as they may be able to.

At the last meeting of the Committee it was agreed unanimously that the use of the recently proposed unit of radiation called the rutherford not be employed for radiological purposes. The various members of the Committee expressed considerable concern over the fact that such a unit has been promoted by the Bureau of Standards' Radioactivity Section without any regard to the opinions and experience of the radiological organizations either of a national or international character. The following resolution was adopted by the Committee:

"The Standardization Committee of the Radiological Society of North America is of the opinion that the introduction of new units for the measurement of radioactivity at this time with the quasi-official backing of the National Bureau of Standards has tended to confuse the radiologists who have been using the curie in their work. It is suggested that some action be taken to remove the uncertainty in the minds of the radiologists as to what units are officially in effect. The Committee feels further that with suitable change in the definition of the curie, this unit will be very satisfactory for the radiological applications of radioactive isotopes and that no new units are necessary".

The Committee reluctantly recognizes that with the advent of radiations having energies about 1 or 2 million volts, the whole problem of radiation measurements and units is again open to change. Above a million volts, the pair production process probably rules out the use of our present methods of ionization measurement. The effect starts in at 1.1 MeV but does not assume serious proportion until about 2 MeV. In the meantime, preliminary considerations are being given to the definition of a new unit of dosage. While this may be called something different from the roentgen, it will not invalidate or change the use of the roentgen in our present energy regions.

The files do not appear to contain a formal report of the Standardization Committee meeting held on December 2, 1947 in Boston. However, what appears to be Taylor's longhand draft of the minutes is available and follows:

REPORT OF THE STANDARDIZATION COMMITTEE OF THE RSNA
December 2, 1947

A special meeting of this committee was called for the primary purpose of discussing its continuity of view of the formation of the Committee (later Commission) on Units, Standards and Protection by the American College of Radiology. It was suggested by the Secretary of the RSNA that the Committee discuss the problem and make whatever recommendations it desired. This was naturally a slightly embarrassing situation since the Committee was placed in the position of deciding its own perpetuation. Four of the

six members of the College were present. The problem was discussed by all persons present, and no serious disagreement on the questions below was evidenced.

It was unanimously recommended that the Committee be continued indefinitely. In this connection it was brought out that the Units Commission of the College (R. R. Newell, M.D., Chairman) should be properly considered as the "action body" for matters relating to Units, Standards and Protection, etc. The Committee of the RSNA should be regarded as having a primary role of education, exploration and discussion of problems in radiological physics. This was left as open and broad as possible.

It was felt that the twenty-year history of continuity, common interest and stable membership was an important bond between the radiological physicists and the radiological societies and should not be broken.

It was agreed that the name of the committee should be changed from the present Standardization Committee to "Committee on Physics".

It was agreed that there was a danger of this Committee becoming in-grown. To this end, it is recommended that the President of the RSNA consider the appointment of some new and preferably younger radiologists and physicists to the Committee. The radiologists should be chosen for their potential as a high grade radiologist together with an interest and broad understanding of the problems of radiological physics. Membership on the Committee will then serve to broaden his contacts and experience. At the same time it would provide a pool of physics-minded radiologists to serve on the units commission of the ACR. It is recommended that the present committee not be enlarged beyond its present number of 16. The present committee will be glad to assist the President in locating and choosing these new members and relieving those present members for whom the committee burden is too great.

The Committee respectfully requires the Board of Directors of the Radiological Society of North America to approve the following recommendations:

1. Continue the existence of the Committee indefinitely.
2. Rename the Committee, "The Committee on Physics".
3. Alter the present composition of the Committee by the gradual appointment of some young, promising, and ambitious radiologists having some strong interest in physics.
4. Appoint to the committee, some of the young and ambitious young physicists having some training and strong interests in biology and radiology.
5. Retain the present committee size.

L.S.T.

That appears to be the end of the draft report. However, there were notes on a number of comments during the second day of the meetings. These reflected the various questions that were raised regarding the manner in which CRUSP would operate. Some participants thought that the committee should be kept small and consultants called in as needed. Taft, on the other hand, felt that CRUSP should be substantially larger. Concern was also expressed on the continuity of CRUSP activities and the likelihood of rapid turnover in membership.

The notes also included an observation that the goals and ideals of the College were shifting from primarily business and lobbying intents to scientifically oriented concerns. There was a general feeling that the radiological societies and their committees should be largely educational and should avoid any political activity. It was pointed out that RSNA, in particular, had been a leader in radiological science for many years, but was now "elderly" and could be thinking of protecting its turf. The Standardization Committee, it was generally felt, should keep its identity, at least until CRUSP had fully established its program and given some indication that it could properly handle the problems previously dealt with by the Standardization Committee.

Newell, Chairman of CRUSP, made the motion that the committee be continued. This was passed and the earlier suggestion to rename the committee "Committee on Physics" was adopted. Following Laurence's comments that the present committee had scientific functions and missions that would fall under such a title as "Committee on Physics," Newell moved that the composition of the committee should include, in addition to a few older people, a younger group who were working on important problems, as well as ambitious radiologists interested in physics. As events developed, a few new people were brought in, but most of those people suggested to the committee were passed over.

At its next business meeting (1948), the Radiological Society of North America decided to establish a Committee on Physics to replace the Standardization Committee, which it had agreed to turn over to the American College of Radiology. Taylor was designated as the committee's initial chairman, a position which he held for the next 5 years. He was

succeeded by Dr. Harold Wyckoff who held the chairmanship for a number of years. In one capacity or another, both Wyckoff and Taylor worked closely together with the Committee on Physics, and with the Commission on Radiological Units, Standards, and Protection.

At the outset, the Commission on Radiological Units, Standards, and Protection attempted to take a leading role in the physical aspects of medical radiology. It acted as the coordinating body on questions of radiation quantities and units preparatory to the reactivation of the ICRU in 1950. It was during this period that H. M. Parker of the General Electric Co., Richland, Washington, was requested to prepare a definitive discussion of the philosophy and background of the development of radiation dose units. This report was not completed until 1955, by which time CRUSP interests had shifted away from detailed consideration of physical matters. Nevertheless, the report presented an exceptionally clear discussion of the subject and, since it has never been published, it is included here by permission of the author.

SOME BACKGROUND INFORMATION ON THE DEVELOPMENT OF DOSE UNITS

By H. M. Parker

(Report submitted to
Commission on Radiologic Units, Standards,
and Protection, November 1955)

C O N T E N T S

PART I

INTRODUCTION

At the request of the Commission on Units, Standards and Protection of the American College of Radiology, the writer has attempted to sketch some parts of the historical background of dosimetry.

The sketch has two objectives:

1. To emphasize areas of past or present possible misinterpretation.
2. More constructively, to suggest future modus operandi that would be widely acceptable and eliminate misinterpretation.

Radiation dosimetry appears to be one of the few fields of scientific endeavor whose basic terminology has never been satisfactorily established.

The term "dosimetry" is readily understandable as the measurement of dose or the science of dose measurement. It is the simple term "dose" that lacks appropriate definition. "Dose" is derived from the Greek word "dosis," meaning "a giving." The most plausible interpretation of "dose" in its present application by radiobiologists, biophysicists and the like is "a receiving." It is not surprising that conflicting interpretations should have developed in a process that has translated "a giving" into "a receiving."

The writer has attempted to review this field through a library with quite limited resources, especially in journals other than in English. This accounts, in part, for a lack of national balance in verbatim quotations.

The relevant dictionary meaning of "dose" is "a measured quantity of a medicine to be taken at one time or in a given period."

The application of the expression "dose" to an X-ray treatment must have come quite naturally to the physicians who become the first radiologists. The primary interest at that time was to establish any defining measurable quantity that would promote reproducibility of treatment. At this stage, at least, quantity of X-radiation clearly meant amount, and not the physicists' formal quantity of radiation

The early X-ray tubes were notoriously fickle, and one can well understand that their users would be satisfied with any measuring device that would measure the "output."

Perhaps this contributed to the focussing of interest on the beam intensity, with secondary interest in what took place in the irradiated tissue.

We have pointed out before that units and instrumentation are inextricably connected [1].* Whenever a new physical agent is discovered, a measuring system is established in

*Numbers in brackets refer to references at the end of this report.

terms of some conveniently observable property of the agent, and from this develops a unit. For different applications, the property chosen may also differ. Thus in the familiar case of electric current, four units are commonly used:

1. The electrostatic unit of current, based on rate of transfer of electric charge.
2. The electromagnetic unit of current, based on the magnetic effect.
3. The ampere, a practical unit, one-tenth of the e.m.u.
4. The international ampere, based on the electrochemical effect, and differing slightly from the ampere.

It is not surprising that various systems of units developed for medical radiological applications. Also, a priori, it is not essential that only one system should remain in general use. One function of this report, after reviewing developments to the present time, will be to make recommendations on how to optimize terminology and the number of systems of units.

EARLY SYSTEMS OF MEASUREMENT AND DOSE TERMINOLOGY

In the early years of radiology, measuring methods were based on many effects of the newly discovered radiation. The calorimetric method dates back to 1897. Various chemical, fluorescence, or scintillation methods were used in the first years of this century; one of the favorite systems depended on the coloration of barium platinocyanide pastilles. The photographic method, based on one of the first observed effects, was made "reasonably quantitative" as early as 1905, although others will claim that it is still unreasonably qualitative in some applications in 1955. Changes in electrical resistivity and a variety of biological effects were also studied for dosimetry. The measurement of ionization in gases gained and maintained an early popularity. Apparently, the first formal statement of a unit based on ionization was due to Villard in 1908 [2]. Unit quantity of X-radiation was defined as that which by ionization liberates one electrostatic unit of electricity per cubic centimeter of air under normal conditions of temperature and pressure. This is the embryo of the roentgen that was not born until 20 years later.

In the fourth edition of Kaye's "X-rays" (1923) we find the following summary of methods of measuring intensity used in medicine [3]:

5 H* units	(Holzknecht; alkaline salt)
= Tint B	(Sabouraud - Noiré; pastille)
= Tint 1	(Bordier; varnished pastille)
= 3 to 4I	(Boudier and Galimard; iodine solution)
= 10 X units	(Kienbock; photographic plate)
= 3.5 Kaloms	(Schwarz; mercury solution)
= Villard dose**	

*Unit 1 H = One-third of the radiation necessary to set up the first signs of reaction in the healthy skin of the face.

**This term is not understood by the reviewer.

In 1928 we find the following comparison of units by Russ, Clark and Watters [4]:

Comparison between the various units
in Measuring the Quantity of X-rays

Sabouraud-Noiré Radiometer	Holzknecht Chromo-radiometer	Kienbock Quantitometer	Furstenau* Intensito-meter	Solomon's Ionto-Quantimeter
-	1 H	2 X	60 - 80	-
Tint B	5 H	10 X	-	1000 R
-	20 H	40 X	-	-

* Based on selenium resistivity.

To those of us who have emphasized the basic simplicity of the ionization-in-air methods of dosimetry, it is a shock to find only one ionization candidate in this 1928 comparison. This is tempered somewhat by the following paragraph:

> "Of late years an attempt has been made to establish a physical dose of X-rays, and although the means by which this dose is produced may vary, its meaning is clear. The dose is called an e dose, and when this dose of X-rays is expended in air, causing ions to be formed, they will allow the transference of one e.s.u. of electricity across 1 c.c. of this air. When an international unit of X-ray energy is decided upon it is probable that the e dose will form the basis upon which all comparisons will be made."

In the following year, we find Mayneord giving practically the general modern interpretation to dose [5]. He points out first that a quantitative system of measurement is not easy "particularly since normally the physicist and radiologist are interested in quite different quantities, though at the moment the radiologist appears (very properly!) to have carried the day." Mayneord goes on to define intensity of an X-ray beam as "the amount of energy carried in one second through 1 sq. cm., and this is the quantity (!) a physicist means when he speaks of 'intensity'." He points out that the radiologist is much more concerned with the amount actually absorbed in the tissues, and defines a second quantity (!), X-ray dose as "the total amount of X-rays absorbed in 1 c.c. of tissue or other absorbing substance." He makes the alternative statement "The dose then is the total quantity of energy absorbed per cubic centimeter" and adds that "the unit of dose is not, as it should be logically, ergs per c.c., but some amount defined in terms of the ionization the rays will produce in air because--absorption in air and in the tissues run roughly parallel."

We note the difficulty of writing such a passage without the semantic difficulty of using the term "quantity" in its general sense, and creating confusion with the "time-integrated flux density" meaning. With this exception, the passage defines a clear approach to a significant quantity (i.e., that character by virtue of which it can be determined as more or less than some other!) that can be called dose.

PREFERRED POSITION OF IONIZATION MEASUREMENTS IN AIR

The first stage of the attempts to standardize measurement of X-ray quantity moved steadily in the direction of an air-ionization technique, supported by the following factors:

1. The existence of a saturation potential in a gas-filled chamber provided a reproducible means of measuring "ions produced per unit volume" by means of accumulation of electric charge or by current measurement.
2. Either current or change of potential can be measured with precision over a wide range.
3. The relationship between radiation intensity and ionization current is linear over a wide range (for a beam of given quality).
4. Dry air is a universally available filling medium for ion chambers.
5. For those interested in effects in tissue, the atomic composition of tissue and air are such that the ionizing effects should be "roughly parallel" for the X-ray energies normally used in therapy in the period considered (the 1920's).

This led to the development of free-air chambers through the work of Friedrich and many others. We shall not document this well-known phase.

The development of instrumentation that give reproducible results and the formulation of a unit were closely related, and led to the definition of the roentgen at the Second International Congress of Radiology 1928.

THE FIRST FORMAL INTERNATIONAL UNIT

The specific recommendations were:

1. That an international unit of X-radiation be adopted.
2. That this international unit be the quantity of X-radiation which, when the secondary electrons are fully utilized and the wall effect of the chamber is avoided, produced in one cubic centimeter of atmospheric air at 0° C and 76 cm mercury pressure, such a degree of conductivity that one electrostatic unit of charge is measured at saturation current.
3. That the international unit of X-radiation be called the "Roentgen" and that it be designated by the letter small "r".

We must note that the original definition makes no reference whatsoever to dose, although almost immediately the roentgen was described as the unit of X-ray dose (e.g., reference 5).

In other translations, we find "international unit of X-ray quantity" in lieu of the expression given above. I do not have access to the original documents but I understand that this variation is the one intended.

What was done, in effect, was to acknowledge the basic physical definitions:

Quantity of radiation is the total radiant energy flowing through unit area of a surface normal to the direction of the radiant beam.

This is identical with time-integrated flux density.* The conventional unit is erg per cm^2.

*Ed. Note: In modern terminology (1980), this would be called time-integrated flux density or energy fluence.

Intensity of radiation is the radiation flux density or the quantity per unit time.

Then it was reasoned that formal quantity was not measurable in general, because it involves total absorption of the energy passing through a unit surface. The roentgen was thus a substitute unit of quantity. Critically examined, the procedure is inadmissible, because two situations characterized by the same quantity in roentgens do not represent the same quantity of radiation in the physical sense, if the relevant absorption coefficients differ. Failla [6] has discussed this matter elegantly in Duggar's text; he resorts to the expression "effective quantity," with its analog "effective intensity."

However, it is firmly maintained by most radiological physicists that the term "quantity" in the definition of the roentgen does mean quantity of radiation" in the conventional physics sense. Paradoxically, some of these same observers emphasize that intensity has a definite physical meaning and should not be expressed in roentgens per sec. To this observer, it appears that if quantity is expressible in roentgens, then intensity is equally well written in roentgens per second. Other reporters categorically deny that "quantity" in the definition has more than the common or household meaning of amount [7]. Lack of clarity on this point has confused the interpretation of dose. Morgan and Corrigan [8] have made a compromise proposal which reads

> "It appears that 'radiant exposure' is a more suitable term to express radiant energy per cm^2 and that the term 'quantity of radiation' should be used in the broader sense which it implies; namely as a quantitative expression of radiant energy in general (e.g., as in the definition of the roentgen)."

EARLY METHODS OF DOSE EXPRESSION FOR RADIUM TREATMENT

So far we have brought the review of X-radiation up to the formulation of the roentgen. It is convenient to make a brief accounting of early radium dosimetry at this point. Whereas the X-ray case was largely conditioned by the need to establish a reproducible method of measuring the "output" of a variable source, the radium case began with sources of fixed emission rate. It became common practice to express dose as the product of milligrams times hours of exposure (or treatment, to avoid ambiguity); obviously such factors as the

distance and disposition of the sources had to be stipulated to complete the therapy prescription. When radon was used instead of radium element, an average strength had to be used or a more elaborate calculation of millicurie-hours affected. The French school, under Prof. Regaud, estimated the degree of radiation as "l'emanation detruite" or millicuries destroyed. Since a 1 mc radon source left in situ permanently contributes 133.3 mc-hours (using the old value of decay rate), the practice developed of packaging radium element in amounts of 6.66 or 13.33 mg to make the arithmetic simple! That dose was expressed in milligram-hours or some similar term for so long was obviously not due to lack of comprehension of the problem, but rather to the then existing technical difficulties of going at it in other ways. As early as 1919, Regaud and Ferroux [9] pointed out the differences between dose emitted and dose received, but continued to use the former. Three dose expressions were commonly recognized in this period, namely:

Dose emitted - The energy released by the radiation source, or some function reasonably related to it.
Dose delivered - The energy arrived at the locus of biological interest, or some reasonably related function.
Dose absorbed - The energy imparted to the locus of interest, or some reasonably related function.

Failla [10] made an early review of the field in which he referred to the desirability of measuring the energy absorbed in tissue if this could be accomplished. An excellent review of the early work in dosage specification in radium therapy was given by Mrs. Quimby in the 1940 Janeway Lecture [11]. This paper can well be considered as part of this review. The brief condensation that follows is given without references, since these are well-covered in reference [11] and in C. W. Wilson's "Radium Therapy" [12].

Innumerable biological effects were considered as bases for dose, including that of Russ (1918), the amount of radiation necessary to kill mouse cancer, and called the "rad," a title that has now been revived for absorbed dose measurements. Perhaps the most widely used biological frame of reference was the erythema dose (E.D., H. E. D., or S. E. D.) especially in the threshold erythema dose of the Memorial Hospital School.

By taking advantage of the first order approximation that absorption and scattering in light elements for radium gamma-rays can be neglected, mathematical systems of evaluating "dose" for external or intracavitary applicators developed. These required only a knowledge of how to integrate radiation functions, based on the inverse square law, together with a reference dose-rate at 1 cm from a unit point source of radium. Sievert, Mayneord, Failla, and Quimby were among the early contributors to this technology. The best known system is that of Sievert, based on the Intensity-millicurie (Imc), a unit "intensity" defined as the intensity of gamma radiation at a distance of 1 cm from a 1 mg point source of radium filtered by 0.5 mm Pt. (This unit is entirely consistent with physical principles if one assumes that the energy spectrum of all sources is the same. This would be sufficiently close for radium sources, but would be unmanageable for general radioisotope work today). From the intensity unit is defined a unit of quantity, Imc-hour, usually referred to as the Sievert dose, and also frequently referred to as 1 cm. mg-el.-hr. The Sievert dose has a well-known relationship to Eve's number, from which the translation to "roentgens" can be made. Mayneord [13] derived such a value (8.7 r) from Reitz's ionization data. Paterson and Parker [14] based their system of dosage on L. H. Gray's ionization data, and have used the value 8.4 r, since 1932. It will be recalled that all such derivations, prior to 1937, were considered unethical because the definition of the roentgen (1928) limited its field to X-rays. We have never been sympathetic to this ethical matter, because we regard X-rays and gamma-rays as identical in nature, except for the trademark representing their origin. Today, when the energy span of producible X-rays wholly includes the narrow energy band of the natural or artificial gamma-ray emissions of nuclides, the identity of the topics is self-evident. However, throughout the history of dosimetry, the available energy span of therapy sources has had a profound and legitimate influence on the development of techniques and units.

TRANSITIONAL PERIOD BETWEEN THE ORIGINAL AND MODIFIED ROENTGEN DEFINITIONS

We are now in a position to consider the period between the second and fifth international congresses of radiology (1928-1937). Many significant events were contained in this period. Some of interest were:

(1) Reliable apparatus for roentgen measurements in the then available range of energy
The various national laboratories reached essential agreement on measuring conditions, largely through the efforts of Behnken and of L. S. Taylor. (References not given to this well-known phase.)
(2) Interest in roentgen measurements for higher energy radiation

This was stimulated in two ways, (a) by the steady increase in available X-ray excitation voltages, and (b) by the growing desire to include radium gamma-rays in the same scheme. Since the only realization of the roentgen was by means of the free-air chamber, numerous experimenters worked with larger and larger chambers. The evaluation of the Sievert dose in roentgens by this technology led to widely divergent results, which contributed to the reluctance to extend standard measurements to this field.

Sievert Dose in Roentgens - Free-air Chamber Methods
(For references see C. W. Wilson [12].)

Bruzau, 1929	6.4 r
Failla and Henshaw, 1931	2.0 r
Mayneord and Roberts, 1934	3.1 r
Kaye and Binks, 1936	7 - 8 r
Friedrich, 1938	7.8 r
Taylor and Singer, 1940	8.2 r

(3) Empirical thimble chamber dosimetry

Before the establishment of the roentgen, considerable work had been done on the development of thimble chambers of X-ray measurement, notably by Fricke and Glasser [15], and commercial dosimeters based on this were available as early as 1927. The definition of the roentgen placed all these devices in the permanent category of empirically founded secondary instruments, requiring calibration against the free-air chamber. After 1928, the extensive work on thimble chambers continued in respect to making an equivalent air-wall, choosing a satisfactory wall thickness, correcting for the absorption in that wall, and extending these propositions to include radium gamma radiation. It is sufficient to point out that the objective for wall composition is to use a material with the same effective atomic number Z_{eff} as air; Z_{eff} is variously quoted at values between 7.3 and 7.69, depending on the relative weight given to Compton and photoelectric absorption. The empiricism in thimble chamber construction offers no obstacle to their successful use, provided this is confined to the demonstrated range of calibration against primary standards. It is a dangerous yet common error to consider thimble chambers as Bragg-Gray cavities.

The lore of the thimble chamber still contains a wide selection of naive concepts, a typical example of which is the following, "A 1 cc chamber is to be preferred especially because the definition of the international roentgen specifies that the roentgen be measured per 0.001293 gm of air, which is 1 cc at 0° C and 760 mm pressure" which appears in a highly reputable text dated 1944. Those of us who have struggled with the difficulties of some radiologists in comprehending what the dose is in one-half a cubic centimeter when the uniform dose is 1 r in a given cubic centimeter of tissue would do well to make sure that the physical house is in order.

(4) Bragg-Gray Principle

The enunciation of Gray's Principle of Equivalence [16] and the fortunate circumstance for radiology that Dr. Gray was shortly thereafter attracted to a career in radiological physics profoundly influenced the course of dosimetry. We should recall that the principle was established by him in connection with studies of cosmic radiation, with simplifying assumptions that might not have appeared attractive if initiated for the then normal range of radiation therapy sources. The study was basically concerned with absorption coefficients and the validity of the Klein-Nishina formulae. Gray indicated that measurements of the emission of Ra (B + C) and particularly of ThC", with its strong 2.6 Mev emission, would be of particular interest. It was through this channel that his work came into the orbit of radiation therapy.

The principle was present again with supporting experimental evidence in 1936 [17], and the results of the radium measurements appeared in 1937 [18].* As is well known, the

*Ed. Note: As an aside, we may note that this sequence has a bearing on the recent editorial trend to decline references to unpublished work. Rutherford, Chadwick, and Ellis [19] gave Gray's results (unpublished) in 1930, which enabled Parker to arrive at the Sievert dose of 8.4 r in 1932. The actual data were not published until 1937.

principle expresses the energy absorption per unit volume of irradiated material in terms of the measured ionization per unit volume in a suitably specified small cavity within that material as:

$$E_v = \rho W \cdot J_v$$

where ρ is the ratio of electron stopping power in the material and the cavity gas respectively

and W is the average energy required to produce one ion pair in the cavity gas.

Since it had been pointed out that W. H. Bragg [20] had arrived at a similar proposition in 1912, the term Bragg-Gray Principle developed. As treated by Bragg, the method was applied only to obtaining relative ranges of secondary electrons in various materials; no reference was made to the possibility of using the method for the absolute measurement of gamma-ray energy, and the ionization vessels used by Bragg were not, in fact, legitimate Bragg-Gray chambers.

Gray has additionally pointed out that Fricke and Glasser [15] independently arrived at the same principle in 1925, and it is essentially contained in a doctorate thesis by M. Bruzau (Paris, Nov. 1928). Nevertheless, the practical establishment of "energy absorption dosimetry" is primarily due to Gray's influence. The definitive work was presented at the 1935 Annual Congress of the British Institute of Radiology [21]. These papers are perhaps the first in which the appropriate dimensions of Bragg-Gray cavities (assumed filled with air at atmospheric pressure) are stipulated. They show that the principle is a convenient one in the range of radium gamma-rays, but extremely taxing for X-rays generated at 200 KV or less. This accounts for many of the observed discrepancies in thimble chamber measurements, which led several authors in this period to the conclusion that there was no valid principle of small chamber dosimetry.

Although Gray's main interest was in the field of radium gamma-ray measurement, he made an attempt to integrate this with current X-ray dosimetry. To this end he proposed to redefine the roentgen as follows:

"The dose at any point is measured by the air-ionization equivalent of that part of the quantum energy which would be transformed into corpuscular energy by absorption in an infinitesimal volume of dry air at 0° C and 760 mm Hg pressure situated at that point, divided by the volume of the air, measured in cubic centimeters. If the amount of ionization is such that the total charge on the ions of one sign would be 1 e.s.u., then the dose is defined as 1 roentgen."

This is a definition of an air-filled Bragg-Gray cavity surrounded by an air wall. It would have permitted measurement with the standard free-air chamber in the appropriate energy range, and with thimble chambers of "air-equivalent" walls in the then practical range of interest. Such an approach is a compromise of technology, since Gray pointed out the advantages of using small chambers made of pure homogeneous materials; graphite would be the material of choice for two reasons:

(1) Its atomic number is not substantially different from that of either air or tissue.
(2) It is conducting, and a chamber needs no internal conducting layer.

Gray criticised the existing definition of the roentgen on two grounds:

(1) It does not state explicitly whether ionization due to scattered quantum radiation is to be included or excluded.
(2) It limits measurements to the free-air chamber which is progressively less satisfactory for higher and higher energy radiation.

He proposed reconsideration of what needed to be measured as follows:

(a) The intensity of the beam of radiation before it enters the body.
(b) The intensity of the X- or gamma radiation at any point in the body.
or (c) The actual absorption of (corpuscular) energy at any point in the body.

Alternative (c) is the most logical approach to the clinically significant dose. The frightening technological difficulties in (c) as applied to interface conditions (i.e., air to skin, keratin-bearing skin to soft tissue, and bone to soft tissue) were judged to contra-indicate course (c).

Effectively, the new proposal was equivalent to (b), an advance from course (a), which is basically that implied in the 1928 roentgen, but was not so used, especially in European practice.

This reviewer feels that the more radical course (c) could have been followed, since we have inevitably come to a need to do precisely this in the 1950's. If the amount of effort that has gone into the design and specification of air-equivalent chambers had been devoted to the problems of tissue-equivalence, radiation biophysics would have achieved a technology that would have contributed much more to an understanding of and practical application of radiation biological effect.

(5) Sundry proposals related to the 1928 roentgen

In addition to the possible ambiguity as to the meaning of quantity in the definition, other ambiguities were recognised from time to time. There even developed a United States roentgen [22] defined as:

"The roentgen is the quantity of X-radiation which, when the secondary electrons are fully utilized and the effects of all scattered radiation avoided, produces in 1 cc of atmospheric air at 0° C and 76 cm mercury pressure such a degree of conductivity that 1 e.s.u. of charge is measured under saturation conditions."

The first underlined change related to the uncertainty as to whether scattered (quantum) radiation was meant to be included. The second change, less consequential, replaced "at saturation current."

(6) Miscellaneous comments on dose units from the period 1928-1937 (approx.)

This section will include a number of statements by qualified radiologists or radiological physicists selected mainly from the period between the two international definitions of the roentgen. We have not attempted to build them into a sequential scheme.

(a) Murdoch, 1931 [23], says that "radium therapy lacks the indispensable basis of all scientific therapeutics, that is to say, an unquestionable unit of doseage."

He recommends a statement of dose actually absorbed by the tissues. This he evaluates from the familiar calorimetric and liquid ionometric measurements of Stahel [24]. Murdoch says, "In 1928, Stahel, owing to the impossibility of measuring directly in r units by means of his small ionization chamber, adopted the suggestion of Dauvillier (1923) and measured the quantities of energy absorbed by units of volume of irradiated tissue, the results being expressed in cal per cc or ergs per cc."

Comment

We see how strongly the roentgen had gripped the profession at this early date. The statement essentially says that Stahel measured what we would consider the quantity of chief significance because his methods would not fit roentgen dosimetry. Actually, Stahel's energy values were high by a factor of about 2.3, but this is no larger than the errors in Sievert dose determined by air-ionization at that time.

(b) G. C. Laurence, 1937 [25], speaks without apology of the "intensity in roentgens per second of X-rays of gamma-rays." He gives the following evaluations of the Sievert dose (a term not used by him, however):

Early Evaluations of the Sievert Dose by Thimble Chambers

For references see reference [25]

Authors	
Friedrich and Schulz	8.0 r
Murdock and Stahel	8.4 - 8.6 r
Mayneord and Roberts	8.5 r
Sievert	8.0 r
Grimmet and Read	8.7 r

Comment

Here an outstanding contributor commits the double ethical error of writing intensity in roentgens, and applying it to gamma-rays before the 1937 redefinition. Note also the comparative consistency of the thimble chamber readings.

(c) W. V. Mayneord, 1931 [26], refers to the ideal method of measuring dose as energy absorbed per cc at any point throughout the mass irradiated. He is driven to an ionization unit, the roentgen, on grounds of convenience, and he attempts to measure radium gamma rays in roentgens.

(d) G. C. Laurence, 1936 [27], writes "The word 'dose' is unfortunately ambiguous. In its most general sense it refers to a detailed description of the physical conditions of a

treatment. In the more restricted sense, it refers to the amount of radiation that traverses the unit region about any particular point, and it is measured in roentgens or similar units. To avoid confusion the term 'irradiation' or 'roentgenage' (ef. voltage, mileage, tonnage, etc.) would probably be more satisfactory."

(e) G. Failla, 1937 [28], discussing tissue dose for all ionizing radiations refers to the desirability of a unit of dose such that the same biological effect would be produced by a given dose, irrespective of quality or kind of radiation.

He writes $D_b = D\ q\ b$

where D_b = biological dose
D = dose or quantity factor (i.e., physical dose?)
q = quality factor (wavelength factor)
b = other factors, that can be held constant in a series of tests for q. (presumably fractionation, protraction and the like)

He proceeds to examine how D should be measured. This paper contains an interesting orientation of the extrapolation chamber to the Bragg-Gray Principle. Failla is led to a "tissue roentgen" defined as:

"The tissue roentgen is the quantity of any ionizing radiation capable of producing 1.62 x 10^{12} ion pairs per gram of air at a given point in a given medium under the conditions in which the radiation is to be utilized."

(f) Failla and Marinelli, 1937 [29], reach the conclusion "that the measurement of gamma-rays in roentgens would lead to serious complications of a practical as well as theoretical nature." They reiterate the value of an ionization dose--that is, a measurement of the ionization that radiation produces in air, under conditions obtaining in the irradiated tissues.

Comment

This was a period in which the European, and especially the British, approach to dosimetry deviated considerably from the American view. It was shown in the clinic by the American practice of measuring "dose in air" and then relating this to "tissue dose" by tables as compared with the British practice of measuring the surface dose in a phantom.

Perhaps it is fair to say that the American school took the roentgen definition literally, whereas the European school reasoned that an r-meter in a phantom measured something proportional to the quantity of radiological interest.

Looked at from the viewpoint of the Bragg-Gray Principle, either a small cavity chamber or an extrapolation chamber could yield an interpretable concept in a phantom. Also, to the British, the different reaction for gamma-rays and conventional X-rays for equal "roentgenage" was a matter of radiobiological interest. It seems to have been much more troublesome to the American school.

(g) H. Behnken, 1934 [30], proposed a change in the definition of the roentgen to "a dose of one roentgen corresponds to the absorption of 83 ergs in one gram of tissue."

(h) L. Grebe, 1934 [31], proposed the definition "The absolute unit of the roentgen ray dose is delivered by that amount of roentgen rays which produces in 1 cc of air irradiated homogeneously at 0° C and 760 mm Hg pressure, an ionization equivalent to 2.09 milliards of single charged pairs of ions."

(i) R. R. Newell, 1939 [32], proposed a battery of new X-ray terms based on a parallelism between radiology and illumination. Unfortunately, the parallel is not entirely a productive one, because the specific terms of illumination engineering and of the physical terminology applicable to radiation are not identical. Perhaps also the proposal of five new units was overwhelming. The paper is of considerable interest as representing a rational attempt to eliminate the loose and conflicting terminology of this period.

Relevant terms were:

Irradiation is the act of applying X-rays to any object; specifically, it is the roentgen flux density applied.

Primary irradiation excludes the roentgen flux secondarily scattered or re-radiated from the irradiated object.

Secondary irradiation at the surface is synonymous with back-scatter.

Exposure is the time integral of primary irradiation, measured in roentgens in the beam.

Rhegma is the proposed unit of tissue dose, 1.615×10^{12} pairs of ions per gram.

(j) Newell, 1933 [33], writes as follows:

"Roentgens: A roentgen beam is a stream of radiation. Its intensity is the energy it carries per square centimeter. . . . . The international unit (i.e., the roentgen-Ed), then, is a measure of the product of quantity and absorbability. . . . . It is not, strictly speaking, a unit of dose, for dose concerns effect, not on air but on flesh. The latter may parallel the former, but it is not identical with it. I am speaking of immediate effect, i.e. ionization.

"Dose: What we are interested in is the tissue dose and I think we would better always call it by its full name, 'tissue dose.'

". . . . it is important that we reserve the word 'roentgen' for amount of X-ray measured in air (in the beam), and so we must find a differently named unit for tissue dose."

Comment

Intensity appears to be defined incorrectly. The roentgen cannot properly be called a measure of the product of quantity and absorbability since it has been defined as a quantity of radiation (although we are inclined to agree that this approach makes more sense). To say that dose concerns effect on flesh is to deviate from the medical (or drug) concept of dose, a deviation that most of us accept, however. We would also quibble with the expression "roentgen beam," but the purpose is not to criticize an author who has elsewhere brought much logical thinking to the field. Rather we use the quotation as an illustration of the American position.

Twenty-one years later, the same author [34] wrote:

> "When you are measuring the output of your tube, a unit of exposure fits your needs perfectly. When your attention is on the patient and his skin and his tumor, then you think of the dose almost as if the tissue were soaking up dye and becoming redder the stronger the dye and the longer the application. A unit of exposure does not fit this concept well, but we have been using our roentgen under an unconscious perversion of its defined meaning so as to fit it to this practical need. In fact, we see in the official glossaries a tendency to redefine 'dose' so that the concept will fit the unit, since the unit does not fit the concept."

If we could substitute "conscious perversion" for Newell's "unconscious perversion," I suspect that we would have a fair statement of the British position since about 1930.

(k) Report of R.S.N.A. Committee on standardization of X-ray measurements, 1934 [35]

This report recommends the definition of the roentgen given already as the accepted U.S. definition in reference [22]. It gives a good account of the ambiguity relating to inclusion of scattered (quantum) radiation. It has forward-looking references to impending difficulties with the definition at voltages above 1.2 Mev, where pair production is possible.

It refers to an earlier recommendation of a unit of "effective X-ray intensity," which is considered preferable to a definition of quantity. It deprecates the use of the term "intensity" as applied to roentgens per unit time, and defines "irradiation" as the quantity per unit time.

It restricts all data given in roentgens to measurements of incident radiation, excluding scattered radiation. All data relating to tissue dose (incident + scattered radiation in the tissue) was to be designated as "tissue dose" if expressed in roentgens.

The term "dosage-meter" of the 1931 international agreements was replaced by Irradiometer to measure irradiation (e.g., r per min.) and Roentgenometer to measure quantity in roentgens.

Comment

Although these recommendations were both logical and practical, they were essentially ignored, and the unconscious or conscious perversion of the unit continued.

The report also states that "a dose is the product of the number of roentgens by some factor which takes all scattering into consideration." This presupposes an agreed definition of dose, which did not exist at this time.

REDEFINITION OF THE ROENTGEN AT THE FIFTH INTERNATIONAL CONGRESS OF RADIOLOGY, 1937 [36]

The redefinition of the roentgen represented a masterpiece of compromise, which apparently left each nation significantly concerned with dosimetry, believing that its main objectives had been achieved, although these objectives were not, in fact, identical.

As a warning to the reader, one should stipulate that this reviewer accepted the new definition in the light of British dosimetry, but has since enjoyed considerable "exposure" to the U.S. approach. Since there seems to be no access to the actual discussions of the units committee, the interpretation may be slightly distorted.

Relevant recommendations were:

Section A: Units

1. The International Unit of quantity or dose of X-rays or gamma-rays shall be called the "roentgen" and shall be designated by the symbol "r."
2. The roentgen shall be the quantity of X- or gamma-radiation such that the associated corpuscular emission per 0.001293 gram of air produces, in air, ions carrying 1 e.s.u. of quantity of electricity of either sign.
3. Measurements of radiation quantity shall be expressed in roentgens. Measurements of dosage rate shall be expressed in roentgens per minute.

Section B: Dose or the Specification of the Conditions of X-Ray Treatment

Not as a quotation--includes designated symbol D for dose e.g. in free air-D; at the surface of the skin (including back-scatter) - D_0 etc. at depth χ cm D_χ. and specifications of Quantity, Quality, and Technic.

Section C: Dose or the Specification of the Conditions of Gamma-Ray Treatments

Includes specifications of Quantity, Particulars of Radium Source, and Technic.

Section D: Instruments (abbreviated here)

X-ray Primary Standards - free air chamber or air-wall chamber for harder radiations.

X-ray Practical Instruments - air-wall chamber.

Gamma-ray Standards and Practical Instruments - air-wall chamber for primary, scattered, or a combination of both radiations. Improvements which we can assume were almost universally acceptable were:

(1) Inclusion of gamma-radiation.

(2) The mass statement 0.001293 gm, replacing 1 cc at 0° C and 76 cm Hg pressure; this was a major simplification, promoted, we believe, by the German school.

(3) Reference to production of ions rather than to measurement "at saturation current."

From the British viewpoint, the definition is as much a stipulation of a Bragg-Gray cavity filled with air and surrounded by an air-wall, as the earlier definition was a specification of a free-air chamber. Yet the free-air chamber was left as one experimental realization of Bragg-Gray conditions.

The term "associated corpuscular emission" was to the British, pure Gray. To the U.S. group, it had two merits:

(1) It excluded scattered quantum radiation.

(2) It included corpuscles other than electrons.

Section A 1 seems to make quantity and dose synonymous. In effect, it permitted maintenance of the quantity concept by the purists, and legitimatized those who had been "conscious perverts."

Section A 2, that which is normally quoted as the definition, does not include the dose alternate of Section A 1.

Section A 3 indirectly eliminates "intensity" from the usable terms. It did not include such alternates as "irradiation" as used in reference [35].

Sections B and C seem, to the reviewer, to use dose in a different sense from that of Section A.

We suggest that dosage, implying the general process of treatment and its attendant conditions would have been more appropriate, reserving dose for the concept of a measured quantity (quantity as a specified amount, not necessarily formal quantity of radiation).

We are puzzled by the phraseology of Section D. The first part evidently promotes continued use of the free-air chamber in the appropriate energy range, and adds the air-wall chamber for harder radiations.

We should note that this is the first time at which either X-rays or gamma rays could be measured by an approved primary method when the incident radiation is not necessarily confined to a narrow beam. This is an astonishing state of affairs when one recalls that therapy, for which special units were designed, rarely, if ever, involves such beams.

The difference in statements for X-ray practical instruments and gamma-ray instruments is peculiar, because the latter specifically includes the "primary, scattered or a combination" phrase. The only physical reason that we can see is that one may assume that the gamma-ray measurements will be made with a thimble chamber that is also a satisfactory Bragg-Gray cavity, whereas X-ray measurements (at then conventional voltages) will almost inevitably be made with empirical thimble chambers far too large to be acceptable cavities; this reasoning is speculative.

The proposals that the British units committee brought to the conference [37] show these points:

1. A provisional assumption that each of the various observed effects of radiation depends on the absorbed energy; and from the standpoint of pure physics, a unit of quantity based on such a conception would probably lead to the simplest correlation of the several effects.
2. The measurement is to be in an arbitrary unit of quantity or "dose" known as the roentgen.
3. Two proposed alternate definitions are both verbal descriptions of the air-filled Bragg-Gray cavity with air-wall (or in one case also air-equivalent wall). The recommendations generally reflect a compromise position recognizing the problems of making air-equivalent walls and especially of interpreting any measurements at interfaces.

In the reviewer's opinion, the expression "arbitrary unit of quantity" provides the same mental safety valve as did Failla's "effective quantity."

(In dwelling at some length on the British and U.S. contributions to the 1937 redefinition, we wish to emphasize that this is not intended to deemphasize the contributions of other groups. It occurs because these two are representative of two different approaches and partly because of the reviewer's lack of library facilities and his inadequacies in foreign languages.)

PROGRESS OF X-RAY AND GAMMA-RAY DOSIMETRY AFTER 1937

The British course has been dominated by the Bragg-Gray principle with progressive improvements in instrumentation to implement it. The roentgen has been universally used as a measure of tissue dose, with periodic excursions into the desirability of expressing dose as energy absorbed per unit volume of tissue. Attempts have been made to measure true energy absorption in various tissues, usually by indirect means, e.g. in Spier's work [38].

In Europe generally there was considerable work with small chamber measurements of radium applicators both before and after this time, as in the work of Sievert, Mallet, Smereker and many others.

In the United States, there was perhaps more reluctance to accept the Bragg-Gray methodology. Taylor, et al [39,40] are clearly dubious of the thimble chamber until they have proved its results against new free-air chambers built to be suitable for high voltage radiation. This is perhaps understandable in view of the excellence of the free-air chamber developments at the U.S. Bureau of Standards.

Terms applicable to dose measurement in roentgen therapy were given in the R.S.N.A. Technical Bulletin #1, 1940, which is still the American standard [41].

Some quotations are:

> "The exposure or dose is a measure of a property of the X-rays at a particular place. This place may be situated in air, in tissue or other material, or even in a vacuum."
>
> "Exposure and dose are used interchangeably. 'Exposure' is preferred because it calls attention to the fact that it describes the radiation to which the place is exposed. 'Dose,' by unfortunate association of ideas, may suggest incorrectly that it

refers to the radiant energy that is absorbed. The older term 'quantity,' which is unsatisfactory, is avoided."

"The average exposure rate, dosage rate, or intensity is obtained by dividing the exposure by the time required to deliver it."

Exposure is always to be used in a compound form, as free-air exposure, skin exposure, tissue exposure.

The roentgen is said to be a unit of dose or exposure of X-radiation. White, Marinelli, and Failla, 1940 [42], in a careful remeasurement of what we have previously called the Sievert dose, use the expression "specific effective gamma-ray intensity of radium" which was found to be 8.47 r per mg-hr at 1 cm (0.5 mm Pt).

Since the biological effects of conventional X-rays (e.g., 200 KV) and of radium gamma-rays are demonstrably different for equal "roentgenage," American practice, in particular, developed a gamma-ray roentgen with the symbol r_γ [43]. Although the proposal had some practical merit in discouraging the wanton addition of X-ray and gamma-ray contributions and unconsciously perverting the sum into a biological dose of significance, we have considered this step as retarding the whole-hearted acceptance of roentgen dosimetry for gamma-rays, where, in fact, it has a greater chance of being significant.

DOSIMETRY BETWEEN THE 1937 AND 1950 INTERNATIONAL CONGRESSES

Normal communication between scientific groups was cut off during much of this period. Since precise terminology is the foundation of scientific communication, one must concede from what has been written so far that communication in radiological dosimetry had previously been inadequate; signals were scrambled by what Newell so aptly calls coterie units. Paradoxically, communication improved in this period, and the period ended with a readiness to concede that dose should be expressed in terms of the quantity of energy absorbed per unit mass at the place of interest. Significant factors were:

(1) Neutron dosimetry

After the customary introductory empiricism, fast neutron dosimetry was placed on a sound Bragg-Gray basis [44].

For slow neutrons, it was pointed out (first by Failla?) that the incident energy may be low compared with the energy made available for ionization in tissue, the energy being released in various nuclear reactions.

It is convenient in the elementary teaching of the Bragg-Gray Principle to describe the incident radiation as a carrier mechanism [45], releasing the associated corpuscular emission at or near the place of interest. Thus X-rays, gamma-rays and fast neutrons are all carriers. In colloquial terms, if the incident radiation is carrying the ball (energy), the slow neutron case is a hidden ball play.

This is a desirable stimulus to forget about the formal teaching that if the incident energy is E, the absorbed energy is $E\sigma$, and that the roentgen is a measure of E, obtained by measuring $E\sigma$. The cells at point x in tissue are concerned only* with "associated

*or, more safely, almost entirely. Also, we must ultimately be concerned with specific ionization.

corpuscular emission" that can reach point x from a determinable sphere of influence. What carrier mechanism is used to achieve corpuscular emission is a matter of choosing a convenient technology.

(2) Proton dosimetry

Occasional excursions into proton irradiation [46] can easily be brought into the Bragg-Gray field.

(3) Beta-ray dosimetry

Workers in this field either measured in e.s.u. per cc of air, and apologetically converted to "roentgens" (e.g., Blomfield and Spiers) [47], or developed a coterie unit as some kind of "equivalent roentgen" (e.g., Marinelli) [48].

The equivalent roentgen, now well known, is based on these concepts:

(a) The roentgen as applied to quantum radiation is a well understood unit (?) in every day use in radiology.

(b) Regardless of literal interpretation, the roentgen "really" means an energy absorption of 83-85 ergs per gram of air. It corresponds with 93-95 ergs per gram of water or soft tissue for hard radiation.

(c) Therefore, an equivalent roentgen is a concentration of energy represented by X-ergs per gram of tissue. Marinelli chose X = 83 for his e.r. Essentially, Mayneord's gram-roentgen, applied to integral dose is the same concept (see C. W. Wilson for a review [49]). Also Gray's energy-unit is equivalent with X = 93.

(4) Dosimetry with mixed radiations

In the atomic energy program beginning in 1942, one was faced with the practical problem of adding the doses received by a large group of workers from quantum radiation, alpha, beta, and neutron radiation. Parker [50] was led to a coterie system that had to be readily communicable. He used rep as an equivalent roentgen, based originally on X = 83, but changed to X = 93 and rem as a biological "unit," obtained by multiplying the components of the dose in rep by appropriate RBE multipliers.

The principal merit of this variation lies in the communication advantage of pronounceable terms, such as rep and rem, and the simple arithmetical scale of relation.

Four bases for what Parker called "energy absorption dose" have been used from time to time.

These are:

(a) Energy absorption per unit mass - ergs per gram.
(b) Energy absorption per unit volume - ergs per cc.
(c) Ionization per unit mass - ion pairs per gram.
(d) Ionization per unit volume - ion pairs per cc.

The middle period of dosimetry usually spoke of (b) as the natural unit. The approach through the roentgen leads to either (a) or (c) in some arbitrary multiple form.

It can now be accepted that (a) is most convenient, and just as natural. At what stage there seemed to be mutual agreement to go to the erg per gram form is not clear from the literature. However, all these concepts are almost as old as dosimetry. For example, Th. Christen [51], as early as 1913 defined physical dose as the X-ray energy absorbed in a body element divided by the volume of the element. The preference of "per unit mass" to "per unit volume" was mentioned by Behnken [52] in 1934, and probably by many others.

(5) Dosimetry with betatrons

This field is concerned both with high energy electron beams and with high energy X-rays. Reports by Laughlin et al [53,54) adequately document the field. Many betatron installations were guided by one of a group of young physicists, who entered the medical radiological field without the bias of experience with the roentgen. It was natural that they approached radiation measurement through the conventional physical approach of formal intensity and formal quantity, measured calorimetrically. It is a natural step from this to the measurement of "dose" in ergs per gram.

We may note that similar conditions applied many years before with very soft X-rays, except that here total absorption in air, rather than in a dense material, was practicable. This technology, exemplified particularly by the German contributions, leads to a similar dose methodology.

We shall discuss the meaning of dose in roentgens at high energy later.

I.C.R.U. RECOMMENDATIONS - LONDON 1950 [55]

It was recommended that for correlation of the dose of any ionizing radiation with its biological or related effects, dose be expressed in terms of the quantity of energy absorbed per unit mass (ergs per gram) of irradiated material at the place of interest.

The definition of the roentgen was unchanged, but its application was restricted to quantum energies below 3 Mev.

Gamma-ray emission was to be expressed in terms of r per mc hr at 1 cm from a point source.

Condon and Curtiss [56] had proposed the unit r.h.m. (roentgens per hour at one meter) to measure gamma-ray source strength. Evans [7] and others have used this in the form rhm per gm for radium, and mrhm per 100 rd or mrhm per 37 rd (which is equivalent to mrhm per mc) for other radioisotopes. Parker [57], following Mayneord and Sinclair [58], has used a k-factor, and has indicated source filtration in parentheses. Thus, one writes

For Ra (in equil), k = 9.3

or k (0.5 mm Pt) = 8.4*

*or 8.25 if one accepts the mean of recent determinations.

This is a convenient way of generalizing the Sievert dose, or of abbreviating "dose-rate at 1 cm from a point source of 1 mc of radium (in equilibrium with its products) is 8.4* r per hour, at 0.5 mm Pt filtration."

I.C.R.U. RECOMMENDATIONS - COPENHAGEN 1953 [59]

Formal definitions of intensity and quantity of radiation, as applied to beams, were given.

Absorbed dose was defined as the amount of energy imparted to matter by ionizing particles per unit mass of irradiated material at the place of interest. It is to be expressed in rads. The rad is the unit of absorbed dose and is 100 ergs per gram.

Integral absorbed dose is the integration of the energy absorbed throughout a given region of interest. The unit is the gram-rad. 1 gram-rad = 100 ergs.

The roentgen, its definition unchanged and its application limited to quantum energies up to 3 Mev, remains the unit of X-ray and gamma-ray dose.

Apparently, absorbed dose is the only formal dose expression that has received unequivocal definition and universal acceptance. We here assume that sufficient evidence has been shown that the definition and interpretation of the roentgen have been ambiguous, and its acceptance variable in different schools.

The absorbed dose concept is not limited to a particular technology, and it is applicable to any ionizing radiation.

RECENT DEVELOPMENTS

Although the absorbed dose approach does not specify a particular measuring system, the Bragg-Gray cavity ionization method is clearly one method of choice. The relation is now usually expressed as:

$$Em = W \cdot S \cdot J_m$$

where Em = energy imparted to unit mass of wall material

J_m = ionization per unit mass of cavity gas

W = average energy expended to produce an ion pair in the gas

$$S = \frac{\text{mass stopping power of wall material}}{\text{mass stopping power of gas}}$$

Several factors contribute to make the method more difficult for high quantum energies. Appropriate values of W and S are in doubt. For W, the value for electrons in air seem to converge on about 34 e.v., and there is perhaps general agreement that W is not independent of electron energy.

For S, there is a correction due to the Fermi polarization effect [60,61,62]. This is a "density effect" in condensed materials such as S for the case of water wall-air cavity may fall by ∿6% from conventional quantum energies to 10 Mev, and a further ∿10% in going to 100 Mev [63]. Another approach to stopping power variation is that of Attix [64], and Spencer and Attix [65], who point out that energy loss of electrons traversing matter is not a continuum of local dissipations of infinitesimal amounts of energy, but includes also some large energy transfers, generating secondary electrons of appreciable range. This concept introduces a modification of S, which should be clearly discernable with chamber walls of different materials. Experimental evidence of this point (e.g., [66,67,68,69]), although conflicting, tends to support the view that the cavity is already aware of this situation

and samples the secondary tracks correctly in terms of what Gray called Group (b) particles in his original presentation [16].

Currently the Bragg-Gray Principle appears to have wide application to absorbed dose measurement subject to refinement in appropriate values of averaged W and averaged S* for

*The formal Principle is based on the geometrical picture of drawing a cavity of dimensions ρ times a geometrically similar element of medium. We now know that ρ is not constant for all groups of the associated corpuscular emission. Some kind of averaging process appears to be feasible.

various cases. It is possible that we are approaching a stage in which absorbed dose will be most conveniently measured in a suitable solid receptor.

As applied to measurements referable to roentgens, these complications are supplemented by others. The answer is unequivocal only if the whole chamber wall is subjected to a radiation field sufficiently uniform for practical purposes. For ultra-high quantum energy the required uniform field is large, many centimeters in radium. The useful concept of measurement at the place of interest (the cavity) tends to be lost. When the chamber wall thickness required to achieve electronic equilibrium in the cavity becomes comparable with the body dimensions, there is no useful meaning to the roentgen evaluation, even if the applied radiation field is essentially constant over the body volume.

The meaning of air-equivalent wall is also distorted because solid "air" will differ from the gas in polarization effect.

For these, and other reasons, the roentgen is currently cut off at 3 Mev, although measurements of N^{16} radiations by the writer's associates have not indicated significant difficulty in the range up to 7 Mev. The cut-off of the roentgen within the range now used in therapy and other activities (e.g., radiation protection) that require a knowledge of dose is certainly one factor to be weighed in deciding whether the roentgen has outlived its usefulness. Parker [1], (and probably many others) has pointed out that whole-hearted acceptance of the energy-absorbed dose concept relegates measurements in roentgens to a secondary status, in principle.

SUMMARY

This lengthy review of the development of dosimetry and the conflicting use of terminology in the field can be summed up as follows:

The terms dose, dosage, and dosimetry came into the language of radiology through the natural clinical analogy with the giving of a dose of some potentially curative agent.

As radiology became a more and more quantitative activity, the meaning of dose was distorted both by the character of the primary interest of the specific observer, and by the existing terminology in the applicable branch of physics or technology.

Notably in early X-ray treatment, reproducibility of treatment was governed by a need to know the "output" of the X-ray beam. The physical approach was to measure formal quantity of radiation E (time-integrated flux density). Since this could not be done directly in most cases, a sampling of the quantity in the form $E\sigma$ (where σ is an appropriate absorption coefficient) was made by the ionization in air under prescribed experimental conditions. The sampling $E'\sigma$ was recognized to be closely related to another sampling $E'\sigma'$, representing ionization in a given volume or mass of soft tissue.

The roentgen defines the quantity E, which has been called dose. The sampling $e'\sigma$, the experimentally observed factor, is thought of as dose. The related unmeasured sampling $E'\sigma'$ in tissue is also logically thought of as dose.

To add to the confusion, the quantity E, for one roentgen, is a variable function of the quantum energy of the radiation. Since unit quantity of radiation means one erg per cm^2 to the physicist, there is a mental block in using the roentgen as a unit of quantity of radiation.

A related mental block occurs in using the expression intensity of radiation.

The principal confusions in terminology affecting dose, the roentgen, quantity of radiation, and intensity of radiation are believed to be contained in the above picture; there is one other which becomes clearer after summarizing the radium therapy development.

In radium therapy, the sources either had constant gamma-ray emission (Ra-element) or the emission could be related to a constant through a known decay factor for radon. Treatment factors were then mainly concerned with the geometrical distribution of sources with respect to the part to be treated.

At a later stage, it became possible, largely through the Bragg-Gray Principle of cavity measurement, to relate gamma-ray measurements to roentgens. At this stage, the deferred confusion referred to above came into play. For equal "roentgenage" and all other factors being equal, biological effect for conventional X-rays and for radium gamma-rays was palpably different. Therefore, for those who thought in terms of "biological dose," gamma-ray measurements in roentgens were unacceptable. There is a double confusion here:

(1) The roentgen, at most, is only one parameter in the concept of biological dose.
(2) For X-rays, biological effect of a given "roentgenage" is certainly not independent of the quantum energy, although it happened to be nearly so over the most frequently used range of therapy in the 1930's.

Subsequent developments included:

(1) Extension of available range of X-ray quantum energies to completely bracket the Ra gamma-ray range; this makes a common system imperative.
(2) Use of gamma-ray sources of other energies (e.g., Co^{60}, Cs^{137}), which has the same effect.
(3) Use of particulate radiation, neutrons, protons, electrons, etc., which de-emphasizes the merit of the roentgen.
(4) Improvements in technology which now make it feasible to determine energy absorption in tissue by more than one method.

These developments have led to a battery of equivalent roentgens, based on the following plausible, but not critically defensible reasoning:

(1) Radiologists understand the roentgen.
(2) The roentgen "really" means an absorption of 83-85 ergs per gram in air.
(3) For hard radiation, this is equivalent to 93-95 ergs per gram in water or soft tissue.

Therefore, an equivalent roentgen is an "absorbed dose" of either 83-85 or of 93-95 ergs per gram, with the latter range favored since 1948. The rad is an agreed compromise for a unit of absorbed dose, based on 100 ergs per gram. This is a physical basis for dose of any ionizing radiation.

Returning to biological dose, which we interpret as a means of satisfying the mental urge to write down one single number that will categorize the anticipated biological effect, we can do this only when all treatment conditions other than nature of the radiation are kept constant. For radiations A and B, we then write, in effect:

Biol. dose in rems = Physical dose in rads X RBE (A/B)

where RBE (A/B) is the relative biological effectiveness of radiation A with respect to the reference radiation B. In measuring RBE, doses of A and B must be stated in rads, which is very rarely done. There will now be some practical cases in which treatment with both radiations A and B can be evaluated in rems (as opposed to the either-or irradiations to obtain RBE). There will be other cases in which the rem components of A and B are not additive, this depending, among other things, on what is loosely called time factor. In any case, the probability of obtaining a single number representation of "true biological dose" covering all types of radiation, all plausible organizations of protraction and fractionation, and all geometrical dispositions is vanishingly small.

RECOMMENDATIONS ON UNITS AND TERMINOLOGY

For this purpose, we must optimize a group of factors, which are mutually conflicting; there is no ideal solution, and the approach is an informal operations research analysis and synthesis.

The principal factors involved are:

1. What unequivocal definitions of standards or units, that will continue to be meaningful (with the same meaning) to radiologists, physicists, and other interested specialists can be used?
   We will note that many existing standards are such as to lead to "unconscious perversion."
2. What are the basic current needs in radiotherapy? e.g., if 90% of all therapy situations involve X-rays of quantum energy between 80 Kev and 400 Kev, or gamma-rays from Ra or Co^{60}, should the standards be made highly convenient and reproducible in these ranges, at the expense, if necessary, of considerable difficulties with other radiations?
3. What are the basic needs of advanced radiobiology?

If these differ from the needs in radiotherapy, how much weight should be given to them?

4. Similarly, what are the needs in other industrial applications which incidentally lead to the irradiation of people or other life forms (e.g., radiography, atomic energy applications)?
5. What are the foreseeable trends in the above, and to what extent can they be promoted by wise decisions on units or standards?
6. To what extent have past or current concepts been irrevocably compromised by conflicting interpretation? e.g., useful concepts can be developed by defining exposure so that it is some measure of an intrinsic property of radiation, with dose defined as some measure of the action in tissue. The U.S. Technical Bulletin has used exposure and dose synonymously for 15 years.
7. To what extent can dormant units be revived and given an entirely new meaning, as was done with the 1918 rad in 1953?

   This is cogent because there is current need for an agreed term which has the general characteristics of either Eve's number or Sievert's Imc. Both these observers are worthy of the honor of permanent recognition in the language of dosimetry.
8. All terminology must be equally differentiable in the principal scientific languages of the world. While the official languages of the international conferences have been English, French, and German, it is suggested that Spanish and Russian be added. e.g., although exposure and irradiation are differentiable in English and French, they appear to correspond with bremsstrahlung in both cases in German. They therefore cannot be given different specific meanings in the field.
9. Should the whole language problem and past confusions be eliminated by a sort of "radiological esperanto"? This question is already answered by three circumstances:
   (a) Newell's plan, rothion, kludon, rhegma, etc. had no acceptance, because a radical change can only overcome the inertia of tradition in a revolutionary atmosphere. There is no occasion for revolution in the present field because all the needed concepts have been in the literature for at least 40 years, and have waxed and waned.
   (b) The rad, interpreted as another equivalent roentgen is a concession to roentgen tradition. Otherwise, it is a redundancy for that which is better expressed as ergs per gram, unless we use it for communication's advantage in going on to such a concept as the rem.
   (c) The Commission's instructions to the writer require a clarification of existing terms rather than re-definition. This is not entirely possible, but is interpreted to call for minimum redefinition.
10. What is the appropriate decision on recommending standards that call for difficult technical procedures in the field?

    This is a most controversial point. The writer's approach to this is that if reasonable standards are provided, there is more probability of promoting the necessary technological advances speedily. For example, if the subject technology involves a knowledge of W and S in various situations, one should accept the standard even though W or S are currently quite uncertain in some applications.
11. A point closely related to 10 involves the correct approach if the technology is good for nearly all applications but highly complex for specific instances, such as at interfaces.
12. Should we be limited to one set of units or is it permissible to establish duplicate systems?

    We have shown that even for so simple a matter as the measurement of electric current, multiple unit systems are commonly used.

    We shall proceed to determine that it is feasible and desirable to use a common system for a measurement of the basic physical interaction of radiation with tissue. It is desirable, although perhaps not absolutely necessary, to have different systems for the measurement of the "output" or "emission" of radiating sources.

We arrive at the following conclusions:

Dose Concepts

Three dose concepts cover the current and anticipated needs of all fields of application of ionizing radiation to living matter.

These are:

(1) A physical dose, corresponding in form with Section AI of the 1950 I.C.R.U. Recommendations [55], and providing a definition of absorbed dose similar to that of Sections I, 3 and 4 of the 1953 I.C.R.U. Recommendations, [59] with these changes:
In I 3: It shall be expressed with in ergs per gram or in rads.
In I 4: The rad is the practical unit of absorbed dose and is 100 ergs per gram. Its use facilitates approximate translation of previous experience in exposures measured in roentgens and further translation to nomenclature in rems.
(2) A dose parameter, which under some conditions permits the statement of dose at the place of interest as a single number of rems, independent of the type of ionizing radiation used, but referable to the pertinent biological effect produced by the same number of rads of an agreed reference radiation.

Such a dose parameter shall be called bio-dose. It shall be expressed in rems. Bio-dose, where applicable at all, is derived from absorbed dose by multiplying it by the appropriate relative biological effectiveness factor.

Thus

Bio-dose in rems = (Absorbed dose in rads) X RBE

In determining the applicable RBE, doses of the radiation to be tested, and of the reference radiation must be stated in rads.

The recommended reference radiation is such X-radiation as yields an average specific ionization of approximately 100 ion pairs per micron of water [70].*

*Ed. Note: There is some objection to this choice because of the uncertainty in the effective specific ionization. A reference radiation could be defined in terms of x rays generated in a specific voltage and with a stipulated filter, or more clearly Co^{60} could be used. The important point is for all to agree on the same reference.

The concept of bio-dose is currently of little value in radiotherapy; it may ultimately have some limited application of combined X-ray and radium treatments for example. It is of high practical value in radiation protection applications.
(3) A dose parameter which reduces all the factors influencing the relevant biological effect of radiation to a common standard of reference. This would be the concept of "true biological dose." As indicated in the review, we conclude that such a parameter will not be developed in the near future.*

*All mathematical expressions involving recovery factors and some current attempts to generalize permissible exposure are steps in this direction.

If it should be developed, it would supercede the proposed bio-dose, and one could reasonably use the term bio-dose with modification to measure in rebs. In the transitional period,

Bio-dose in rems - takes account of the spatial arrangement of ionizing events along single tracks in tissue.

Bio-dose in rebs - takes account of the complete spatial and temporal distribution of ionizing events in the relevant tissue, and of all other parameters contributing to the biological response of interest.

We propose that absorbed dose be normally shortened to dose, when the context is clear.

Bio-dose, rarely used in radiotherapy, will always be written bio-dose.

We strongly support the British position [71,72] on the adoption of the rad in therapeutic practice. This endorsement does not necessarily imply agreement with specific values of W and S proposed in these papers; references [71] and [72] are not internally consistent on this.

The roentgen will no longer be a unit of dose. "Tissue doses" in roentgens in present practice may be converted to absorbed doses in rads without numerical change, until such time as more reliable experimental data are available. This fortunate situation arises because with W = ∿34 ev. the familiar figure of 93 ergs per gram per

roentgen in water, for hard radiation is raised to ∿97 ergs per gram. The 3% difference between this and the rad is inconsequential in therapy.

In more complex situations the dose in rads will require direct measurement. The complications at or near interfaces are well known. Since they are generally ignored in clinical practice, nothing is lost by temporarily using dubious rad values, until physical practice catches up to its obligations to measure absorbed dose. As an aid to stimulating this process, we would personally recommend a technique for isodose curves presented at the 1937 International Congress [73]. This involves normalizing the curves to 100% of the central axis at an agreed depth (5 cm was recommended). In this way, the transitional layer at the surface can be marked "uncertain" and improved data written in, from time to time, without disturbing the curves in the balance of the charts.

Output Concepts

Measurements of "output," "source strength," or "emission" of radiating sources are still useful as a tool to promote reproducibility of treatment within a clinic, and between clinics. Since the topic includes all types of ionizing radiations, we conclude that it is neither necessary nor immediately desirable to attempt to use a universal system. One anticipates that the general solution would come through a specially designed rad meter. We will here consider some of the radiations separately.

Quantum Radiation

(a) X-radiation

The output of an X-ray tube may be measured in roentgens per unit time at a convenient point relatively uninfluenced by scatter from surrounding objects.

The physical quantity measured is an exposure-rate.

Exposure-rate is defined as the flux density of a beam of quantum radiation expressed in terms of roentgens per unit time.

Exposure-rate is to be differentiated from intensity as defined in current I.C.R.U. Recommendations. For equal intensity, exposure-rate is a function of the quantum energy.

The only violation of present practice in this suggestion, is that exposure-rate and dose-rate are not synonymous.

(b) Gamma radiation

The output of gamma-ray sources of quantum energy up to 3 Mev can be stated as an exposure-rate in roentgens. For many applications of radio-isotopes, it is of practical significance to have a specific name for the gamma-ray exposure-rate in roentgens per hour at 1 cm from a point source of activity 1 mc. Such a quantity has obvious relationships to Eve's number K, originally applied to RaC, and to Sievert's Imc. The term K-factor or K-value has been applied to the coefficient involved.

Eve's number, in its original form, has outlived its usefulness; yet there is a considerable body of opinion favorable to the permanent recognition of Dr. Eve's contributions in the language of gamma-ray measurement. We propose the international acceptance of K in these terms:

"K is the numerical coefficient of the gamma-ray exposure rate stated in roentgens per hour at 1 cm from a point source of 1 mc of a specified radioisotope. It may be applied also to the radiations from a family of radioisotopes in which the activity of the controlling parent is 1 mc. K is to be recognized as a modern application of Eve's number, with which it is not identical." As examples,

For Co^{60}	K = 13.6
For Ra(in equil)	K = 9.2
and K(0.5 mm Pt)	= 8.3

We see no advantage in formulating an arbitrary term for the compound unit "roentgens per hour at 1 cm," although a uniform method of abbreviation would be useful. There is no occasion to accept the r.h.m. (rum) in radiotherapy or radiobiology. It may be a convenient coterie unit in such operations as radioisotope shipment. The principal defect of our terminology for quantum radiation is that it excludes all radiation not measurable in roentgens.

For X-rays, this includes betatron output, which we expect to be measured in the formal terms of intensity.

For radioisotopes, it includes about 20 nuclides which have gamma-ray emissions above 3 MeV. None of these has current significance in therapy. Some pose significant radiation problems in atomic energy work.

Particulate Radiations

For the particulate radiations, the methods appropriate in conventional physics should be used. These include:

(1) Particle flux density (in the form N/cm^2 sec).
(2) Energy flux density (in the form NE/cm^2 sec).
(3) For slow neutrons, the familiar product, nv.

NOTE: In many applications for quantum or particulate radiation, calculation proceeds most naturally from the total emission rate of an infinitesimally small surface or volume element, or from the emission-rate into unit solid angle. Such methods are appropriate provided that the physical terms are clearly defined.

Related Terminology

The above concepts, if accepted, require modification in the use of the terms dosage, dosage-rate, dosage-meter and dose-meter (or dosimeter).

We submit that dosage is a general term relating to the process of administering radiation to a specified object (usually a patient, tumor, or biological target). It is that part of the total specification of treatment conditions directly connected with determining dose.

Dosage-rate is preferably superceded by dose-rate. Dose-rate refers to time-rate of dose as measured in ergs per gram per unit time, or rads per unit time. It excludes measurements in roentgens per unit time, which reports exposure-rate. Dosage-meter is eliminated. As an alternate, dosage-meter is a general term including exposure-rate meter or dose-rate meter or dose-meter; it is any radiation measuring instrument contributing directly (i.e., by measuring absorbed dose or dose-rate) or indirectly (i.e., by measuring exposure-rate) to dose determination.

Exposure is the time integral of exposure-rate; it is not needed in this scheme, but may be a convenience in translating exposures in roentgens into doses in rads.

Alternate: Since exposure and dose have been used synonymously in some countries, it may be preferable to substitute irradiation for exposure. We then have irradiation and irradiation-rate. We prefer to reserve irradiation for the voluntary or involuntary act of irradiating or causing ionizing radiation to encounter a material of interest.

The term tissue dose is no longer specifically required although it can be appropriately used in the sense of absorbed dose at a place of interest in tissue. Similarly, one can use tumor dose, skin dose, bone dose.

The term air-dose should be avoided, with exposure taking its place, this will eliminate confusion between roentgens and rads. Integral absorbed dose is used in the sense of the 1953 ICRU Recommendations. With the present scheme this could be successfully contracted to integral dose.

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APPENDIX TO "SOME BACKGROUND INFORMATION ON THE DEVELOPMENT OF DOSE UNITS"

Certain interesting features found in the literature were not included with the general background material. Delayed availability of reference material was partly responsible for this.

In reviewing the literature, I have found a number of cases in which an original reference has been given erroneously (there are 3 variations of reference [16]); when subsequent review papers repeat the same error one suspects that the reviewer has not personally referred to the original work. Where the items in question are options or interpretations, rather than numerical results, this is dangerous and misleading. I have tried to avoid this. However, references [2], [9], and [68] were unavailable.

No copies of Arch. d'élec. med., J. de Radiol. et d'électrol., Med Klin, or old issues of Acta Rad., and B.J.R. prior to 1928 were available.

This appendix will give some quotations or references that were not woven into the background review, but which may be of general interest to other reviewers. They are not necessarily in logical order:

1. B.J.R. (Roentgen Society 23, April 1927)

   According to C. W. Wilson [12], this contains an account of the steps by which the 1938 definition of the roentgen was reached.

   He notes that both Mallet and Coliez pointed out the necessity of unification of dosage so that the X-ray unit would also be available for measurements of gamma-rays.

2. 15th Convention of the German Roentgen Ray Soc., Berlin 1924. Also in Am. J. Roentgenol. 12, 185, 1924.

   This contains a definition that was a close forerunner of the one adopted for the roentgen in 1928:

"The absolute unit of roentgen-ray dose is obtained from that roentgen-ray energy which by fully utilizing the secondary electrons produced, and by avoiding secondary radiation from the walls of the ionization chamber produces in 1 cc of atmospheric air, under normal conditions, such a degree of conductivity that the quantity of electricity measured by saturation current equals 1 e.s.u."

The unit was written as "R." I understand that "normal conditions" in the German technology referred to a standard temperature other than 0° C, requiring a small correction factor to reduce R-units to roentgens. Note that this definition states that the "unit of dose is obtained from that radiant energy which . . . . ." The roentgen is "that quantity of radiation (i.e., radiant energy per cm^2) such that . . . . ." It is on this variation of terminology that much of the confusion of interpretation of the roentgen is founded.

3. J. A. Crowther, Brit. J. Radiol. 2, 1975, 1929 Crowther, discussing the newly formed "roentgen" says:

"It may be necessary to define our unit dose, in practice, in terms of a material standard in a certain ionization chamber rather than in terms of ionization per cc of air.

"We need not feel unduly alarmed at this possibility as we should only be following the example of the curators of the meter and the kilogram."

Now if we accept the rad in therapeutic practice we can well dispense with the roentgen, using in its place a standard rad meter, that is one certain equipment to which all secondary standards would be referable.

The objections to this are philosophical only. We find the method palatable for the basic C.G.S. units, length, mass, and time, but less so for derived units (ergs per gram). Contributing to this is a comfortable, but misplaced, confidence in the immutability of the material standards, which should be shaken if we re-examine them from a frame of reference moving at relativistic velocity with respect to them.

4. Seitz, L., and H. Wintz "Unsere Methode der Roentgen - Tiefentherapie und Ihre Erfolge." Urban and Schwartzenberg, Berlin 1920. This appears to be the defining reference to "skin unit dose."

5. Sievert, R. M., Acta Rad., 18, 742, 1937

Sievert objects that no dose method is adequate unless each spectral component contributes in the same proportion to the measured effect; this is a "biological dose" concept.

Sievert opposes the roentgen for gamma-rays. If accepted, he would differentiate

rc for curietherapy
and rr for roentgentherapy.

Obviously, Sievert's objection would also make the roentgen useless for X-rays.

He re-proposes his Intensity-curie (Ic) unit for Ra gamma-rays, and points out that intensity is already used in many different senses in physics, and that one more application, provided it is properly defined, is not objectionable.

6. Laurence, G. C., Radiol. 40, 92, 1938

Laurence points out that the definition of the roentgen is a definition of a standard, and only a partial definition of the unit. This is true generally of the definitions of physical units. The definition of the unit includes a full discussion of the procedures of observation and the arithmetic that are involved in measurements in terms of the unit--details established by common agreement and practice.

We suspect that this duality of function of the definition has contributed to misinterpretation.

7. Failla, G., Conference on Electronic Instrumentation in Nucleonics and Medicine, New York, Oct.-Nov. 1949

   This paper gives a lucid account of the meaning of quantity of radiation as used in the definition of the roentgen. To the objection that the quantity is a variable amount per roentgen (depending on quality) he responds that the same situation occurs in other units. For example, the curie, a unit of radioactivity, is defined as the quantity of any radioactive nuclide in which the number of disintegrations per second is 3.700×10^{10}. Failla assumes that quantity is here synonymous with mass, and the mass per curie depends, of course, on the nuclide of interest. The parallel is valid if one writes that the roentgen is a unit of dose (or exposure, if we accept the revised terminology); it is not so acceptable if one writes that the roentgen is a unit of X-ray quantity, as was originally done, for then the parallel statement would be "the curie is the unit of mass of nuclides" which is objectionable.

8. Chalmers, J. A., Phil. Mag. 6, Suppl. 7. 745, 1928

   This is an interesting reference to the pure physicist's approach to measuring intensity of radiation by ionization methods through the use of an "ionization function."

   In physics, one is still measuring the ionization function, whereas in radiology one has agreed (in roentgen dosimetry) that the parameter of interest is the product intensity x ionization function, which is called exposure-rate, and is used in the time-integrated form, exposure.

   Formerly, the ionization function was generally written K, which would create a conflict with K derived from Eve's number. We believe that this use of K has been obsolete long enough to create no confusion.

9. Glasser, O., Am. J. Roentgenol. 33, 293, 1935

   Glasser proposed the name "eve" for what we would call the gamma-ray exposure in one hour at 1 cm from 1 gm Ra element. Converting to a mg basis, Glasser's value at that time was 7.35 r. The reference is included to show an interest in the suitable honoring of Dr. Eve, which we propose to achieve through the coefficient K.

10. Gray, L. H., Brit. J. Radiol. 22, 677, 1949

    This paper refers to measurements of dose of internal beta-gamma emitters by incorporation of the nuclide of interest in a Bragg-Gray chamber wall.

    It is included to complete the cycle of Bragg-Gray type measurements.

    It uses the term radiation dose for the total energy dissipated per gram of tissue. This is in the same sense as the later-defined absorbed dose.

11. Gray, L. H., Brit. J. Radiol. 17, 327, 1944

    Gray gives here a good description of the significance of dose and dosage-rate, in radiobiology.

    Dose in appropriate units is a measure of the quantity of energy dissipated, either as ionization and excitation, or as heat per unit volume of the tissue irradiated (we could now agree to change this to "unit mass"). The most important primary effect is ionization. In most circumstances in therapy, dose in roentgens is proportional to the total number of ions formed per unit volume of tissue.

Dosage-rate is a measure of the rate at which one passage of an ionizing particle is followed by the next or, alternatively, dosage-rate controls the interval between successive groups of ionizing events within a given region of the cell. The effect really depends on this time interval, not on dosage-rate per se. The influence of dosage-rate is different for different qualities, so it may differ even at different depths in the same treatment.

12. Energy Absorption and Integral Dose

C. W. Wilson [49] gives an adequate review of this field. Much of the British literature uses energy absorption as synonymous with integral dose, or what is called integral absorbed dose in I.C.R.U. Recommendations. In some American teaching, energy-absorption dose has been used for what is now formally absorbed dose. There is a possibility of confusion if these terms are used out of context.

In our opinion, energy-absorption dose would have been preferable to absorbed dose. The latter concept may imply that the dose existed before it was absorbed. For our purposes, we are effectively saying that the significant parameter is energy-absorption (per unit mass) and that this parameter shall be called dose.

13. Reviews of Dosimetry

There are many reviews of the earlier concepts. Some useful references are:
a. Glasser, O., Radiol. 37, 221, 1941
b. Quimby, E. H., Am. J. Roentgenol. and Rad. Ther. 45, 1, 1941, for Ra. [Our reference (11).]
c. Quimby, E. H., ibid. 54, 688, 1945, for roentgen rays.

For recent reviews, read:
Marinelli, L. D., Ann. Review of Nuclear Science 3, 249, 1953, and Marinelli, L. D., Rad. Res. 1, 23, 1954

14. Conversion of Roentgens to Rads

Assuming that W = 34 e.v. will be a generally accepted value, 1 roentgen corresponds with 87.7 ergs per gram in air. Hard gamma rays which yield 87.7 ergs per gram of air will yield 97.7 ergs per gram of water. Beta rays which yield 87.7 ergs per gram of air will yield 101 ergs per gram of water.

Since the difference between 97.7 ergs per gram and 100 ergs per gram is less than the incidental error in radiotherapy, there will be many cases in which a one-to-one numerical conversion of roentgen to rads is appropriate.

The closer the value of W approaches 34.8 e.v., the better does this conversion become. Such a value is temptingly close to the value 35 e.v. acceptable for protons. There would be little loss in accuracy in practical cases if W = 35 e.v. were accepted for all absorbed dose calculations.

15. Memorandum by the Netherlands Dosimetry Commission July 1955 on Revision of 1953 I.C.R.U. Recommendations

This memorandum relates to the inadequacies of the definition of the roentgen which have been covered in the background material. These are briefly:

1. Ambiguity due to defining the roentgen as a "quantity. . . . of radiation such that . . . ." The proposal is to write "The roentgen shall be a dose of . . . . radiation such that . . . ."
2. Stipulation that the roentgen serves only as a unit for description of the radiation itself at the place of interest, independent of the medium present at that place.
3. Ambiguity concerning contributions to ionization by scattered (quantum) radiation in standard measurements. In principle, we agree with the first two points raised by the N.D.C., and would support them if exposure were proposed

As to the third point, we submit that the intention to avoid scattered (quantum) radiation in measurement is clear, and that the method of correcting for it, if present, is an experimental detail.

However, as we support the proposal to use the rad in therapeutic practice, we relegate the roentgen to a secondary status and anticipate its obsolescence. We therefore recommend that no change be made in the formal definition. The definition can be amplified by constructive explanations of the ambiguities brought out here, with the substitution of exposure for dose.

16. Memorandum Prepared by Dr. E. W. Emery, U. of Manchester for the N.P.L. Advisory Committee on Radioactive Standards. BRU/49A. Emery objects to the definition of "quantity of radiation" as time-integral of intensity. Normal usage in physics, according to him, would imply that quantity of radiation should have the dimensions of energy rather than energy per sq. cm.

    We are not familiar with this usage. However, greater clarity is assured by writing: radiant flux density instead of radiant intensity, and time-integrated radiant flux density instead of quantity of radiation.

    Strictly speaking, we believe that "radiant energy flux density" or "radiation flux density" are preferable to the "radiant flux density" expression as used by Morgan and Corrigan.

In moving away from the terms intensity and quantity of radiation as defined in the 1953 I.C.R.U. Recommendations, we doubt that any gain is made.

The term "flux" is certainly ambiguous, sufficient proof being offered by its different definitions in Sections I and II of the N.R.C. Glossary of Terms in Nuclear Science and Technology. In addition, we consider it quite acceptable to speak of photon flux, quantum flux, particle flux and the like, where the interest lies in the rate of passage of particles through a surface. This is to say that flux should mean rate of flow across a surface; whether the flow refers to a material thing or to energy is stated separately, e.g., particle flux or energy flux. If one writes radiant flux, flux intrinsically means rate of flow of energy across a surface; radiant flux then means rate of flow of radiant energy across a surface, and we cannot use particle flux without changing the meaning.

CRUSP-1
Part III

SUGGESTED GLOSSARY OF TERMS

absorbed dose: Amount of energy imparted to matter by ionizing particles per unit mass of irradiated material at the place of interest. Expressed in ergs per gram or in rads.

bio-dose: Term used for doses converted to biological equivalence with respect to radiation type by the operation

$$\text{bio-dose in rems} = \Sigma\,[(\text{absorbed dose in rads} \times \text{RBE}]$$

summed over all the types of radiation involved.

Bio-dose: Term reserved for the future to express "true biological dose" by converting physical dose by mathematical operators including factors for RBE, protection and fractionation etc. It is to be expressed in rebs. The concept of Bio-dose in rebs may not become practical; if it does, it would replace bio-dose in rem.

biological dose: A concept of dose in which equal doses would produce equal biological effect.

bio-dose (rem) is a partial biological dose which takes account of spatial arrangement of ionizing events along single tracks in tissue.
Bio-dose (reb) is a true (but currently hypothetical) biological dose that takes account of spatial and temporal distribution of ionizing events and any other contributing parameter.

bone exposure: Exposure in roentgens at a point in bone.

bone dose: Dose in rads at a point in bone.

dosage: That part of treatment conditions directly related to determination of dose.

dosage-meter: Preferably not used; alternatively, any radiation measuring instrument contributing directly (absorbed dose or dose-rate) or indirectly (exposure or exposure-rate) to dose determination.

dosage-rate: Preferably not used.

dose: Specifically means absorbed dose; it is expressed in ergs per gram or in rads. Generally dose is also used in various ways as a parameter for measuring radiation or certain of its manifestations--see Part I.

dose-meter: A device for measuring dose in rads, or dose-rate in rads per unit time.

dose-rate: Time-rate of absorbed dose.

dosimetry: Science of measuring dose, specifically absorbed dose or bio-dose.

emission: Process of release of energy from a radiation source, or specifically, the magnitude of the energy release.

emission-rate: Time-rate of emission of energy.

exposure: Time-integrated flux density as measured in roentgens. Exposure is a measure of a property of quantum radiation at a particular place.

exposure-rate: Flux density as measured in roentgens per unit time. Used to measure output of sources.

flux: Rate of flow of energy, particles, etc. can be specified as energy flux, particle flux, quantum flux, etc.

flux density: Flux normal to unit surface.

free air exposure: exposure in a region in air where the exposure of scattered radiation from objects external to the source is negligible.

gram-rad: Unit of integral absorbed dose equal to 100 ergs.

integral absorbed dose: Integration of the energy absorbed throughout a given region of interest. The unit is the gram-rad.

integral dose: Same as integral absorbed dose.

intensity: Intensity of radiation is the energy flowing through unit area perpendicular of the beam per unit time.

irradiation: In the general sense, the voluntary or involuntary process of act of irradiating, i.e., causing ionizing radiation to encounter a material of interest. (Specific) Irradiation could be used in lieu of exposure if differentiation between exposure and dose is judged to have been compromised by past practice.

irradiation-rate: Used instead of exposure-rate if irradiation is substituted for exposure.

K: A numerical coefficient defining the gamma-ray output of a radioisotope.

Gamma-ray exposure rate = K roentgens per hour at 1 cm from a 1 mc point source

output: General term for the concept of the amount of radiation emitted by a source; not a scientific term, and is variously applied to emission-rate, exposure rate at an arbitrary distance, and so on.

physical dose: The general concept of dose measured in physical terms as by energy absorption; specifically physical dose is expressed as absorbed dose.

quantity of radiation: Time-integral of intensity, or time-integrated energy flux density, or energy density.

rad: Unit of absorbed dose, 100 ergs per gram. The rad is an arbitrary "roentgen equivalent" replacing such terms as rep.

reb: A unit of "true biological dose;" the term is reserved in the event that such a concept should become useful.

reference radiation: An arbitrarily chosen radiation type used as the standard of comparison in RBE measurements; usually X-rays yielding about 100 ion pairs per micron of water.

rem: "Unit" of bio-dose defined by the relation bio-dose in rems = (absorbed dose in rads) x RBE

rhm: Roentgens per hour at 1 meter, an arbitrary compound unit useful in some applications of radioisotopes.

roentgen: Considered as a unit of exposure. Its magnitude is as defined in I.C.R.U. Recommendations.
skin exposure: Exposure at skin surface.
skin dose: Dose in rads at skin surface.
tissue exposure: Total exposure at a place in tissue.
tissue dose: Dose in rads at a place in tissue.
tumor exposure and tumor dose: Used similarly to tissue exposure and tissue dose, generally expressing an average value throughout the tumor, unless otherwise specified.

CHAPTER 16. NBS RADIATION PHYSICS PROGRAMS (1949-1963)

By 1949, with the programs of the X-Ray Section so diversified and still expanding, it was decided to organize the work into several sections under the administrative title of a "laboratory," which was equivalent to a branch in other Government organizations. The Atomic Physics Division was renamed Division of Atomic and Radiation Physics, with the "Atomic Physics" part also designated as a laboratory.

Beginning in 1950, the Radiation Physics Laboratory prepared summaries of its activities for such informal uses as recruiting new staff members and for budgetary discussions, as well as for the general information of outside organizations with which the laboratory was involved. The following material has been drawn largely from these reports, which were produced on an irregular basis from 1950 to 1961.

In 1950, the organizational structure was as follows:

Radiation Physics Laboratory, Chief, Lauriston S. Taylor
Section 8 Nuclear Physics, Chief, U. Fano
Section 9 Radioactivity, Chief, L. S. Taylor, Acting
Section 10 X-Rays, Chief, H. O. Wyckoff
Section 11 Betatron, Chief, H. W. Koch
Section 12 Nucleonic Instrumentation, Chief, H. O. Wyckoff, Acting
Section 13 Radiological Equipment, Chief, S. W. Smith
Section 14 AEC Radiation Instruments Branch, Chief, R. L. Butenhoff

At that time, the work of the laboratory was described as falling into six categories which did not necessarily adhere to sharp section lines. The categories were: Protection and Shielding Research (experimental and theoretical); Radiation Protection Recommendations and Codes; X-Ray, Gamma-Ray and Radioisotope Standards, Measurements, and Instruments; Theoretical Studies; General Atomic and Nuclear Physics Research; and X-Ray Equipment Research and Development.

The initial report pointed out that most of these activities required large and complex equipment together with highly specialized installations of a type not likely to be found in private or industrial institutions. This was particularly so in the high-voltage regions, where the NBS facilities for standardization and research were probably unparalleled. Section 14 was the Instrumentation Branch of the Atomic Energy Commission, which had been moved from Oak Ridge to NBS for closer collaboration with the Bureau's programs. This enabled the Bureau to perform improved services for other agencies by utilizing instrumentation experience of the AEC. Because the AEC Branch was in touch with industry-wide developments, practically all industrial work in the field of radiation instrumentation sooner or later involved the Bureau's Radiation Physics Laboratory.

In addition to the high-voltage and radiation laboratory built in 1940, the radiation complex now included a new betatron laboratory built next to the original building. The laboratory had an open bay, 60 x 25 x 30 feet high, designed to house a 50-MeV betatron and a 100-MeV betatron, the latter of which was converted at little additional cost to a 180-MeV synchrotron before delivery and installation. Adjoining the bay was a completely instrumented measurement room into which the radiation beams could be directed through any type of shields up to thicknesses of 10 feet. The beams could also be taken out-of-doors for a distance of some 500 yards.

Between the high-voltage and the betatron laboratories was a one story annex housing special generators, transformers, and switch gear for operating the various units in both buildings. The annex also contained a large 200-volt storage battery installation for exciting the motor-generator fields to obtain steadier output voltages.

The radioactivity laboratories, which were engaged in the measurement of radioactive samples, the calibration of radioisotopes, the development of special counting equipment, beta-ray spectrometry, and the sampling of radon air, were located in other buildings. A 60-inch cyclotron at the Department of Terrestrial Magnetism of the Carnegie Institution was also available to the Radiation Physics Laboratory.

At the time of the U.S. involvement in Korea in 1950, the NBS Director sent a Commerce Department memorandum to all divisions requesting each to list its capabilities and plans to convert to military research and development, and to indicate the existing programs that might be curtailed. The Radiation Physics Laboratory respectfully declined to list any curtailment on the grounds that its unique staff capabilities and laboratory facilities constituted a strategic resource which would be particularly important in the event of a long drawn-out conflict. Moreover, the laboratory was already engaged in a variety of projects of direct or indirect importance to the Department of Defense and the Atomic Energy Commission. A somewhat startled Director agreed.

Theoretical Studies

Fano's programs started in 1948 with theoretical determinations of the multiple scattering of radiation in large barriers. This work, spearheaded by L. V. Spencer and later M. J. Berger, was closely coordinated with the experimental programs. It was designed to gain a better understanding of the progressive transformation which radiation beams from betatrons and synchrotrons, as well as other high-energy sources, undergo while traversing protective barriers and other materials. These transformations involved a chain of degradation processes of pair production, scattering, secondary radiation, etc. During this period (1948-1950), Fano also started an extensive program devoted to the collection and the evaluation of nuclear and atomic cross section data. These compilations, by Gladys (White) Grodstein and later J. H. Hubbell, became landmark sources for such information.

To provide experimental verification of their theories (Refs. 144 and 211), the Radiation Theory Section also carried out, under Evans Hayward, model experiments using cobalt-60 and cesium-137 sources in a large water tank of the NBS Hydraulic Laboratory. For the first experiments with the gamma-ray source in the middle of the tank, the response of ionization chambers and counters in water was followed up to distances of 16 mean-free-paths of the primary rays. Fano took much deserved pride in the practical applications to which his theoretical studies contributed.

Similarly, the theoretical work on electron penetration and diffusion, begun in the mid-1950's under C. Blanchard and L. V. Spencer and extended by M. J. Berger, was complemented by parallel experimental programs carried out by F. H. Attix under H. O. Wyckoff. Among the principal outputs of these programs were the famous NBS Circular 499 (Ref. 257a) and NBS Monograph 1 (Ref. 670), which remain the definitive works in the field, along with the first modern tabulation of electron stopping powers (1956) by Ann T. Nelms (Ref. 659). Basic to much of this work was the first systematic tabulation of Mott cross sections, developed in the mid-50s by Spencer and J. Doggett.

While the laboratory had less direct contact than was originally planned with the biological and medical radiation field, Fano carried out some liaison activities by taking advantage of his earlier association with radiobiological laboratories. A specific activity in this field related to the establishment of new units and methods for the measurement of radiation doses in radiation and biology; a proposal by Fano and Taylor contributed to the adoption of the rad in 1953 (Ref. 113). In a related activity (1955-56), L. V. Spencer and F. H. Attix refined the theory of cavity ionization, an important improvement of our understanding of the response of cavity ionization chambers and related detectors (Refs. 255 and 257).

Extended experimental studies dealt with the back-scattering of narrow beams of gamma rays by walls of different materials and under various angles of incidence. The back-scattered radiation was analyzed with a scintillation spectrometer and then was solved for theoretically by M. J. Berger and his associates using the "Monte Carlo" method. Applications of the results of the program were quickly found in problems associated with the design and the development of submarine nuclear reactors.

Indeed, the studies by Fano and his collaborators on radiation penetration and diffusion, and on radiation interactions, have become classics in the field. Those dealing with x-ray penetration phenomena were summarized in a paper by Fano, Spencer, and Berger in Volume 338 of the Handbuch der Physik (Ref. 456).

In 1966, Fano left the Bureau to accept a professorship at the University of Chicago, and after an interim period under Dr. L. V. Spencer, Dr. M. J. Berger was appointed Chief of the Radiation Theory Section.

Spencer and Berger continued some of the on-going programs and moved in other directions. They applied the same type of exhaustive theoretical treatment to problems involved in civil defense operations, particularly problems of radiation shelters. This work began in 1955 under Fano at a time when only the crudest information was available on

radiation protection from the effects of nuclear weapons. By applying the new concepts and methods systematically to shielding problems, a body of usable shielding data for analysis of structure shielding against fallout gamma rays was developed. Further collaboration by Spencer and C. M. Eisenhauer with representatives of the Federal Civil Defence Agency and its contractors led to a methodology for the Federal programs of the 1960s, largely based on Spencer's shielding data (Refs. 666 and 669).

Radioactivity

This discussion does not cover the radioactivity programs of the radioactivity group during the period from 1927 to 1949. When it was part of Dr. Mohler's section (Atomic Physics, Radium and X-rays), it had little communication with the rest of the section. Then in 1940 the group was detached from Dr. Mohler's section and formed into a separate section. By about 1946, the section, enlarged to include neutron measurements, reported to Dr. Condon, the NBS Director, who also took on the Atomic and Molecular Physics Division. (This was the old Optics Division, with E. C. Crittenden, Chief, and L. S. Taylor, Assistant Chief, being replaced by Condon and Dr. R. D. Huntoon, respectively.)

In 1949, Condon, realizing he could not properly manage the whole Bureau and a growing division, appointed Huntoon to be chief of a slightly reorganized Division of Atomic and Radiation Physics with Taylor as assistant chief. The new division was divided into two parts--Atomic Physics, under Huntoon, and Radiation Physics, under Taylor. At the same time, the Radioactivity Section under L. F. Curtiss was divided into two parts with Curtiss as chief of a Neutron Measurement Section in the Atomic Physics Laboratory part of the division. The radioactivity part of the section was placed in the Radiation Physics Laboratory part of the division, with Taylor as acting chief while a permanent chief was sought. (As noted later, page 318, the neutron measurement programs were also transferred to the Radiation Physics Laboratory in about 1950.)

In late 1951, Dr. W. B. Mann of England accepted this appointment. Until that time, the radioactivity program had consisted of three main parts. The first was the checking and certification of sealed radium sources, such as used in medicine and industry. (This activity was begun in 1913, and although it began to taper off by 1950, the demand for the service was sufficient to keep the laboratory in operation.)

A second major program had been in preparation, calibration, and certification of radioactivity standards, especially the radionuclides produced in reactors.

This program had been initiated by L. F. Curtiss in the late 1940's and was progressing satisfactorily within the "state of the art." Then Dr. George Manov extended the program by establishing a "measurement school," wherein he gave lectures and laboratory instruction to the medical users of iodine-131, thus encouraging wider application of the new medical tool.

By 1953, the Radioactivity Section had extended the availability of radioactivity standards to include the following: radium-226, cobalt-60, strontium-90, thallium-204, radium (D+E), and radioactive ores. The Bureau also took part in a series of intercomparisons of such standards between the National Laboratories of the United States, the United Kingdom, and Canada. Of the 12 radioactive nuclides, Mann's laboratory supplied those for cobalt, iodine, phosphorus, and carbon. In addition, his section prepared radioactive solutions and ore samples used as comparative standards in other U.S. laboratories. For many of these measurements, it was necessary to develop new counting techniques and procedures.

A third extensive activity involved the measurement of air and breath samples containing radon to determine if any radium was being ingested by workers and to measure the amount of radon present in the atmosphere of uranium mines and radium plants. (This had also been started by Curtiss as an active program during the war years, but it had gradually tapered off by the mid 1950's.) Radon measurements were also used to assay uranium ores.

In addition to continuing the existing programs, Dr. Mann initiated a major program on the determination of radioactive decay schemes. At that time, there was serious disagreement between NBS, Oak Ridge, and the British laboratories in the intercomparisons of the standards for iodine-131 and phosphorus-32. New techniques, such as the development of coincidence and 4π counting by H. H. Seliger and L. M. Cavallo (Ref. 133), provided the results needed to improve this situation. In another area, a particularly pressing problem was the standardization of Strontium-90, which was beginning to be used in medical applicators and luminous dials for aircraft and other such instruments. This was predominently an instrumental research problem since the measurement of the beta rays from strontium-90/yttrium-90 was so critically dependent on geometry. Other problems involved the recently developed scintillation crystals and phototubes for which the detection

efficiency was poorly understood. Much research was thus required before such items could be adequately utilized for absolute or clinical purposes. Suitable alpha-emitting standards were also required to calibrate instruments to be used in the clean-up of any lingering radioactivity following a nuclear detonation.

While these service-type programs were proceeding, the Radioactivity Section organized some basic new ones. A solenoid beta-ray spectrometer, on which work had been started by Curtiss, was modified and completed by R. W. Hayward. This development provided a series of more reliable determinations of the decay schemes of radionuclides--information of immediate value to Katherine Way's Nuclear Data Group, which was turning out the latest information on nuclear decay parameters (Ref. 657a). The development of new high-speed counting techniques contributed materially to the success of the program.

Several studies relative to alpha-particle scattering were carried out. Techniques were developed for the investigation of alpha-gamma coincidences and alpha-gamma angular correlation, which were used in the case of ionium (thorium-230) to ascertain the spin of the first excited state of radium-226. Using the wide-belt electrostatic generator at the Department of Terrestrial Magnetism, studies were carried out on alpha-particle interactions up to 3.4 MeV in order to find the s-wave and d-wave phase shifts as a function of energy.

The elastic scattering of alpha-particles by nitrogen led to the discovery of at least two new levels in fluorine-18. The (α, p) reaction on nitrogen was also measured as a function of both angle and of incident alpha energy. Such measurements represented the first of this kind with the Van de Graaff helium ions. In addition, a magnetic separator was used to obtain monoenergetic beams of particles whose angular distribution and energy loss after passing through thin foils of various atomic number were studied.

In cooperation with the NBS Heat and Power Division, a nuclear-alignment project at very low temperatures was carried out in the cryogenics laboratory to verify nuclear magnetic resonance of radioactive nuclei near 0.01 K (Ref. 172). Also in the cryogenics laboratory, E. Ambler, R. W. Hayward, R. P. Hudson, and D. D. Hoppes participated with Professor C. S. Wu in the famous test of parity conservation in beta decay for which C. N. Yang and T. D. Lee were awarded the Nobel Prize (Refs. 369, 372 and 373).

The above represents just a few of the projects carried out in the Radioactivity Section, but they all showed the meticulous care and skill which Dr. Mann brought personally to the program. One item in particular was his development of a microcalorimeter with which the rate of energy emission of weak sources of alpha-particle radiation could be measured thermally (Ref. 214). As he explained, this was a modification of an idea proposed as far back as 1913, but had either been overlooked or was thought to be too difficult to perform. Fortunately, this latter point did not discourage Dr. Mann. The section's skill was also evidenced by a more accurate determination of the half life of carbon-14, an important contribution to nuclear data (see Ref. 537).

X Rays

This section discusses selected parts of the Bureau's x-ray activities not previously covered in earlier chapters dealing with the central core programs of x-ray protection and standards (see chs. 5, 6, and 11).

In the late 1940's, x rays were being produced by special beryllium window tubes operating at perhaps 50 kV, and having outputs ranging in the hundreds of thousands of roentgens per minute. Because radiations at these intensities could not be measured by ionization means with any high degree of certainty, other program approaches were made. There was also special concern about the radiation-induced electrical leakage in solid dielectrics such as those employed in ionization instruments which showed varying degrees of leakage under conditions of high energy or high intensity radiations.

A positive-ion accelerating tube was developed by G. Kamm for use with the 1.4 MeV generator for neutron production. By the acceleration of deuterons, high intensity monoenergetic neutrons of 2.5 MeV (using a deuterium target), and 14 MeV (using a tritium target), were obtained. To allow greater program flexibility and independence from the 1.4 MeV source, a seperate 280 kV d-c power supply was installed. This was essentially two 140 kV, reversible polarity units such as used in the 1.4 MV set. Initial studies dealt with the attenuation in water of monoenergetic fast neutrons of both of the above energies. This experiment provided a critical check of theory. The experimental conditions approached a theoretically simple situation of an isotropic point source of monoenergetic neutrons in an infinite medium. The work also included an absolute calibration of the energy response of the then-available fast neutron dosimeters. This program was replaced in 1955 by a greatly expanded line of research employing a 2 MeV Van de Graaff accelerator supplied by the AEC.

Upon discontinuance of the low-energy positive-ion accelerator part of the neutron measurement program, the voltage source was extended by Motz and Placious to 560 keV for use with an electron accelerator to produce high-intensity x rays (see photo No. 43, ch. 11). This was primarily to study the basic cross sections for the production of continuous x-ray spectra from both thick and thin targets over the range of 50 to 500 keV (Refs. 278 and 423). A logical extension of this was the study of the polarization properties of the radiation and the elastic and inelastic scattering of electrons involved in the penetration and diffusion of electrons through matter (Refs. 424, 504, 591, and 592).

Neutron measurement studies, initiated by L. F. Curtiss in 1946, were transferred temporarily to Wyckoff's X-Ray Section when Curtiss left the division in 1949. Among his staff, which was distributed among several sections, was J. A. deJuren, who with G. Kamm from the Atomic Physics Laboratory, became the principal investigator in the neutron studies under Wyckoff.

Some theoretical and experimental work was carried out on the depth doses for electrons of energies in the range of 500 to 1,400 keV. The agreement of NBS experimental work with that by others lent confidence to the results, although there was considerable initial disagreement with the theoretical treatment. This work was carried out in close cooperation with Fano's nuclear physics group.

As noted earlier, it was in the late 1940's that the light response produced in anthracene and other crystals by incident electrons and positrons provided an important measurement tool (see p. 181). Light output was studied as a function of the crystal thickness and the incident electron or positron energy over a range of 0.5 to 10 MeV.

Dr. J. W. Motz carried out experimental studies to determine x-ray spectra in absolute intensity units as a function of the initial electron energy in the range from 50 to 100 keV, different target materials and thicknesses, and the angle of the x-ray emission with respect to the incident electron direction. For this Motz utilized the magnetic Compton Spectrometer that he had developed in connection with Operation Greenhouse (see p. 180, also Refs. 155, 183 and photo No. 48). Also, measurements of thin target x rays were made with a total absorption scintillation spectrometer at 500 and 1000 keV. Polarization studies were also carried out.

In cooperation with the neutron attenuation and dosimetry group, a new Van de Graaff accelerator was obtained for making cross-section measurements.

NOTE: In closing the discussion of the X-Ray Section, an interesting incident is recalled. Before the Manhattan District operation was started, the Bureau had the only x-ray program in Washington. Therefore, it was no surprise when, in 1941, the Secret Service asked for assistance in the inspection of mail and other packages addressed to the White House. Wyckoff, who had just joined the staff, was dispatched to study the problem. In a nearby garage, they had set up an old piece of surplus x-ray equipment and, without any other protection, were examining each package with a small hand fluoroscope--a type that had been declared unsafe 20 years before. Wyckoff helped them obtain a modern inspectoscope for these examinations. This was just one of the many calls for help that were received by the X-Ray Section.

Betatron Research

The equipment and facilities for the Betatron Laboratory were unique compared to those of some 20 other laboratories in the country at that time. The 50-MeV machine was chosen for its high output at maximum operating energy and workable outputs when operating at 5 MeV. Likewise, the 180-MeV machine was capable of high output as well as workable yields in the range as low as 50 MeV. The basic justification for this expanded NBS program lay in the potential applications of high-energy radiations in therapeutic radiology. But before this could be considered, systems of radiation measurement and protection had to be devised.

The Betatron Section program under Dr. H. W. Koch was divided roughly into three categories: radiation measurement, nuclear physics research, and instrumentation. By the time the 50-MeV betatron had been in operation for about a year, an addition to the Betatron Laboratory building was required for the betatron program in order to provide operating space for the 180-MeV synchrotron, which was expected to be in operation by about the end of 1953.

Funding for the Betatron Section was a continuing problem, dating back to the original appropriation for the building. The Congressional Committee had denied personnel funding until the building and equipment installation was completed. Yet year after year, those funds were not provided.

Meanwhile, with such a very large investment in the laboratory, it was necessary to divert funds from other programs. Because the basic NBS program was centered around Wyckoff's programs, virtually all of the direct appropriated funds were tied up in that one section. The other sections had smaller amounts of direct funds and were very heavily dependent upon transferred funds, which during the 1950's were relatively accessible. This nevertheless caused some internal difficulties and led to personal frictions.

The "bread and butter" program, which was the main justification for the Betatron Laboratory, centered around potential medical measurements. Various types of ionization chambers had been made and used by numerous experimenters for measuring the radiation from a betatron, and a certain degree of informal standardization had been established using a Victoreen 25 R chamber surrounded by lead or lucite. Although investigations by the Betatron Section yielded data that correlated well with the results obtained by the various dosage measuring systems, none of the methods was considered to be fully satisfactory.

As part of the measurement program, a four-crystal scintillation spectrometer for measuring x-ray energies up to 12 MeV was designed and calibrated using boron (p,γ) gamma rays produced by the ion tube in the X-Ray Section (see p. 310). This spectrometer had an energy resolution of 6 percent at 4.45 MeV and 15 percent at 11.6 MeV.

Radiation measurements of exposure in roentgens at energies above 5 million electron volts were difficult to interpret due to the fact that x-ray energy is imparted to material in a two-step process: conversion of x-ray energy to kinetic energy of electrons and positrons in a material, and deposition of the kinetic energy of the electrons and positrons in the material. (The deposition may be by means other than ionization.) Energy present as ionization generally represents energy "absorbed." Since the secondary particles have long ranges, the two steps occur at different locations, and the attenuation of the primary x-ray beam could be appreciable in the intervening distance. Therefore, the interpretation of the ionization chamber readings becomes more difficult at these high energies.

In order to avoid these difficulties, a calorimeter was constructed by McIlhinney, Zendle, and Domen to measure the total energy absorbed in a block of lead placed in the x-ray beam (Refs. 328 and 335). A comparison of those measurements with the more commonly used commercial ionization chambers provided a calibration in the energy range from 5 to 180 MeV.

Other experiments by Koch and his associates included depth-dose studies in water using x rays up to 46 MeV, thus providing information on the spatial distribution of the energy absorbed (Ref. 337). This was fundamental to the proper utilization of these very high-energy x rays for therapeutic applications.

In cooperation with Wyckoff's section, studies were made of the attenuation of x rays in concrete barriers up to thicknesses of 8 feet for energies from 5 to 46 MeV. This information provided a basis for recommendations for personnel protection around similar installations and for comparison with theoretical predictions. The results of the various NBS studies were included in NBS Handbook 55, entitled "Personnel Protection for Betatron, Synchrotron radiations up to 100 million Electron Volts," which was prepared by the NCRP Committee on "Radiation Protection for Electrons, Gamma Rays and X rays above 2 Million Volts" (Ref. 625).

Although the section did perform the above studies of the "bread and butter" type, its main interest and emphasis was in the general field of nuclear physics research for which the facilities were highly adaptable. Two such studies carried out with the support of the AEC were "Neutron Field Studies," which involved the measurement of the total number of neutrons resulting from (γ, n) reactions as a function of the x-ray energy (Refs. 404 and 405), and "Neutron Energy Distribution" which dealt with the experimental measurements of the energies of neutrons during (γ, n) and $(\gamma,$ fission) reactions. Another project, entitled "Nuclear Elastic Scattering of Photons," was carried out by Fuller and Hayward under the sponsorship of the Air Research and Development Command (Refs. 406 and 407).

While these studies were in progress, an electromagnetic extractor was designed and constructed to remove the electron beam from the 50-MeV Betatron (Ref. 239). The device was highly successful, allowing electrons with energies up to 20 MeV to be extracted with an efficiency of about 60 percent. The emergent electron beam was first used in the neutron yield studies mentioned above. From this point on, the betatron was devoted entirely to nuclear physics research rather than medical dosimetry purposes for which it was originally justified.

The x-ray beams from the betatron and synchrotron comprised an entire spectrum of photon energies ranging from the kilo-electron-volt region up to the kinetic energy of the electrons that produced the x rays. To solve the problem of using available inefficient detectors for such a multitude of photon energies, a crystal spectrometer was developed with a photon detection efficiency exceeding 80 percent and an energy resolution of 11 percent up

to 50 MeV (Ref. 240). The heart of the spectrometer was a large sodium-iodide crystal in which the energy dissipation of an individual x-ray photon produced a visible light pulse which could be detected and measured by phototubes. The light pulse magnitude was proportional to the total energy absorbed in the crystal.

Detailed knowledge of the interaction of high-energy x rays with matter requires an examination of the characteristics of the x-ray source, the x rays, and their macroscopic and microscopic interactions with atoms and nuclei. This complete examination is required especially for nuclear physics programs, but it is also useful for a thorough understanding of all other applications of high-energy radiations. Such was the purpose of a number of programs carried out by section staff members in the following areas:

1. Photo Nuclear Physics. The high-energy x-rays from the betatron and synchrotron were used to study the interaction with the atomic nucleus.

a. Photo Neutron Cross Sections. A detailed examination of the shape of the photoneutron cross sections for a wide range of target materials; Fuller and Hayward (Refs. 551 and 552).

b. Photo Proton Cross Sections. Measurement of the photo proton cross sections of carbon in which the residual boron nucleus is left in the ground state; Penner and Leiss (Ref. 462b).

c. Photo Meson Cross Sections. Measurements of the angular distributions of neutral mesons, photo produced by 170 MeV Bremsstrahlung from the synchrotron for carbon, aluminium, copper, cadmium, and lead; Schrack, Penner, and Leiss (Ref. 510).

d. Nuclear Elastic Scattering Cross Sections. Measurements of the differential cross sections for the elastic scattering of x rays by the nucleus at energies ranging from 4 to 40 MeV and for a wide range of atomic numbers; Fuller and Hayward (Refs. 238, 242 and 552).

e. Total Nuclear Cross Sections. Measurements of the total nuclear cross sections between x-ray energies of 15 and 80 MeV using long absorbers of carbon, water, and aluminium together with a scintillation spectrometer capable of resolving the total nuclear cross sections in the spectra transmitted by the absorbers; J. Wyckoff and Koch (Refs. 287 and 519b).

2. Radiation Physics.

a. Pair-Production Cross Sections. The evaluation of the total attenuation cross sections for 60 MeV x rays in carbon, water, and aluminium using the long absorber technique and the high-resolution spectrometer; Nelms (Ref. 558).

b. Bremsstrahlung Cross Sections. Experimental determinations of the cross sections for the production of x rays by the bremsstrahlung process using the resonance fluorescence of the narrow 15.1 MeV level in carbon; Koch and Motz (Ref. 462a).

3. Measurement Techniques and Standards.

a. Calorimeter for Measuring X-Ray Beam Energy. The total integrated x-ray energy incident on a patient or experimental arrangement is a physical quantity which can be measured accurately. Both the calorimeter and a scintillation spectrometer were used to measure this quantity; Pruitt and Domen (Ref. 561).

b. Absorbed Dose Calorimeter. An absorbed dose calorimeter was developed in connection with an investigation of the feasibility of using it to calibrate small volume ionization chambers; Domen et al (Refs. 328 and 335).

c. Absolute Calibrations of Accelerator Energy Scales . An instrument was developed with which photon energy thresholds for photoneutrons, photoprotrons, and photomesons can be determined. These are important for providing calibrations to the energy scales for electron accelerators.

d. X-Ray Scintillation Spectrometer. A sodium iodide spectrometer was developed with a resolution of 2.5 percent at 17.6 MeV, which, though comparable to that of a pair spectrometer, had a detection efficiency several orders of magnitude better.

Nucleonic Instrumentation

The Nucleonic Instrumentation Section was established as a part of the 1949 reorganization noted above with Wyckoff as Acting Chief. Soon after its formation, from

part of the Radioactivity Section, Harold A. Thomas was made chief, to be succeeded in 1951 by Louis Costrell.

Created to serve primarily as a complementary unit to all other sections of the Radiation Physics Laboratory, the section was also encouraged to engage in developments of its own choice, funds and time permitting. In both areas, Costrell developed an enviable reputation. For about the first three years of its existence, the section, augmented by staff members from other sections in the laboratory, was heavily engaged in weapons tests programs for the AEC and the Armed Forces Special Weapons Project. It was for the latter that outstanding successes were achieved. One such achievement involved the measurement and telemetering of information in connection with weapons tests, wherein sensors or radiation detectors could be located at any desired point in the test area and the information transmitted over field lines to a convenient recording center. Costrell later carried the work even further by making it possible to sit at a desk in Washington and record the information from tests taking place in Nevada, with monitoring stations responding automatically when dialed over commercial telephone lines (Ref. 395). While the radiation physics laboratory stood ready to engage in emergency programs when warranted, such activities did have a disrupting influence on other programs.

Perhaps the more notable Costrell achievement was the subsequent standardization of electronic modules, which was adopted by the American National Standards Institute and the International Electrotechnical Commission. The standardized modules are now in very extensive use worldwide (Costrell, 1974).

The section was divided into four general working areas with flexibility to meet changing circumstances. The primary aim of one was the development of skills and facilities in the field of very high-speed counting techniques (10^{-9} seconds or better) (Ref. 348). These techniques had broad applications, especially in the Betatron and Radioactivity Sections. An early problem was the high speed gammagamma coincidence measurement of large radioactive sources, whose solution involved the construction of special equipment applicable to specific problems in the other sections. It was engaged in varied and complicated development problems to assist the research programs of other sections. Among these mid-1950 projects were the following:

a) Integrator for energy control of synchrotrons.
b) Shaped injector circuit for synchrotron.
c) Orbital contractor circuits.
d) Pulse height analyzers and associated problems (Ref. 399).
e) Betatron yield stabilizer.
f) Magnetic field regulators.
g) Electron beam energy control for Betatron.
h) High dynamic-range amplifiers.
i) Remote control radiation intensity recording devices for weapons tests programs.

Items a-g above were carried out primarily for, and in close collaboration with, the staff of the Betatron Section.

A second group was, by training and experience, especially well adapted to the development of various types of radiation detection instruments for gamma rays, electrons, and, to a lesser extent, neutrons. Working in conjunction with Smith's radiological equipment section, the group had access to unusual facilities for an overall radiation instrument development program--a program enhanced by close collaboration with a staff having many years of experience with the radiation characteristics and requirements of detectors such as ionization chambers and counters. To meet a need in connection with radiation sources used for therapy purposes, measurements were made of scattered radiation from ^{60}Co and ^{137}Cs sources and the results "unfolded" to accurately determine the energy distribution (Refs. 546 and 547).

The third group, working with the first two, was concerned with the design and construction of less complicated electronic circuits and equipment for radiation detection and measurement as required by the Radiation Physics Laboratory. This included such things as quenching circuits, gates, preamplifiers, ionization gauges, special scalers, and counting circuits. The fourth area of work was primarily the servicing and repairing of general electronic equipment for the other sections.

The Nucleonic Instrumentation Section was one of the most difficult sections to keep staffed. Almost anyone with electronic experience could be attracted to industry by

salaries ranging up to double those that could be paid by the Government. Many of the offers came from companies working on "cost plus" contracts for the Government.

Radiological Equipment

In programs carried out for the Armed Services during the early part of the war, certain limitations in the available wartime x-ray equipment were revealed. After the war, the Bureau was asked to extend its program to the development of a new family of military hospital and field x-ray equipment. Such equipment was to satisfy not only normal operational requirements, but also tropical and arctic conditions, including transportation at high altitudes and low temperatures.

Working in close collaboration with the military services, NBS developed a 15 milliampere tube and transformer unit for field use and additional 30 and 100 milliampere diagnostic equipments, together with housings and cables, to meet new requirements. A lightweight x-ray table was designed to replace the existing one. New equipment was developed which could be knocked down into rugged components that could be shipped safely and even air dropped.

In the early 1950's, there were as many types of high-voltage cable terminations as there were manufacturers, making it impossible to exchange cables and other devices. This was an obvious military disadvantage and the Bureau solved it by the standardization of both high-voltage cables and terminals.

In another program, for the Veterans Administration, the Bureau studied all types of diagnostic x-ray equipment and accessories employed in general hospital use and drew up specifications jointly with the VA. Following acceptance of the specifications by industry and the medical profession, samples of purchased equipment were sent to the Bureau for checking. Factors not previously covered by the pre-war specifications were studied to determine their effect on the general functioning of the equipment and whether they should be included in future specifications. Such studies included complete electrical characteristics tests of transformers and generating equipment, operation of the x-ray tube including cooling and radiation output, functioning of Bucky diaphragms, spot film devices, stereo devices, cassettes, tables, timers, and control units.

The Radiological Equipment Section had the responsibility for the checking, testing, and calibration of dosimeters and portable radiation measuring equipment. For these services, the laboratory maintained two principal standards at the outset. The first was the primary guarded-field free-air standardization chamber similar to the type used in the international comparisons in 1931. It also had the free-air pressure ionization chamber for use up to 1.5 MeV. This chamber, the only one of its kind, had been calibrated against the low-voltage primary standard where the ranges overlapped. In the lower energy range (5 to 50 kV), the Bureau maintained special guarded-field free-air chambers designed to minimize air absorption and provide exact correction determinations. These were also available for making standard measurements at low energies for dosages as high as 10^6 roentgens per minute. The research and development for the design and construction of the equipments utilized by the laboratory were carried out mainly by Wyckoff's X-Ray Section.

Secondary standards, usually in the form of cavity or enclosed chambers, had been under continuous study in the laboratory for nearly 25 years. Out of these studies came three sets of specially constructed and calibrated secondary standards for use in the radiac programs of the three military services. The design factors of the enclosed ionization chambers that influenced the energy and dose response were also studied.

A radiac instrument calibrator, using cobalt-60 as the radiation source, was developed by the laboratory particularly for military use, and served as the prototype for commercial models produced for the Navy and the Signal Corps. Some 50 such devices were submitted to the laboratory for pre-production tests and calibration.

The studies and calibration of radiation detecting and measuring instruments and devices for energy dependence and dose or dose-rate response were carried out in conjunction with the military services, AEC, and the Civil Defense Administration. Close collaboration with the manufacturers during their design, research, and testing stages allowed many NBS modifications and improvements to be incorporated into the radiation measurement devices. Radiation instruments tested included ionization chambers, G.M. counters, scintillation counters, chemical solutions, and radio-sensitive glasses.

As mentioned earlier, the three military agencies had individually requested the help of NBS in their extensive radiac procurement programs, which were ultimately integrated into a single program for all Government agencies (see p. 180).

Environmental tests were also performed, including immersion, humidity, shock, vibration, high and low temperatures, and medical and electrical properties of the instruments. At that time, this laboratory was probably the most completely staffed and equipped unit for that type of work.

Studies of ancillary equipment used in conjunction with x-ray installations included models of film dryers, x-ray tubes, x-ray generators, field x-ray tables, and gasoline-electric power units. A "dummy load" was developed together with a means of combining the 100 mA transformer with the field x-ray table.

It became apparent that more information was needed regarding the protection of operators. To meet this need, simulated operating conditions were set up in the laboratory, using movable walls and rice-filled dummies into which the instruments could be inserted (see photo No. 49). With this facility, it was possible for Smith and Brooks to make scattered radiation measurements under realistic conditions and investigate the shielding properties of special devices such as lead-glass clothing fabric, lead-rubber aprons, and lead glass. (Ref. 512).

In addition to the instrumental tests, the laboratory conducted a broad program in radiation sensitivity of photographic emulsions, mainly for the Atomic Energy Commission and the Signal Corps (Ref. 204). This program, started in 1949 by Dr. Margarete Ehrlich, was expanded with the assistance of W. L. McLaughlin in 1951 to include programs on chemical and solid-state dosimetry. As experts on the utilization of the film badges, the laboratory staff participated in four weapons test operations and handled the entire film radiation measurement program for Operation Greenhouse. In connection with this program, Ehrlich developed a new type of compensating film badge container with a flat energy response over a wider energy range than theretofore possible (see p. 182). This was an important contribution to subsequent radiation sensitometry programs leading to the development of optimum processing and calibration procedures for energy--independent film dosimeters (Ref. 385). The effects of secondary emission from the film package, which were of critical importance in the sensitometrical use, were also studied. Special emulsions of the type used in polaroid film were important as well as reversal effects due to excessive radiation exposure for both polaroid and conventional emulsions.

Studies were also made of the effects of variations of temperature and relative humidity both during exposure and during storage of the film before processing. The results were important in weapons tests as well as for evaluating personnel dosimetry procedures in which films were allowed to remain in their holders over periods of up to 3 months before being processed and read (Refs. 192 and 528).

Calculations were carried out to determine the relative response of photographic emulsions to x- and gamma-ray photons, to electrons, and to neutrons, based on the energy and the number of quanta truly absorbed in the emulsion. These data were compared with experimental results.

Most of Ehrlich's subsequent photo sensitometric programs were related to AEC Nevada above-ground testing. During the early years of testing, the AEC shipped all exposed film to NBS for processing and interpretation. This allowed first hand observation of difficulties with the system in the field. One of the observed difficulties was that, for some shots, and for exposure levels for which both the sensitive and the insensitive films in a given badge showed readings in their respective useful range, the exposure interpretations (by means of laboratory calibration curves) from the two films differed by as much as a factor of two. Later, when the processing and interpretation were performed by a contractor, another puzzling phenomenon was observed. Film exposed in the field showed considerably higher density than the maximum that could be reached during calibration. These difficulties with the field results led to interesting findings.

One such finding was that the ratio of fast neutron-to-photon response was roughly inversely proportional to the average grain size of the silver halide in a particular emulsion (Ref. 487). The thermal neutron sensitivity of the several films used was about the same as that for 3-MeV neutrons. The difference in grain size between the two films used in the field badge was such that the difference in the exposure interpretation of the field results could be explained, at least qualitatively, by the presence of fast neutrons.

Another study concerned the rate dependence of the response of film exposed to high-energy photons. When the AEC consulted Ehrlich on the contractor's problem noted above, she found that, for the sake of convenience, the contractor was using the same (weaker) developer for both visible-light and x-ray film processing. Subsequent investigations with weak ("surface") development of x-ray films showed no rate dependence in the ascending branch of the density-versus-exposure curves (Ref. 350). However, rate dependence became apparent in the onset of reversal (i.e., the maximum density reached) and in the extent of

reversal, which was particularly pronounced with surface development (Ref. 351). This phenomenon fully explained the contractor's difficulties. It also aroused the interest of both national and international photographic scientists and led to further challenging problems in the photographic field.

One of the challenging photographic problems that was pursued further was that of the effect of different types of sequence exposures. Extensive laboratory studies of the photographic response to successive exposures of different types included exposures of a given film sample to x rays and gamma rays of different photon energies and intensitities, to gamma radiation and visible light, or to visible and infrared radiation. It was shown that it is possible to predict the gross photographic behavior as a result of successive exposures to two types of radiation (Ref. 527).

To meet military and civil defense needs, there was considerable interest in the use of solid state devices for measuring ionizing radiations. These had to be of rugged construction and produced in a variety of sizes, be simple to operate and have a long shelf life. They also had to have quick response and show very good reproducibility for the measured radiation effect.

An investigation of the possibility of using silicon solar cells for dosimetry of ionizing radiation indicated that these photovoltaic cells of the P-N junction type showed a favorable response to x rays with regard to proportionality to exposure-dose, response time, and dependence on photon energy. Measurements also established the influence of radiation damage on the photo responses of such cells at high exposure-dose rates of high-energy radiation.

Radiation-induced chemical reactions were finding increasing use for the quantitative measurement of ionizing radiation. Different systems had shown high stability and permitted a high degree of accuracy in measuring the product of the radiation chemical reaction. The chemical dosimeters, which allowed the direct measurement of absorbed energy, were usually made of aqueous solutions with absorption characteristics similar to those of body tissue and body fluids.

An NBS study of the most widely used chemical dosimetry system--the ferrous sulfate dosimeter (Fricke dosimeter)--showed that the energy dependence of G-values (ferric ion yield per 100-eV absorbed energy) for medium energy x rays were in good agreement with those obtained with high-energy x- and gamma rays. An investigation of the spectrophotometric method of determining the radiation-produced ferric ion yield showed that the sensitivity of the method could be increased by measuring the absorbance of the irradiated ferrous sulfate solution at a wavelength of 224 mμ, as in the usual standard method. A further advantage was the smaller temperature dependence and less acidity of the absorbance at 224 mμ.

When a portion of the radioactivity section was transferred to the Radiation Laboratory in 1949 as a part of Dr. Mann's program, he designed a special building in 1954 to house all operations associated with the handling of radium preparations. However, before the building was constructed, new requirements had developed and a decision was made to place this service in the Radiological Equipment Section under Smith. A 1 story building was constructed in 1950 to meet three major needs. The first was to provide a radium calibration and handling system which, for the first time, was specifically designed to minimize problems of contamination and maximize the capability for radioactive "sanitation," To meet the latter, most of the equipment and most of that portion of the laboratory was lined with stainless steel (see photo No. 50).

The second requirement was for instrument calibration ranges using radiation sources of cobalt-60 and cesium-137 of a few curies each. The sources were arranged in pits in the floor of a large square room with a ceiling approximately 20 feet high, and constructed of low atomic number materials so as to minimize scattering from the radiation beam. A vertical calibrator track allowed movement of the instrument being calibrated over a range of several meters by varying the radiation field strength. Initially, the radium calibration system was operated by Tom Davenport and the equipment calibrator range by Tom Loftus.

The third part of the facility consisted of sources of cobalt-60, totalling 50,000 curies, arranged in a cylindrical configuration so that ionization chambers or other radiation detectors could be located on the axis of the array or in other positions relative to the sources. The total source was made up of 50 sealed stainless steel tubes each containing a thousand curies of cobalt-60. The technique of welding the tubes to insure complete sealing was worked out by the Bureau's Instruments Shops under Frank Brown. To provide adequate shielding for the workers, the entire radioactive unit was located at the bottom of a grill-covered pit of water about 6 feet square and 15 feet deep.

When the radioactivity portion of Curtiss' original Radioactivity Section was transferred in 1949 to the new Radioactivity Section in the Radiation Physics Laboratory, the neutron measurements program continued under Curtiss in the Atomic Physics Laboratory until about 1953, when he was given a new assignment. At that time, the neutron work was also transferred to the Radiation Physics Laboratory, first under Wyckoff and later, as a new section, under Dr. R. S. Caswell, who assumed direction of the ongoing work and initiated a number of new programs which are summarized below.

For the national standard of neutron emission rate, a Ra-Be(γ,Σ) source was adopted. For a thermal neutron flux density standard, an assembly which moderated neutrons produced by two Ra-Be(α,Σ) sources was used to produce an arbitrary thermal neutron flux density which could be measured. Under the leadership of Dr. J. deJuren, the national neutron standard source had been calibrated by two methods: by the activation of calibrated foils in a water bath (Ref. 275), and by activation of a manganous sulfate solution in water (Ref. 276). After the laboratory shift, a new calibration was carried out (1961) using an ^{124}Sb-Be photoneutron source in heavy water (Ref. 595). This method eliminated uncertainties due to the ratio of the manganese activation cross section and the hydrogen thermal neutron disappearance cross section, essentially by eliminating most of the hydrogen. The national standard source was then compared to the absolutely-calibrated ^{124}Sb-Be source by bath activation, a precise but relative method.

The standard thermal neutron flux was recalibrated by a method based on the thermal neutron activation cross section of gold, using $4\pi(\beta\text{-}\gamma)$ coincidence counting (Ref. 538). The new calibration, together with a previous calibration based on the thermal neutron reaction cross section of boron, provided a flux density known to about 1.5 percent (standard error). Intercomparison of that flux density with the absolutely calibrated fluxes of other laboratories in the United States and in the world was carried out over a period of several years, notably through an intercomparison held under the auspices of the International Bureau of Weights and Measures (BIPM).

Neutron sources calibrated by NBS were used as standards for fast neutron flux density measurement, cross section measurements, fast neutron dosimetry, and to produce known thermal neutron fluxes in moderators. Gold foils activated in the standard neutron flux were used chiefly by groups working with nuclear reactors to check or to establish their own thermal neutron flux measurements. Neutron dosimetry measurements were made with homogeneous proportional counter detectors (polyethylene-ethylene) which distinguished between neutrons and gamma rays. The neutron dosimeter (gamma-insensitive) was calibrated in known fluxes of monoenergetic neutrons from the Van de Graaff accelerator.

This was part of a new installation at the east end of the main bay in the high-voltage laboratory. The accelerator was located on a platform close to the wall of the building. From that point, the beam could be taken out horizontally to a "low-scatter" room built outside and on the end of the main laboratory building. The gamma-ray dosimeter (neutron-insensitive) was calibrated in x-ray and gamma-ray fields of known exposure. The pair of instruments could then be used in mixed gamma-ray and neutron radiation fields to separate out the neutron and gamma-ray doses.

In connection with studies of neutron penetration, measurements were made of the thermal neutron distribution, the indium resonance age, and the fast neutron distribution about monoenergetic neutron sources in ordinary water and in heavy water (Refs. 359, 433, 541).

Experiments on the fast neutron dose distributions from 14 MeV (d+t) and 4 MeV (d+d) neutrons in water were important as fundamental experiments relevant to neutron radiation protection which could be compared accurately to theoretical calculations due to the simple geometry used. The measurements of thermal neutron distributions and indium resonance age were used to check theories applied to nuclear reactor core design. Experimental measurements of neutron elastic and inelastic cross sections and angular distributions by the time-of-flight method were carried out on the new 2-MeV Van de Graaff accelerator. Initial measurements for carbon and calcium were made at 14 MeV. The beginning of the neutron flight was indicated by detection of the alpha particle from the ^{3}(d,n)^{4}He reaction which produces the neutron. The scattered neutron was detected in a large plastic scintillator after a flight from the scattering sample of 1.2 meters. Resolution time of about 1 nanosecond was obtained. Internal pulsing of the Van de Graaff accelerator later served to reduce background and permit measurements with good time resolution (about 3 nanoseconds) detecting ^{3}He particles from the ^{3}H(p,n)^{3}He reaction at 1 MeV and below.

An alignment experiment for ^{165}Ho was carried out using 14-MeV neutrons from the Van de Graaff accelerator. This experiment showed the change in cross section when all of these

ellipsoidal shaped nuclei were lined up in one direction at low temperature as compared to random orientation.

Theoretical calculations using the nuclear optical model were made of neutron cross sections and polarization. These calculations gave results which could be compared to the elastic scattering measurements and were useful for neutron penetration calculations. Calculations by methods such as the moments method, developed in the Radiation Theory Section, were of considerable importance.

Radiation Instruments Branch (USAEC)

Originally, as an extension of the AEC Division of Production in Oak Ridge, Tennessee, the branch's primary function was the coordination and development of commercial sources of supply for radiation dosimetry and measurement instruments and components needed by the atomic energy program and users of radioisotopes and radiation sources. The many different types of instruments needed were categorized and the required performance specifications were prepared. Using these specifications, small production engineering contracts were let for the construction of prototypes for testing and evaluation. For several years, the branch centrally procured and stockpiled radiation instruments for atomic energy projects to assure reliable and commercial availability of such items as geiger counters, scalers, survey meters, pocket dosimeters, and film badges.

The branch served as an information channel between industry and the atomic energy program as to what instruments were needed and what their performance characteristics should be. This function was enhanced by frequent workshops and symposia which proved especially valuable in the development and eventual commercialization of scintillation counters and phosphors.

In early 1950 the Radiation Instruments Branch, headed by R. L. Butenhoff, was moved from Oak Ridge to NBS, where it was housed on the fifth floor of the High-Voltage Laboratory until 1959. This proved to be a very fortunate arrangement. In addition to having full access to all NBS laboratory and shop services and facilities, the branch was treated organizationally as a section of the NBS division headed by Taylor, who 2 years earlier was instrumental in organizing the USAEC biophysics program with which the branch was associated.

At NBS, the Radiation Instruments Branch continued to coordinate the commission-wide radiation instrumentation requirements and development activities. It assisted in establishing sources of supply for instruments, not only for the AEC, but for research and biomedical investigators in universities and other research institutes, for radioactive-ore prospectors, and for civil defense monitoring teams. A radiation instrument catalog was prepared and kept up-to-date by the group. Working closely with NBS, the branch's program to develop radiation instrument performance specifications, its prototype measurement and testing program, and its close ties with industrial research groups played a significant role in the development and availability of improved radiation instruments and equipment.

Beginning in its Oak Ridge days, the branch had been active in emergency monitoring (civil defense) and in atomic weapons test operations. Following its move to NBS, the branch placed increased emphasis on these activities with participation in Pacific tests and at Nevada as a part of the Civil Effects Test Group. The efforts were conducted in close cooperation with the Radiation Physics Laboratory. Of special note were the film-dosimetry program organized by G. Ehrlich and W. McLaughlin, and the development of on-site and off-site radiological monitoring telemetry systems by L. Costrell et al. R. Johnston, the branch project officer who worked with Costrell on the latter project, was later given the Flemming Award for his outstanding accomplishments. In 1959, the branch, then headed by R. Johnston, moved to the newly completed USAEC headquarters buildings in Germantown, MD. However, the close ties established during its 10-year residence as part of the NBS family continued to exist.

Health and Safety

Immediately after the war, at a time when NBS was conducting a variety of research projects for the Manhattan District, Dr. Mohler was designated as Program Coordinator between the AEC and the NBS. When Mohler became completely immersed in his mass spectrometry programs, Taylor was named in about 1950 to succeed him as AEC Coordinator.

In addition to coordinating the research projects between AEC and NBS, the coordinator was responsible for the handling of classified documents and for assuring AEC security at NBS. He was also responsible for the health and safety of all operations involving ionizing

radiations at the Bureau, including not only those using radioactive materials for the AEC, but also accelerator sources and natural radioactivity.

As it turned out, Mrs. Carin Thew, a staff assistant, had developed sufficient experience to carry out all but the most technical aspects of the program.

To operate the safety program, a Health Physics Group was organized under Taylor with Dr. A. Schwebel from the Radioactivity Section in charge. The health physics laboratory's main objective was Bureau-wide maintenance of good practices in handling of ionizing radiations and radioisotopes. This included the monitoring of personnel and machines that produced ionizing radiations such as x-ray machines, x-ray spectrometers, cyclotrons, betatrons, and Van de Graaff generators. Also monitored were all laboratories in which radioisotopes were used in any way, which sometimes required bio-assays for such nuclides as tritium and uranium, and the development of new techniques to detect, for example, the presence of strontium-90 activity. Services were available to anyone in the Bureau planning to install equipment or laboratories or needing advice on shielding, type of equipment, handling of waste, and other related safety problems.

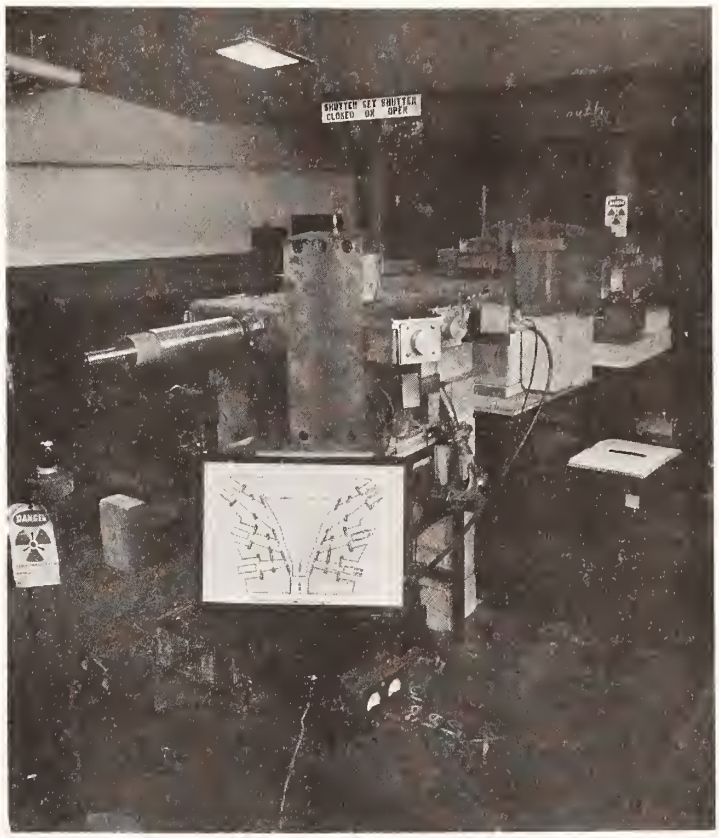

Photo No. 48. Compton magnetic spectrometer for measuring thick-target x-ray spectra (1955).

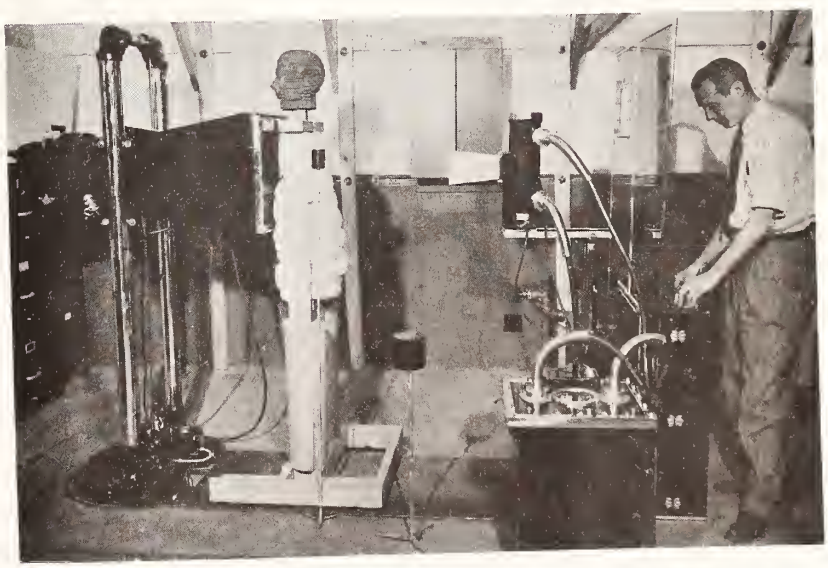

Photo No. 49. Test of photo-fluorographic system for chest x-ray examinations using a rice dummy. J. R. Brooks (1950).

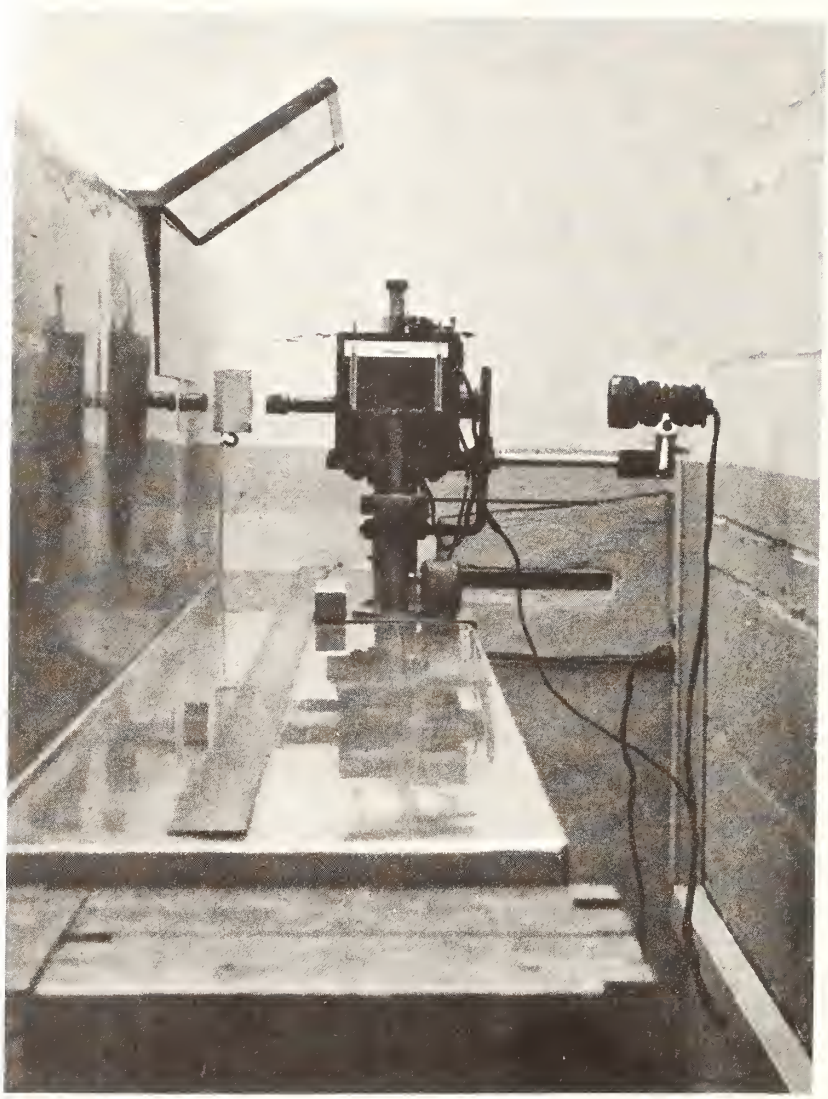

Photo No. 50. Gold-leaf electroscope used in radium testing (1925 to present). (1950 installation)

CHAPTER 17. INTERACTIONS BETWEEN NBS AND RSNA (1949-1955)

This chapter deals generally with the question of membership in the RSNA for so-called "Allied Scientists" and the instigation of a new program series by the RSNA Physics Committee of which Taylor was the chairman. Following the war years, the enormous expansion in the development of artificial radioactivity and high-energy radiations with their biophysical applications led to a relatively large increase in the number of scientists whose interest impinged on the activities of the radiological profession. Among these were physicists, biologists, chemists, and engineers.

In the 1920's, the Radiological Society of North America began to extend "Associate Membership" to non-medically trained individuals and to acknowledge the strong role they played in the technical affairs of the society. However, by the late 1940's and early 1950's, it was becoming more and more obvious to these individuals that they were, in a sense, second-order citizens as far as the radiologist was concerned. Their participation in radiological matters was welcomed but full membership was strongly resisted by the radiological organizations. The one exception was the American Radium Society, as evidenced by its 1955 physicist president, Dr. Edith Quimby.

In England, the British Institute of Radiology not only offered full membership to medical and non-medical people, but recognized an unwritten rule that every 3 or 4 years the president should be a radiological physicist or some other non-medical individual. Finally the feeling among U.S. "allied scientists" reached the point that full membership was imperative if the close bonds between them and radiology were to survive. When this matter was brought before the Physics Committee in about 1949, Chairman Taylor agreed to spearhead an effort to achieve full membership.

The matter was taken up with the RSNA President in the fall of 1951, and on December 5th an informal committee consisting of Edith Quimby, Marshall Brucer, Lauriston Taylor, Robert Stone, Ira Lockwood, and Eugene Pendergrass met to discuss the problem. Surprisingly, there was no great difficulty in reaching agreement on a number of basic points and, after a thorough review by the members, the following items emerged in a letter from Taylor to Eugene Pendergrass:

January 8, 1952

I have looked over your minutes of the Chicago meeting of December 5. While you have covered the essence of the discussions, I am left with the feeling that the emphasis does not quite reflect the feelings as expressed in our informal group. The main points were:

1. It was pointed out that because border-field scientists could not be members of the Radiological Society of North America, there was an increasing tendency for them to draw away. It was recognized that this was not desirable.
2. It was proposed that means be found whereby border-field scientists could become full members of the RSNA and hence participate more actively in the Society's over-all program and objectives.
3. To safeguard the medical dominance within the Society, border-field membership should be limited to 20% of the total membership.
4. To encourage and promote greater border-field participation in meetings, it was proposed to enlarge the scope of the meetings to include a larger proportion of border-field papers and refresher courses.
5. Because of space difficulties at the next meeting in Cincinnati, it was suggested that parallel morning sessions be held at a nearby hotel. These sessions would include papers on radiation physics, radio-biology and specialized therapeutic topics, etc.
6. Selected review papers, omitting most technical detail, would be given at the joint afternoon sessions.
7. If the above suggestions are approved by the Board of Directors, it was suggested that L. S. Taylor work with the By-laws Committee to assist in the implementation of the proposals.

While I don't think you meant it quite as it sounds, any such extended membership should not be considered as an "allied group". Their activities and programs should blend in with the others and I believe this should be reflected in pertinent committee responsibilities. For example, the main program committee should include the border-field activities, possibly by sub-committee action, but certainly not by separate committee. The entire program planning should be predominantly in the hands of radiologists. I believe we all felt rather strongly that no steps should be taken which have any real possibility of weakening the medical-radiological attributes of the Society. All of this can be done without any appearance of segregating the Society membership into two distinct groups.

There would automatically be some division of interests at the annual meetings, and this would be met by holding parallel morning sessions as we used to do. Refresher courses on a somewhat broader basis would also be held in parallel. Some of these would be directed primarily to such groups as radiological and hospital physicists and thus encourage them to participate in radiological problems. The whole idea is to take active steps to strengthen medical radiology which has become a complicated mixture of many scientific fields.

L.S.T.

While these agreements dealt primarily with program orientation and refresher courses, they were a critical first step towards recognizing the "allied scientists" as a coherent non-medical group.

The second step was taken on January 10, 1952, when Dr. Ira Lockwood, then President of the RSNA, sent a message to the Society's Counselors. After an explanation of the situation, he posed the question of full membership for the allied group. Dr. Lockwood's communication follows:

As Counselor of the Radiological Society, you are a member of the Membership Committee of which I am chairman. You are charged with the duty of maintaining and increasing the membership of our Society.

(1) One of the major objects of our society is that it includes and represents all qualified radiologists devoting the major portion of their time in medical practice to radiology in one or all of its branches.

This problem regarding the "major" portion of their practice being devoted to radiology was not entirely settled by the Board of Directors and if you have anything further to add, your chairman would be very glad to hear from you.

(2) For many years there has been a group of an allied profession, the "physicist", closely associated with The Radiological Society of North America, Inc., who have presented papers, prepared exhibits, and served on appropriate committees, who are now classified as associate members. They pay the same dues as anyone else, have no vote, nor can they hold office. They have accepted cheerfully requests for regular presentations of refresher courses, have served on the program committee and have reviewed papers for publication in the Journal. At no time has any action been taken regarding making this allied group members of The Radiological Society of North America, Inc., with power to vote and hold office.

In earlier days, The Radiological Society of North America, Inc., led in a forward-looking policy in radiological physics. This leadership was acquired by the participation of the physicist. He was attracted to our meetings and for a while new men were added. This growth of associate member physicists has largely stopped and some believe that unless our radiological society embraces the radiological physicists, biologists, and the allied groups, that further separation will take place with their withdrawal from our clinical radiology. The question that arises is: Should it be the policy of The Radiological Society of North America, Inc. to add this allied group as full members to The Radiological Society of North America, Inc.?

(3) Would you consider ways and means of doing away with the reading of the names of those to be elected to membership in the Society at an executive session?

I would appreciate an early reply to the enclosed questionnaire. I realize that you do not have a great deal of time to think this over, but if it is humanly possible, won't you let me have your answer before February 6th, 1952? There is to be a meeting of the Board of Directors of The Radiological Society of North America on February 10th, 1952, following the Teachers Conference. This letter is being sent to all of the Counselors, the officers, and chairmen of committees. I would like to call your

attention to No. 8--Should there be a registration fee for non-members of The Radiological Society of North America? Everyone wants and needs more money; your Society needs the same thing. I feel that if we were to adopt a registration fee for non-members of The Radiological Society of North America, we could then drop the charge for the refresher courses and consequently they would not be paying any more money than they are now. As you know all members are admitted to the refresher courses without charge.

Attachment: Questionnaire (sample)

Please retain this copy for your files.

1. Have you any further suggestions regarding qualification of members for the Radiological Society of North America?

   No recommendations with regard to radiologists.

2. Consideration of association of allied professions with RSNA as full members.

   Would like to recommend that scientists for fields closely allied to radiology be admitted to full membership. Their qualifications should be set at a high level. This is being taken up separately with the executive committee.

3. Would you consider ways and means of abolishing the reading of names of candidates for election at executive sessions?

   I think it would be most desirable to abolish the reading of names of candidates.

4. Have you any suggestions regarding the program of the General Assembly?

   I would like to see the general programs broadened to include more papers from non-radiological fields which are very closely related to radiology, i.e. radiobiology, radiochemistry, radiophysics, etc.

5. Have you any suggestions regarding the refresher courses?

   To improve the relationships between radiology and allied sciences, I would suggest including refresher courses for such groups as radiological physicists, hospital physicists, etc.

6. Have you any suggestions regarding publicity of the annual meeting?

   None

7. Have you any suggestions regarding the date of the annual meeting?

   I would like to see the meetings somewhat more remote from Christmas. They used to follow immediately after Thanksgiving which was not too bad but at the present they run pretty well into Dec. and I have heard many adverse comments regarding this. A meeting even before Thanksgiving might be considered.

8. Should there be a registration fee for non-members of the RSNA?

   I believe it is entirely fair to charge registration fees for non members of the RSNA.

I.L.

The suggested program changes were immediately put into effect (early 1952). To facilitate the changes, Taylor was added to the RSNA Program Committee, an assignment which he held until about 1960.

Apparently, a principal stumbling block of full membership for non-medical individuals was the strong feeling among radiologists that such persons should not be allowed to hold an elective office, lest this could somehow undermine or divert the purely medical interests of

the organization. Taylor's thoughts on this subject were outlined in the following letter of January 28, 1952 to Dr. Childs, then Secretary/Treasurer of the RSNA:

January 28, 1952

Apropos our conversations on Sunday together with Drs. Ball, Pendergrass and Quimby, I will outline my views regarding the membership of non-medical scientists in the RSNA. The fact that the RSNA recognizes the need for close cooperation between radiology and allied sciences is clearly set forth in Article II of the Articles and By-laws.

It is believed that the technical and scientific advancement of Radiologists and allied scientists can both be enhanced by including the latter in the R.S.N.A. as full members. However, to insure that the R.S.N.A. continues along its present dominantly medical lines, there should be some minor restrictions regarding the privileges of the non-medical members. Specifically, it is recommended that non-medical members enjoy all rights and privileges except holding the offices of President, President-elect and Chairman of the Executive Board. Memberships on all committees should be permissible and in some cases required.

The following committees or Boards should have at least one non-medical member among its membership: Executive Board, Program Committee, By-laws Committee, Board of Censors, Refresher Course Committee, etc.

Qualifications of non-medical members should be established on a plane at least as critical in their field as for radiologists. The applicants should have at least three years posteducational professional experience in a field of activity closely related to the field of medical radiology. He should be a member of a recognized professional society covering his professional field, i.e. physics, chemistry, biology, zoology, etc. (The Board of Censors should set up a listing of such recognized professional societies for its own guidance.) The non-medical membership applications should be endorsed by one non-medical member who is capable of evaluating the applicant's professional experience and ability. At least until such time as the R.S.N.A. membership includes an adequate number of non-medical scientists in the various fields, letters of recommendation should be obtained from outside scientists of established standing, and who are familiar with the applicant's professional work. Certification by the Board of Radiology should satisfy the requirements of professional status, though not necessarily the requirements as to time in the field.

To encourage the participation of younger non-medical scientists in the activities of the R.S.N.A., they should be eligible for a limited membership equivalent to "membership-elect" for medical members. Such membership should imply that they are working closely with full members or others of recognized standing in a field closely allied with medical radiology. They should be selected for their potentialities in their field and when professionally engaged under such circumstances as to insure their training and growth along scientific lines closely allied to Radiology. Upon completion of the requirements for full membership, they should then be eligible therefor.

I believe that the establishment of a membership policy along some such lines as outlined above will be a definite forward step by the RSNA.

L.S.T.

The first RSNA program clearly recognizing the role of the allied scientists was the annual meeting to be held in Cincinnati in December 1952. In this connection, an editorial entitled "Radiology and the Related Sciences" by Taylor was published in the May 1952 issue of Radiology (Ref. 160). The planning for this initial program suffered some limitations. Many of the main radiological programs were already in place, forcing the "allied science" programs into parallel sessions in a nearby hotel. The same situation held for the refresher course programs, which included eight new courses as well as four given each year. Following is Taylor's Radiology editorial announcing the program:

RADIOLOGY AND THE RELATED SCIENCES
May, 1952

Radiology, as we think of it today, and properly so, is dominantly a medical field in which the major objectives are the diagnosis and treatment of disease. But like many other fields, including medicine in general, its growth and development depends to a considerable extent upon advances in other sciences and hence upon close interrelationship with them. In its first days, before it could even lay claim to a title all its own, radiology was practiced by photographers, engineers and physicists.

In fact, engineer Sylvanus Thompson is one of the best known names in early British radiology. The name of physicist W. D. Coolidge is almost synonymous with radiology the world over, not only because of his discovery of the hot cathode X-ray tube but because of his continuing contributions to the radiological field. This partnership has prospered to the benefit of both radiology and physics.

As radiology began to mature in the early twenties, it found itself in possession of the tools to perform deep therapy on a reliable day-to-day basis--reliable, that is, to the extent that the X-ray equipment could be counted upon to perform its duty without the tantrums and tempers of the gas tubes. This brought with it the necessity for strict dosage control, an understanding of depth dose, and the concept of radiation dosage units which it was hoped would apply under all irradiation conditions. Then entered the physicist to work with the radiologist in a borderline field beyond the ken of all but a very few medical radiologists. This partnership has prospered to the material benefit of both radiology and physics.

Physicists have become associate members of our radiological societies; they have served on our technical committees relating to x-ray units, standards and protection; they are members of the team in an increasing number of radiological departments and institutions and they are now certified as Radiological Physicists by the American Board of Radiology--the outgrowth of a movement started in 1935 by the Standardization Committees of the radiological societies of this country. The closeness of the bonds between these two sciences has increased steadily, if for no other reason than the fact that with advances in physics and engineering, the techniques of radiology have become almost too complicated for the radiologist to attempt to handle alone. All this was so, prior to the advent of the atomic energy era; now the same situation exists on a greatly magnified scale. In addition, it has given accent to other borderline fields--always important and so recognized, but now sought after as a part of the family Radiology. With the new tools of radioactive isotopes for research, therapy and diagnosis, the radio-biologist, the radio-chemist, the radio-physicist, and to a great extent the surgeon and internist using isotopes must be brought closer together with the radiologist for the accomplishment of their common aim.

The Radiological Society of North America has always been aware of the necessity for bringing the scientists of borderline or associated fields into closer relationship with its radiologist members. It now recognizes that this union of interests must develop on a broader scale and that to accomplish this, the workers in the various fields must be brought together on a common meeting ground. The ideal medium for the union would appear to be the annual scientific meeting of the Society.

At the annual meeting which will be held in Cincinnati in December of this year, the Radiological Society of North America is expanding its program facilities to include the presentation of a much larger number of scientific papers and discussions in the borderline sciences. In addition to the usual sessions on more purely medical radiology, there will be parallel scientific sessions on Tuesday through Friday mornings devoted primarily, and about equally, to subjects in radio-physics, -biology, and -chemistry. The single afternoon sessions will also include borderline science reports which are of a more general and less technical nature. It is expected that the time limitations will permit the presentation of at least sixteen papers in the parallel morning session.

In addition, the Refresher Course program will also be expanded in the parallel session, permitting the presentation of twelve lectures or demonstrations. This will mean the addition of about eight new courses which have never before been given, together with the four which have been given yearly in the past.

It is hoped that with these expanded program facilities, many of our scientists in associated fields will find the coming annual meeting of the Radiological Society of North America a fruitful ground at which their ideas and programs may be discussed. Here they should better learn the needs of the radiologist and at the same time let the radiologist learn of their fields. Only by this means can we realize the ultimate benefit from all of our complex scientific aids.

The following list is indicative of the recommended refresher courses and instructors:

SUGGESTED REFRESHER COURSES

Isotope laboratory design - Manov or (?)
Clinical Handling of Isotopes - Dick Chamberlain
Isotope Measurement - N.Y. people or Corrigan

X-ray protection design - Wyckoff
Training of Radiation Physicists - Braestrup
Health Physics - Ralph Overman
Cross-fire Techniques and Dosage Control -
Radioactivity Assay - some radio chemist
Radium Dosimetry - Quimby
Construction and Application of Ra and Co Plaques -
Dosimetry - Parker
Supervoltage therapy techniques -
Radiological Monitoring and Instruments -
Superficial Therapy Techniques and Measurements -
Theory of Biological Actions of Radiation - Fano
Conference of Registered Radiological Physicists -

L.S.T.

In addition to the editorial, the following announcement for the March issue of Radiology was recommended by the Physics Committee:

"At the Cincinnati meeting, the Radiological Society of North America will undertake for the first time, a broad expansion of its technical program. Additional interest will be placed on the work being carried out in fields closely associated with radiology, such as, radiophysics, radiobiology, and radiochemistry. In addition to the usual sessions which will be held in the Netherlands Plaza Hotel, there will be a set of parallel sessions held at the Gibson Hotel during the mornings of Tuesday through Friday. These sessions will include twelve refresher courses in the general field of radiophysics, biology and chemistry, together with some highly specialized problems in therapy.

"Following the refresher program will be scientific sessions during which four or five papers per morning will be given on subjects in these borderline fields.

"It is hoped by these meetings to encourage closer bonds between radiology and other related sciences which are a necessary adjunct to radiology of today."

It had been customary for the Society to charge a registration fee for all attendees including speakers, unless they were members or associate members of the Society. Since it could be expected, at least at the outset, that a considerable number of the borderline scientists would not be members, there was some concern about the $15 fee for those presenting papers. When questioned by Taylor, the Board decided that the fee would be waived for all authors of papers. This seemed like a small item and indeed it was, but the fact that such points could be worked out cooperatively by the radiologists and the non-radiologists was indicative of the progress being made.

The variety and calibre of the papers submitted for the December meeting are reflected in the following list of titles and authors. There were a number of other deserving papers which could not be scheduled for lack of time.

PAPERS SUBMITTED FOR RSNA MEETING

June 1952

1. PHYSICAL AND CHEMICAL PROTECTION AGAINST RADIATION

Transport of Radium Sulphate from the Human Lung.
L. D. Marinelli, P. F. Gustafson
Argonne National Laboratory

Education and Training of Health Physicists.
E. E. Anderson
Oak Ridge National Laboratory

Attenuation of Obliquely Incident 0.66 and 1.25 Mev Radiations in Concrete and Steel.
R. Kennedy, H. O. Wyckoff
National Bureau of Standards

Effects of Acute Total-Body Irradiation on Salt and Water Metabolism and their Clinical Significance.
J. Z. Bowers, V. Davenport, N. Christiansen, J. Goodner
University of Utah

Personnel Protection for a Medical Betatron Installation.
H. W. Koch
National Bureau of Standards

2. CLINICAL APPLICATIONS

Central Nervous System Changes following Betatron Irradiations.
A. Arnold, P. Bailey, L. Haas, John S. Laughlin
University of Illinois

Physical Aspects of Electron Beam Therapy.
J. S. Laughlin, J. Ovadia, J. W. Beattie, W. J. Henderson
University of Illinois

Conventional and Rotation Distributions of Radiation from 1000 Curie Co 60 Unit.
H. E. Johns, S. O. Fedoruk, E. M. Brown, T. A. Watson
Saskatoon Cancer Clinic

Survey of the Use Factor for X-ray Therapy Equipment.
E. D. Trout, J. P. Kelley
General Electric Company, Milwaukee

Teletherapy Design Problems (Cs-137 sources).
Marshall Brucer
Oak Ridge Institute of Nuclear Studies

3. RADIOBIOLOGY (Dowdy's Symposium)

Synergistic Lethal Action of Selected Isotopes Administered in Combination.
H. L. Friedell and Salerno
University Hospitals of Cleveland

Intracavitary Colloidal Gold.
G. A. Andrews
Oak Ridge Inst. of Nuclear Studies

A Sensitive Gamma Detecting Device (The Scintigraph) as a Clinical Tool in Diagnosis of Morphologic Abnormalities of Human Thyroid Gland.
F. K. Bauer, R. Libby, B. Casson and W. Goodwin
Veterans Administration, Los Angeles

New Technique for the Diagnosis of Metastatic Carcinoma of the Liver Using Iodinated Human Serum Albumin.
L. A. Stirret, E. T. Yuhl and R. L. Libby
Veterans Administration, Los Angeles

Diagnosis and Treatment of Hyperthyroidism by Radioiodine.
E. R. Miller
University of California

4. RADIATION DOSAGE MEASUREMENTS AND MISCELLANEOUS

Ca^{45} Deposition Studies Utilizing Microscopic, Autoradiographic, and Radiomicrographic Techniques.
J. E. Askin, L. L. Yates, E. W. Cauldwell, R. A. Harvey
University of Illinois

The Dosimetry of Beta Radiations.
Robert Loevinger
Mt. Sinai Hospital, New York

An Improved Clinical Dosemeter for the Measurement of Radiation.
S. O. Fedoruk, H. E. Johns, E. R. Epp, T. A. Watson
Saskatchewan Cancer Commission

Comparison of Bactericidal Effects of Radiation from a Kilocurie CO-60 Source and of 3 Mev Cathode Rays and X-rays Produced by a Van de Graaff Accelerator.
S. A. Goldblith, B. E. Proctor, and C. J. Bates
Mass. Institute of Technology

Fast Neutron Dosimetry.
G. S. Hurst, R. H. Ritchie
Oak Ridge National Laboratory

5. PAPERS POSSIBLY SUITABLE FOR GENERAL SESSION

Protection of Radiologists in Fluoroscopy.
Scott W. Smith and J. Brooks
National Bureau of Standards

Advantages of High Voltage Roentgenography in Pelvic Analysis.
B. S. Kalayjian
Womens Hospital, Detroit

6. PAPERS NOT INCLUDED IN PROGRAM

Physical Measurements of Radiation through a Grid.
T. E. Sopp, L. Stanton
Philadelphia General Hospital

Pharmacological Studies on Irradiated Animals
or
Endocrine Influences on Radiosensitivity.
F. Ellinger
Naval Medical Research Inst., Bethesda

Biological Response of Analogous Mammary Tumors of X-radiation.
Anna Goldfeder
Cancer Research Lab, New York

Water Decontamination.
C. P. Straub
Oak Ridge National Laboratory

Partial Irradiation of Single Cells.
R. E. Zirkle and Bloom
University of Chicago

10 Million-Volt Betatron Spectra.
J. Motz
National Bureau of Standards

Attenuation of 10 to 50 Million-volt Betatron Radiation in Concrete.
F. Kirn, R. Kennedy
National Bureau of Standards

Review of Penetration and Degradation of High Energy Radiation.
H. O. Wyckoff
National Bureau of Standards

Backscattering of Narrow Beam Co-60 Gamma Rays from a Wall.
E. Hayward
National Bureau of Standards

Cavity Ionization Measurement and the Validity of Gray's Law for Betatron X-rays in Water.
J. McElhinney, H. W. Koch
National Bureau of Standards

Use of a Calorimeter to Determine the Absolute Response of Secondary Standards for Measuring Betatron X-ray Intensities.
J. McElhinney
National Bureau of Standards

Absolute Intercalibration of an X-ray Spectrometer, a Calorimeter and some Secondary Standards to Determine the Response to Betatron X-ray Photons.
H. W. Koch, J. McElhinney
National Bureau of Standards

Elemental Radium Poisoning.
E. L. Saenger
University of Cincinnati

Damage and Repair in the Intestinal Epithelium of the Mouse following Total Body Irradiation with X-Irradiation.
W. Montagna, H. B. Chase, P. F. Fenton, M. H. Hatch, J. W. Wilson
Brown University

Meanwhile, the Physics Committee continued its normal business although its major preoccupation involved program planning for the forthcoming meeting. Actually, there were fewer scientific matters to be considered by the Physics Committee because of the supposed shift of technical activities to CRUSP and the increased activities of the NCRP, the ICRP, and the ICRU.

These changes were reflected in the Committee's Annual Report to the RSNA business meeting on August 26, 1952, as follows:

REPORT OF PHYSICS COMMITTEE
August 26, 1952

The principal project of the Physics Committee of the Radiological Society of North America has been the organization of a special program of technical papers and refresher courses to be given at the Cincinnati meetings. The need has been felt for some time and by a great many people, that the scientific programs of the annual meetings did not have sufficient time or space to encourage the presentation of many papers outside of the strictly radiological field. To overcome these difficulties, the 1952 meeting was set up to have parallel sessions run in the mornings for the delivery of refresher courses in the fields of physics, radiobiology, and radiochemistry. Twelve such courses have been arranged for.

Scientific contributions covering the same fields will be presented in 20 papers to be given in the latter half of the morning session. The views of a large number of people were solicited with regard to the formulation of this program. The reaction was unanimously favorable and the indications were that a considerable number of people would attend the sessions. About twice as many papers were submitted for presentation as could be accommodated within the time limits available. It is anticipated that if this program is a success, future steps will be taken for its continuance.

It was not until several years later that the Radiological Society admitted the "allied scientists" to full membership, and in 1970, Dr. Harold Wyckoff, a physicist, was elected to the position of Treasurer of the Society, a position which he held for 6 years.

Joint programming successfully continued with the responsibility for the "allied scientists" portion resting, in principle, in the hands of the Physics Committee. In actual

practice, Taylor and Wyckoff not only assumed this responsibility, but they introduced two interesting innovations described below.

Most technical societies were notorious for their willingness to allow speakers to use completely inadequate slides. The two principal faults were the use of lettering or diagrams that were too small or lightlined, and the inclusion of more data on a slide that could be reasonably absorbed by the listeners or discussed by the speaker. After one or two attempts to get speakers to prepare proper slides, speakers on the allied science program were informed that their slides must meet the minimum standards described in an accompanying bulletin or they would not be allowed to speak (Taylor, 1963). After three or four rejections, the message seemed to get across and, at least for a while, the allied science papers were accompanied by exemplary slide presentations.

The other innovation grew out of the fact that the Radiological Society program, normally scheduled for December, was essentially frozen by mid-spring. By the time of the meeting, the material was frequently stale. This was particularly troublesome in borderline sciences where researches and studies moved much more rapidly. At Wyckoff's suggestion, a half-day program was initiated consisting of papers for which the title and abstract were accepted up to 6 weeks prior to the meeting. Entitled "Work in Progress," the program, which allowed brief papers on work recently completed or in progress, was exceedingly popular and drew a large number of the radiologists in addition to the borderline scientists. The first such program was on December 14, 1955, and consisted of six papers with the following titles:

1. Neutron Radioactivated Pure Chromium Crystals as a Source of Gamma Rays for Radiation Chemotherapy.
2. A Thorium-227 Accident.
3. Simplified Procedures for Dose Preparation and Infusion of Colloidal Gold.
4. Modification of Lethal Radiation Effects in Rats by Short-Term Protraction of Dosage.
5. Extrapolation Chamber Measurements with Nickel-63 Beta-Rays.
6. Response of Photographic Emulsions to Charged Particles and Neutrons.

By 1957, there were 10 work-in-progress papers, and in 1959, 12; a demonstrated success.

CHAPTER 18. AN EPILOGUE

This concludes the summary of National Bureau of Standards work on radiation measurements and protection, spanning 50 years of effort. The crucial problems at the outset have long since been solved and other major interests and concerns have taken their place. Today radiation measurement standards continue to be improved but refinements are mostly in the next decimal place. In one sense the period up to 1963 may be considered not as an epilogue of a half century of progress, but as a prologue of the changing pattern of interest and events in the next half century.

The development of nuclear energy introduced many new and complicating problems into the radiation field. The needs of basic and applied nuclear physics led to the development of new families of particle accelerators, and associated measurement problems required more clear-cut traceability to the basic measurements at NBS.

Meanwhile the radiological profession has expanded to the point where its very size now stifles the personal relationships which were so important in the early days between NBS and the radiological community, as represented by the National Council on Radiation Protection and Measurements (NCRP), the International Commission on Radiation Units and Measurements (ICRU), and the International Commission on Radiological Protection (ICRP). Moreover, these close ties were enhanced by membership and offices held by Bureau scientists in these organizations over the years as far back as 1928, including chairmanship of NCRP from 1929 to 1964, and of ICRU from 1953 to 1956 (Taylor, 1958A, 1958B, 1958C).

Though this close and very effective working relationship on a physicist-to-radiologist basis has largely disappeared, the Bureau does continue an important role in the ionizing radiation community on a physicist-to-physicist relationship.

In 1964 the Bureau began the move to its new Gaithersburg site, where the laboratory facilities for radiation work had been greatly expanded for on-going as well as new fields of research. Despite the ever increasing portion of efforts that is diverted from creative research to housekeeping/paper work chores, NBS staff productivity, as measured by the number and quality of publications, has remained high. From 1929 through 1941, with a staff of three, there were 44 research publications. From 1946 through 1963, there were 613 publications overall, and from 1964 to 1976, an additional 638, for a total of over 1300. Indeed, an important part of this volume of NBS work in ionizing radiation and radiation protection is appendix B, prepared from x-ray laboratory records. This is the first complete listing of the NBS Radiation Laboratory's published output--an output that has played a very significant role in our knowledge of ionizing radiation, its accurate measurement, and its safe applications to the benefits of mankind.

Yet in spite of all that has been accomplished in the field of ionizing radiations, critical problems remain to be faced--not just by the Bureau but by the scientific community in general and the radiation community in particular. During the past decade or so, ionizing radiation has surfaced in the political arena. In the minds of some, it is almost a dirty word. Nevertheless, radiation is with us to stay as a critically important tool in medicine, in commerce, and as an unwanted by-product of nuclear-generated electrical energy.

The Bureau's programs must continue as almost the only radiation activities in Government devoid of self-interest and regulatory posture. As in the past, this will require an adequate and technically competent staff that is highly motivated and supported by management.*

*As this tome is brought to a close, the author has a recommendation for those involved in current and future efforts in this field, particularly if there is to be a sequel to this volume. Convinced that reasonably detailed perspectives are essential to orderly future growth of any organization or society, he urges that adequate plans be made to select and preserve pertinent records of Bureau work. It was fortunate, indeed, that the major sources of information for this volume had been preserved by interested individuals and private organizations. Otherwise, it would have been impracticable to reconstruct the scientific and philosophical development (good or bad) of the quantities, units, and measurement systems needed to use and control ionizing radiations.

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APPENDIX A

Reports of Standardization Committee of RSNA

APPENDIX B

Radiation Publications by Staff of NBS (1925-1963)

1925

1. HUNT, FRANKLIN L.,
Barium sulphate as a protective material against roentgen radiation. Am. J. Roentgenol. Radium Ther. 14, 524 (1925).

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Improved form of gas type X-ray tube. J. Opt. Soc. Am. 16, No. 1; Rev. Sci. Instrum. 16, 68 (1928).

1929

3. TAYLOR, LAURISTON S.,
Cathode ray dosimetry. Radiology 12, 294 (1929).

4. TAYLOR, L.S.,
The precise measurement of X-ray dosage. BS J. Research 2, 771 (1929), RP 56; Radiology 14, 372 (1930).

5. NICHOLAS, W.W.,
Continuous spectrum X rays from thin targets. BS J. Research 2, 837 (1929). RP 60.

6. TAYLOR, L.S.,
Roentgen-ray protection. Am. J. Roentgenol. Radium Ther. 22, 45 (1929).

7. RICHTMYER, F.K. and TAYLOR, L.S.,
The relative intensity of X-ray satellites. Science 70, 616 (1929).

8. TAYLOR, L.S.,
Analysis of diaphragm system for the X-ray standard ionization chamber. BS J. Research 3, 807 (1929) RP 119; Radiology 15, 49 (1930).

9. CURTISS, L.F., ASTIN, A.V., STOCKMANN, L.L., and BROWN, B.W.,
An improved radiometeorograph on the Olland principle. BS J. Research 2, 97 (1929) RP 1169.

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10. TAYLOR, L.S.,
The problem of international X-ray standardization. Radiology 14, 551 (1930).

11. TAYLOR, L.S. and SINGER, G.,
The calibration of the "Fingerhut" ionization chamber. BS J. Research 4, 631 (1930) RF 169; Radiology 15, 227 (1930).

12. RICHTMYER, F.K. and TAYLOR, L.S.,
The intensity of X-ray satellites. Phys. Rev. 36, 1044 (1930).

13. TAYLOR, L.S. and SINGER, GEORGE,
An improved form of standard ionization chamber. BS J. Research 5, 507 (1930) RP 211; Radiology 15, 637 (1930).

14. TAYLOR, L.S.,
Absorption measurements of the X-ray general radiation. BS J. Research 5, 517 (1930), RP 212; Radiology 16, 302 (1931).

15. TAYLOR, L.S.,
Apparatus for the measurement of high constant or rippled voltages. BS J. Research 5, 609 (1930) RP 217; Radiology 16, 893 (1931).

16. NICHOLAS, WARREN W.,
Efficiency of production of X-rays. BS J. Research 5, 843 (1930), RP 235.

1931

17. TAYLOR, L.S.,
Recent progress in X-ray standardization Radiology 16, 1 (1931).

18. TAYLOR, L.S. and SINGER, G.,
Further studies of the X-ray standard ionization chamber diaphragm system. BS J. Research 6, 219 (1931), RP 271; Radiology 17, 104 (1931).

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*The following designations are used throughout this list.
RP Research Paper. These are papers which appeared in the Journal of Research of the National Bureau of Standards.
C National Bureau of Standards Circular.
H National Bureau of Standards Handbook.
M National Bureau of Standards Monograph.
TN National Bureau of Standards Technical Note.

19. TAYLOR, L.S.,
Accurate measurement of small electric charges by a null method. BS J. Research 6, 807 (1931), RP 306; Radiology 17, 294 (1931).

20. TAYLOR, L.S.,
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Report of committee on standardization of X-ray measurements. Radiology 22, 289 (1934).

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614. Medical X-ray protection up to two million volts, issued Mar. 30, 1949. Report of the National Committee on Radiation Protection and Measurements (NCRP), NBS Handb. 41 (Supersedes H20, which superseded H15; and in turn is superseded by H60).

615. Safe handling of radioactive isotopes, issued Sept. 1949. Report of the National Committee on Radiation Protection and Measurements, NBS Handb. 42.

616. Recommendations of the International Commission on Radiological Protection and of the International Commission on Radiological Units, 1950, issued June 29, 1951, NBS Handb. 47. Note: The 1953 recommendations of the ICRU have been published in Radiology 62, 106 (1954), in Am. J. Roentgenol. Radium Ther. Nucl. Med. 71, 139 (1954), and in Nucleonics 12, No. 1, 11 (1954). The 1953 recommendations of the ICRP have been published in the British J. Radiology, Suppl. 6, 1955.

617. The 1956 recommendations of the ICRU have been published as NBS Handb. 62; the 1956 recommendations of the ICRP were reported in Radiology 70, 261 (1958), and published by Pergamon Press, New York, 1959.

618. Control and removal of radioactive contamination in laboratories, issued Dec. 15, 1951. Report of the NCRP, NBS Handb. 48.

619. Recommendations for waste disposal of phosphorus-32 and iodine-131 for medical users, issued Nov. 2, 1951. Report of the NCRP, NBS Handb. 49.

620. WYCKOFF, H.O. and TAYLOR, L.S., X-ray protection design, issued May 9, 1952, NBS Handb. 50.

621. Radiological monitoring methods and instruments, issued Apr. 7, 1952. Report of the NCRP, NBS Handb. 51.

622. Maximum permissible amounts of radioisotopes in the human body and maximum permissible concentrations in air and water, issued Mar. 20, 1953. Report of the NCRP, NBS Handb. 52 (Superseded by H69).

623. Recommendations for the disposal of carbon-14 wastes, issued Oct. 26, 1953. Report of the NCRP, NBS Handb. 53.

624. Protection against radiations from radium, cobalt-60 and cesium-137, issued Sept. 1, 1954. Report of the NCRP, NBS Handb. 54 (Supersedes H23, which superseded H18).

625. Protection against betatron-synchrotron radiations up to 100 million electron volts, issued Feb. 26, 1954. Report of the NCRP, NBS Handb. 55.

626. Safe handling of cadavers containing radioactive isotopes, issued Oct. 26, 1953. Report of the NCRP, NBS Handb. 56 (Superseded by H65).

627. EHRLICH, MARGARET, Photographic dosimetry of X- and gamma rays, issued Aug. 20, 1954. NBS Handb. 57.

628. Radioactive-waste disposal in the ocean, issued Aug. 25, 1954. Report of the NCRP, NBS Handb. 58.

629. Permissible dose from external sources of ionizing radiation, issued Sept. 24, 1954. Report of the NCRP, NBS Handb. 59.

630. X-ray protection, issued Dec. 1, 1955. Report of the NCRP, NBS Handb. 60 (Supersedes H41, which superseded H20, which superseded H15).

631. Regulation of radiation exposure by legislative means, issued Dec. 9, 1955. Report of the NCRP, NBS Handb. 61.

632. Report of the International Commission on Radiological Units and Measurements (ICRU), 1956, issued Apr. 10, 1957, NBS Handb. 62 (Superseded H47 and is in turn superseded by H78 issued in 1961).

633. Protection against neutron radiation up to 30 million electron volts, issued Nov. 22, 1957. Report of the NCRP, NBS Handb. 63.

634. WYCKOFF, H.O. and ATTIX, F.H., Design of free air ionization chambers, issued Dec. 13, 1957. NBS Handb. 64.

635. Safe handling of bodies containing radioactive isotopes, issued July 10, 1958. Report of the NCRP, NBS Handb. 65 (Supersedes H56).

636. Safe design and use of industrial beta-ray sources, issued May 28, 1958. Report of Subcommittee on Sealed Beta-Ray Sources of ASA Z-54 Sectional Committee, NBS Handb. 66.

637. Maximum permissible body burdens and maximum permissible concentrations of radionuclides in air and in water for occupational exposure, June 1959, NBS Handb. 69 (Supersedes H52).

638. Measurement of neutron flux and spectra for physical and biological applications, July 1960, NBS Handb. 72.

639. Protection against radiations from sealed gamma sources, issued July 1960. NBS Handb. 73.

640. Measurement of absorbed dose of neutrons, and of mixtures of neutrons and gamma rays, issued Feb. 3, 1961. Report of the NCRP, NBS Handb. 75.

641. Medical x-ray protection up to three million volts, issued Feb. 9, 1961. Report of the NCRP, NBS Handb. 76.

642. Report of the International Commission on Radiological Units and Measurements (ICRU), issued Jan. 16, 1961. NBS Handb. 78, superseded in part by Handb. 84-89.

643. Stopping powers for use with cavity chambers, issued Sept. 1, 1961. Report of the NCRP, NBS Handb. 79.

644. A manual of radioactivity procedures, issued Nov. 20, 1961. Report of the NCRP, NBS Handb. 80.

645. Radiation quantities and units, issued Nov. 14, 1962. ICRU Report 10a, NBS Handb. 84.

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Diffusion in infinite media. C.H. Blanchard.
Range of electrons in the energy interval 0.5 to 1.3 MeV. J. Fleeman.
Backscattering of positrons and electrons. H. H. Seliger.

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Energy loss and range of electrons and positrons. (Dec. 30, 1958). NBS Circ. 577 Supplement.

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X-ray attenuation coefficients from 10 keV to 100 MeV. (Oct. 30, 1959). NBS Circ. 583 Supplement.

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Preparation, maintenance, and application of standards of radioactivity. (June 11, 1958). NBS Circ. 594, p. 47.

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Energy spectrum resulting from electron slowing down. (Feb. 20, 1959). NBS Circ. 597.

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Shielding against gamma rays, neutrons, and electrons from nuclear weapons. (Feb. 1964). NBS Mono. 69.

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Determination of total x-ray beam energy with a calibrated ionization chamber. (June 1962). NBS Mono. 48.

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Structure shielding against fallout radiation from nuclear weapons. (June 1, 1962). NBS Mono. 42.

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Energy dissipation by fast electrons. (Sept. 10, 1959). NBS Mono. 1.

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Transmission and reflection of electrons by aluminum foils. (Apr. 1963). TN 187.

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The construction of calorimeters for the measurement of absorbed dose. (Nov. 1962). TN 163.

673. McLAUGHLIN, W.L.,
Evaluation of unexpectedly large radiation exposures by means of photographic film. (Aug. 1962). TN 161.

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A Fortran code for calculation of eigenvalues and eigenfunctions in real potential wells. (Aug. 1962). TN 159.

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An experimental study of beta decay using the radiations from oriented nuclei. (Aug. 1961). TN 93.

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Determination of the K fluorescence yield of argon by proportional-counter spectrometry. (May 1961). TN 91.

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An evaluation of Kacser's second order Born approximation to the bremsstrahlung differential cross section. (June 1961). TN 81.

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Photographic dosimetry at total exposure levels below 20 mr. (Oct. 1959). TN 29.

682. BERGER, M.J. and SPENCER, L.V.,
Penetration of gamma rays from isotropic sources through aluminum and concrete. (June 1959). TN 11.

683. LEISS, J.E.,
Calculated behavior of a fast neutron spectrometer based on the total absorption principle. (Apr. 14, 1959). TN 10.

684. NICASTRO, L.J. and CASWELL, R.S.,
A double-pulse total-absorption fast neutron spectrometer. (Apr. 24, 1959). TN 1.

APPENDIX C

RADIATION PHYSICS STAFF: 1913 TO 1964

Given below is a listing of most of the technical staff members of the X-Ray Group and Radiation Physics Laboratory of the National Bureau of Standards during the period of 1913 through 1963. (Excluded is the Radioactivity Section from 1926-1946, for which records are not available.) Source material of these records is variable. From 1951 to 1961, informal laboratory reports listed the established staff members but excluded some junior ones. Publication records gave additional information and also included names of authors from other parts of the Bureau. The dates of tenure were, in many cases, determined from telephone directories and therefore may vary with the actual dates in some cases.

Guest workers and summer students are not included in this roster even though their names may appear in the publication list. Over the period of about 1949-1963, the level of supporting personnel varied somewhat and the following numbers at any time are rough:

Junior Professional - - -	13
Sub Professional - - - - -	20
Administrative - - - - - -	11
Professional - - - - - -	70-80

Information given for each person is necessarily brief and therefore incomplete in some cases. Where a date reads, "1963+", it means that the individual was on board at that time and for some uncertain time thereafter. The notation, e.g., "1955-date," means the person was still on the staff at the end of 1980. Following is the staff roster as described above:

ATTIX, F.H. 1950-1958. Calibration of x and gamma-ray survey meters

BACH, R. 1953-1966. Radiation field from rectangular sources.

BARE, D.D. 1945-1948
Instrument maker.

BARRANS, PAUL B. 1957-1964. Radiation instrumentation.

BAY, Z.L. 1955-1961. Measurement of W, fast coincidence experiments.

BERGER, M.J. 1952-date. Theory of gamma ray diffusion.

BERGER, R.T. 1955-1960. X or gamma-ray transfer coefficients.

BEVERLY, W.B. 1955-1963+. Neutron polarization and spectroscopy.

BLANCHARD, C.H. 1950-1954. Theory of electron penetration and diffusion.

BOAG, J.W. 1953-1954. Distribution of LET for fast neutrons.

BRABANT, J.M. 1951-1954. Calibration of standard methods for electron dose measurements.

BRADFORD, W.R. 1948-1949.

BREUCKMANN, R.E. 1948-1963+. Engineering of x-ray equipment.

BROOKS, J.R. 1947-1962. Radiation shielding measurements.

BROWN, B. 1954-1956. Neutron spectroscopy.

BROWN, GILES 1949-1953. Ionization of liquids, CO-60 instrument calibration sources.

BRYSK, H. 1952-1955. Theory of penetration of photons and electrons.

CASWELL, R.S. 1952-date. Chief: Neutron Physics Section (1957)

CAVALLO, LUCY M. 1947-1980. Radioisotope standardization.

CHAPPELL, S.E. 1959-1963+.

CHARLTON, A.L. 1934-1942. X-ray standards.

CHIN, J. 1954-1963+. Neutron Activation cross sections.

CIALELLA, C. 1949-1954. Compton spectrometer.

CLELAND, M.R. 1951-1952. High energy gamma ray and neutron spectrometers.

COSTRELL, L. 1946-date. Chief: Nucleonic Instrumentation Section.

COYNE, J.J. 1960-1963+. Nuclear cross sections and penetrations.

CREW, J.E. 1957-1959. Energy dissipation in air by fast electrons.

CUNNINGHAM, J.A. 1950-1956. Electronics associated with 180 MeV Synchrotron.

CURRIE, L.A. 1960-1963+. Tritium labelling by uranium hydride.

DANOS, M. 1954-date. Photonuclear process in spheroidol nuclei.

DAVENPORT, T.I. 1951-1955. Radium measurements.

DAY, F.H. 1942-1958. X-ray standards.

De LaVergne, L. 1953-date. Radiation instrument studies.

deJUREN, J.A. 1951-1955. Standardization of neutron sources.

DICK, C.E. 1961-1963+. Large angle scattering of 500 keV electrons.

DOGGETT, J.A. 1952-1957. Gamma ray diffusion.

DOMEN, S. 1951-date. High energy x-ray calorimetry.

DORSEY, N.E. 1913-1919. Calibration of radium preparations.

EDWARDS, W.L. 1947-1955. Engineering Design.

EHRLICH, MARGARETE 1948-date. Photographic sensitometry and dosimetry.

EISENHAUER, C.M. 1958-date. Gamma ray and neutron penetration through matter.

ERNST, H. 1952-1954. Disintegration of barium-133.

FANO, UGO. 1946-1960. Chief: Nuclear Physics, Theory.

FEISTER, I. 1946-1951. Nuclear decay schemes, radiation instruments.

FERLAZZO, J. 1935-1941. Calibration of ionization chambers.

FITCH, S. 1949-1952. Film dosimetry.

FLEEMAN, J. 1950-1952. Film dosimetry of electrons.

FOOTE, R.S. 1949-1953. Removal of electrons from 50 MeV Betatron, high energy spectrometer.

FRANTZ, F.S. 1949-1955. Electron dose measurements, electron scattering and absorption.

FULLER, E.G. 1950-date. Photodisintegration of helium-4.

GABBARD, R.F. 1952-1953. Attenuation of neutrons in water.

GARFINKLE, S.B. 1949-1963+. Gamma and Beta ray standards.

GERSTENBERG, H.M. 1967-1963+. Neutron yield curves.

GIBSON, H.F. 1948-1953. Cavity ionization chambers, studies of electrons in range 0.5 to 1.4 MeV.

GOLDSTEIN, N. 1950-1952. Directional distribution of X rays.

GORTON, W.S. 1918-1921. Protective materials.

GRISAMORE, N.T. 1953-1956.

GROVE, G. R. 1950-1957. Measurements with pressure ionization chamber.

HARDING, J.E. 1948-date. Determination of radium in solution.

HAYWARD, EVANS V. 1950-date. Analysis and measurement of diffused and scattered gamma rays.

HAYWARD, R.W. 1950-1980. Nuclear decay schemes.

HILL, O.H. 1956-1963+. Radiation shielding.

HIRSHFELD, A.T. 1960-1963+. Cryogenic aspects of nuclear polarization.

HOBBS, T.G. 1959-date. Health Physics.

HOOPER, E.B. 1956-1958. Phosphor matrix for neutron detection.

HOPPES, D.D. 1950-date. Nuclear spectroscopy.

HUBBELL, J.H. 1950-1963+. X-ray attenuation coefficients.

HUMPHREYS, J.C. 1960-1963+. Spectral distribution of scattered radiation from cobalt-60.

HUTCHISON, J.M.R. 1957-date. Radioactivity standards.

JENKINS, F. 1949-1951

KAMM, G. 1949-1953. Positive ion tube and (p, gamma) reactions.

KENNEDY, R.J. 1946-1955. Gamma and x-ray attenuation and protection.

KIRN, F.S. 1949-1955. Gamma ray spectrometry, x-ray attenuation, development of dosimeters.

KOCH, H.W. 1949-1963+. Chief: Betatron Section.

LAMKIN, J.C. 1956-1958. Gamma ray penetration into shelters.

LAMPERTI, P.J. 1959-date. X-ray standards.

LEE, R.M. 1956-1961. Calibration of encapsulated radium sources.

LEISS, J.E. 1954-1979. Neutral photomeson production, Linear accelerator design.

LEWIS, MARGARET. 1950-1953. Energy distribution of secondary electrons ejected by ionizing radiations.

LOFTUS, T.P. 1949-date. Radiological equipment calibration.

MALMBERG, C.G. 1927-1932. X-ray standards.

MANN, W.B. 1951-1980. Chief: Radioactivity Section.

MARLOW, W.F. 1957-1962. Radiochemistry.

MAXIMON, L.C. 1958-1963+. Scattering theory.

MEDLOCK, R.W. 1952-1976. Recalibration of NBS carbon-14 standards.

MESHKOV, S. 1960-1963+. Nucleon Structure.

MILLER, WILLIAM. 1948-1956. Theory of X-ray production and detection.

MINTON, G.E. 1952-1956. Very high speed counting circuits.

MORRIS, E.E. 1959-1963+. Energy dissipated by fallout beta rays.

MOSBURG, E.R. 1956-1962. Neutron flux standards.

MOTZ, J.W. 1949-date. High energy gamma ray spectrometry and electron energy loss.

MULLEN, PATRICIA. 1948-date.

MURPHY, W.M. 1958-1963+. NBS standard neutron flux.

McCRAVEN, C.C. 1951-1955. Comparison of 4 radium standards.

McELHINNEY, J.A. 1949-1955. Photoneutron resonance energy.

McGINNIES, R. 1956-1959. Electron spectrum from electron slowing down.

McGINNIS, C.L. 1952-1960. Disintegration of indium-117 and antimony-117.

McLAUGHLIN, W.L. 1959-date. Photographic sensitivity and dosimetry.

McLERNON, F.D. 1955-1963+. Delayed coincidence studies.

NELMS, A.T. 1951-1959. Atomic form factors.

NEWMAN, P.A. 1958-1960. Ionization in air by alpha particles.

NEY, W. R. 1957-1964. Technical aide to Division Chief.

NICHOLAS, W.W. 1928-1931. X ray theory, thin target x rays.

NICASTRO, L.J. 1957-1959. Double-pulse fast neutron spectrometer.

NOYCE, R.H. 1959-1961. Neutron source emission rate.

OLIVER, D.W. 1957-1959. Indium resonance for D-D neutrons.

OPPENHEIM, I. 1954-1960. Atomic form factors.

PADGETT, D.W. 1950-1954. Calibration of photoneutron standard.

PAOLELLA, L. 1951-1956. National radium standards.

PEARLSTEIN, R.M. 1957. Slow component in alpha ionization.

PENNER, S. 1957-date. Neutral photomeson studies.

PESSOA, E.F. 1956. Gamma radiation from zinc-63.

PETREE, B. 1951-1961. Pulsed voltage photomultipliers, extraction of electron beam from 50 MeV betatron.

PLACIOUS, R.C. 1952-1963+. Bremsstrahlung cross sections.

PRUITT, J.S. 1953-1963+. Nuclear emulsion techniques, pattern amplifiers.

REAVES, J.H. 1954-1957. Bias supply for direct-coupled circuits.

REINGOLD, I. 1955-1960. Annihilation radiation contribution to gamma-ray flux.

RITZ, V.H. 1955-1960. Cavity ionization.

RICHARDSON, A.C.B. 1957-1963+. Neutron elastic and inelastic scattering cross sections.

ROSENWASSER, H. 1949-1953. Diffusion of thermal neutrons in water.

SAUNDERS, E.R. 1948-1955. Nucleonic instrumentation.

SCHARF, K. 1957-1973. Solid state and chemical dosimetry.

SCHRACK, R.A. 1956-date. Neutral meson decay.

SCHWEBEL, A. 1947-1980. Nuclear Chemistry, health physics.

SELIGER, H.H. 1948-1959. 4-pi counters, radioactivity standards.

SINGER, G. 1927-1946. X-ray standards and protection.

SLAWSKY, M.M. 1949-1951. Coincidence counting.

SMELTZER, J.C. 1950-1953. Scintillation detectors.

SMITH, C.C. 1953-1955. Comparison of 4 national radium standards.

SMITH, S.W. 1947-1963+. Chief: Radiological Equipment Section.

SNEDEGAR, W.E. 1949-1951. X-ray standards.

SNYDER, W.A. 1949-1950. Radiation shielding materials.

SPENCER, L.V. 1948-date. Theory of radiation penetration and diffusion.

SPIEGEL, V. 1955-1963+. Neutron ages in water, neutron polarization.

SPOKAS, O.E. 1953-1963+. Electron spin resonance detection.

STINSON, F. 1950-1951. X-ray diffusion.

STOCKMANN, L.L. 1926-1963+. Radon testing, radium analysis of ores and sludges.

STONEBURNER, C.F. 1930-1932. Lenard ray measurements.

STOVALL, T. 1961-1963+. The 4-body system.

TAYLOR, L.S. 1927-1965. Chief: Atomic and Radiation Physics Division.

TEMMER, G. 1951-1954. Alpha ray instrumentation and alpha ray scattering.

THEW, CARIN T. 1949-1963. AEC Liaison and security.

TITUS, W.F. 1954-1955. Angular distribution of scattered x rays.

TUCKER, K.L. 1930-1932. X-ray tube comparisons.

WANG, P.K.S. 1947-1952. Analysis of 10 MeV Betatron radiation.

WANG, T.J. 1949-1950.

WEAVER, J.T. 1956-1963+. Radiation instrument calibration.

WHITE, GLADYS R. 1941-1959. Critical data analyses.

WIENER, M. 1948-1951. Analysis of 10 MeV betatron radiation.

WYCKOFF, J.M. 1952-1977. Alpha particle instrumentation.

WYCKOFF, H.O. 1941-1966. Chief: Radiation Physics Laboratory.

ZANDONINI, E.M. 1948-1963+

ZENDLE, B. 1950-1955
Calorimetry of 50 MeV x-rays.

ZIEGLER, C.A. 1955-1956. Multiplier phototube noise.

ZIMMER, G.W. 1962-1964. 2-pi flow counter for alpha and beta particles.

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NOTE: No attempt has been made to include in this index the names of the NBS staff and members of the various committees who were intimately involved in the programs under discussion; they are named and identified throughout the body of the report. The name references included here are primarily those who were indirectly or marginally associated with these activities.

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An account of the initial U.S. concerns with, and subsequent efforts to cope with, the safe use of ionizing radiation is given. National interest was focused in the National Bureau of Standards where radiation programs were established at the urging of, and with the close cooperation of, the radiological profession. The National Bureau of Standards' ionizing radiation research programs, the first in the U.S. Government, were dedicated to establishing a sound scientific base for x-ray measurement and protection. Along with the support and constant efforts of the national and international radiological organizations, the Bureau played a critically important role in the Nation's ability to use radiation effectively and safely. Based on widely scattered files assembled from both Government and non-government sources, this document represents a single source record of significant committee papers, correspondence, and decisions during the period 1913 to 1964, along with the author's summary of National Bureau of Standards' work, and the interrelated efforts of many others in the scientific community. A complete listing of the published output of the National Bureau of Standards' radiation staff is also included in this document. These papers contribute significantly to our understanding of ionizing radiation.

12. KEY WORDS (Six to twelve entries; alphabetical order; capitalize only proper names; and separate key words by semicolons)

dosage; dosimetry; health and safety; high voltage; ionization chamber; ionizing radiation; ionometer; radiation quantities and units; radiation standards and protection; radioactivity; radiology; radium; roentgen ray; tubes(x-ray); x-ray measurement; x rays.

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