NISTIR 5556

"From Superconductivity to Supernovae" -

The Ginzburg Symposium

Report on the Symposium held in honor of Vitaly L. Ginzburg

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U.S. DEPARTMENT OF COMMERCE
Technology Administration
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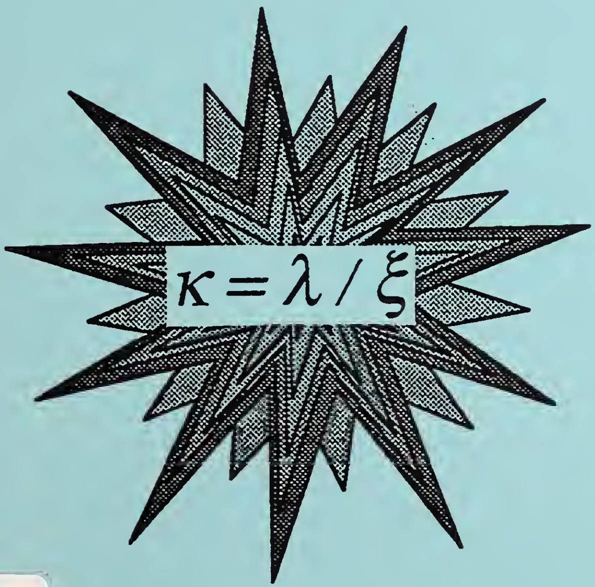

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Mary L. Good, Under Secretary for Technology

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Gaithersburg, Maryland

L. P. Gor'kov
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PREFACE

A one day symposium honoring Professor Vitaly L. Ginzburg was held at the National Institute of Standards and Technology in Gaithersburg, Maryland on May 22, 1992. In addition to honoring Prof. Ginzburg and his many contributions to the world of physics, the purpose of the symposium was to explore future directions for research. This was done in the context of his extensive involvement in the rapid developments in physics in the last fifty years and includes reports of some of those recent developments. The focus was primarily on, but not limited to, those fields where Prof. Ginzburg has made significant contributions, such as superconductivity and astrophysics. A limited number of invited talks were given followed by extended time for discussion. These talks are listed in the Table of Contents.

Discussion was encouraged to identify the historical linking of theory with experiment and also between different fields of physics as exemplified by Prof. Ginzburg's remarkable career, and thus to stimulate the observations of directions for future research. To this end, open discussion followed each talk and a collective discussion was held at the end of the symposium addressing the question raised in Prof. Ginzburg's own address "What problems of physics and astrophysics seem now to be especially important and interesting?" The informal and talkative nature of the day comes through very clearly in the transcribed texts that follow, and the absence of figures, which was done initially for convenience, has only enhanced this feeling. Two of the texts and all of the discussions were edited by myself, as chair and moderator of the symposium, and my apologies are extended in advance to the lecturers and participants if they don't accurately reflect their intent.

The symposium was sponsored by the National Institute of Standards and Technology, Office of Naval Research, Naval Research Laboratory, University of Illinois, University of Maryland, University of Rochester, and SUNY Stony Brook.

A. F. Clark

INTRODUCTION

A.F. Clark

Welcome to NIST and the Ginzburg Symposium, which we have titled "From Superconductivity to Supernovae". I would like to start with a disclaimer. Professor Ginzburg has asked me to clearly state that he has had absolutely nothing to do with the organization and the concept of this symposium, particularly anything that has to do with it being in his honor.

He's right. It's entirely the fault of Ed Edelsack and myself, along with a large and enthusiastic group of his colleagues and friends. It also has little to do with his recent 75th birthday or that this is his avowed last trip abroad. So any resemblance of the recent and exciting results we hear today with any of his past work is statistically insignificant - - - perhaps!

This is strictly, as you see, a do-it-yourself kind of meeting, do-it-yourself name tags, donated doughnuts from the conference next door, and you get to buy your own lunch. It's good food in the cafeteria, and if you came away without your wallet, we'll extend you a short-term loan, as long as you're willing to put up a scanning electron microscope as collateral.

Because he is the last speaker, I would like to introduce Professor Vitaly Ginzburg at this time. With introductions I will follow my personal philosophy of dispensing with all the polite platitudes about where the speakers got their degrees and who their major professor was. The speakers are Bill Little, Ken Kellerman, Vladimir Kresin, James Kurfess, Lev Gor'kov and Reuben Ramaty. We've all been allotted about 45 minutes. They have agreed to talk only 30 to 35 of those minutes, so there will be plenty of time for discussion. In that spirit, they have all asked to field questions immediately in the middle of the talk, don't wait. All we would ask is, if you ask questions, please speak loudly and clearly, tell us who you are. Nothing is more irritating than to have the front row attendee have a one-to-one discussion with the speaker and the rest of us miss it all, so please speak up.

As we stated in the announcement, the purpose is to explore future directions of research. This is exemplified by the talk of Professor Ginzburg, "What problems in physics and astrophysics seem now to be especially important and interesting?". But we ought to do this in the context of his career, which has spanned many different fields, experiment and theory, physics and astrophysics, and like we said in the title, from superconductivity to supernova.

In his talk, Professor Ginzburg will put up a list of these problems for us to consider. We will be asking of the speakers and asking of you, what does your list look like? What would you add to your list, and why? Especially in the context of the recent results we hear today, a lot of exciting things are happening, everything from the expanding frequency spectrum as a probe for astronomy, to the multitude of oxides and organics that have recently been discovered as superconductors. These all challenge our ability to understand.

I'd like to quote an old English poet who said, "the world is full of wonderful and mysterious phenomena, patiently waiting for us to get smarter and smarter". So let us begin by hearing about some of these new, wonderful and mysterious phenomena.

NEW PHENOMENA IN PROXIMITY EFFECT TUNNELING IN HIGH T_C SUPERCONDUCTORS

W. A. Little

It's a very great pleasure to be able to be here, and I thank you for that. I thank you, Vitaly, for being around to have this particular symposium.

I've known Vitaly for about 25 years and have interacted with him in a particular area of physics, superconductivity. It's always been a pleasure to have his view of superconductivity clarifying many of the things that were dressed up in such formalism that it was very difficult to get to the underlying physics. Time and time again, you find a little paper by Ginzburg that puts a finger on the physical problem and clarifies it. Then, I find that in addition to the very narrow field that I've been working in high temperature superconductivity, where both of us have been involved, there are many other fields out there, that literally span "From superconductivity to supernova" where Ginzburg is also involved, and where there are other "Littles" interacting with him in each of these areas, too. Today's program illustrate quite nicely the tremendous breadth of his contribution.

I'd like to try and put in context the technical aspects of what I'll speak about here, in regard to the interactions that we have had together. Then I will show you what it is that we have been doing recently. I had presumed that this would be a fairly general audience, so I will first talk in generalities.

When I first met Vitaly, he was already well known from his work on the Ginzburg-Landau equations, which were written about 14 years before I had any interaction with him. How our interaction came about was as follows. In 1964, I had published a paper where I suggested that it should be possible to synthesize an organic superconductor which would consist of what is basically a polyacetylene, labelled with certain polarizable molecules. The argument in this paper was that the virtual polarizability of these molecules would provide an attractive interaction which could lead to superconductivity by a mechanism other than the phonon mechanism, and give rise to superconductivity at high temperatures. We proposed to replace the phonon with an electronic interaction.

This mechanism is now referred to as the excitonic mechanism, to which I am indebted to Vitaly for naming it so. This paper made three points. One, was that up to then, it was rather an alchemist's approach which had been taken to the making of superconductors - people mixed things together, something came out and you measured their properties. That was very much the approach of Bernd Matthias, a major contributor at that time. The organic chemists' approach, on the other hand, was a much more deliberate approach, and that was what we were proposing.

The second point was that we suggested that superconductivity might occur in a system of limited dimensionality. This wasn't the first time that limited dimensionality had been discussed. In fact, there is a small paper by Ginzburg a few weeks before this in which he discussed superconductivity in a two-dimensional structure. But, what we stressed was that it might be possible to avoid some of the difficulties of theorems which prohibited classical phase transitions in limited dimensions, in the case of superconductivity, and that one might be able to obtain thus, superconductivity in one dimension.

The key point of the paper, however, was that it might be possible to obtain superconductivity at high temperatures. When this was published, we got at best a cool reception in this country, the US. But in the Soviet Union, spearheaded by two of the speakers here today Vitaly Ginzburg and Lev Gor'kov, a strong interest developed. Vitaly in particular, recognized the possibility of using this mechanism, but also recognized that it would be a hard fight to obtain superconductivity in a one-dimensional system and, instead considered a two-dimensional version of the mechanism. He proposed his famous Ginzburg sandwich, which was a structure in which the same type of interaction as we had suggested was obtained between electrons in a thin metal film, induced by the virtual polarization of a medium on either side. This was a more attractive approach in many ways -- it was a physical approach, as distinct from a chemical approach, as he referred to it, but which might nevertheless lead to higher temperature superconductivity. Note that what he proposed was a two-dimensional structure. Later, the discovery of Bednorz and Müeller, high temperature superconductivity highlighted the importance of such two-dimensional systems. We were in good company! Later John Bardeen developed this model further, and we, ourselves, took a page out of this book by including a metallic element in the organic materials we attempted to prepare.

There was a lot of controversy at this time, the 60's, as to whether one could have superconductivity in these types of materials, but there was enough good physics involved that it appeared worthwhile to hold a meeting to discuss the subject. In fact, we were able to get money from the Office of Naval Research to hold such a conference, and it was held in Hawaii. This was an international symposium on the physical and chemical problems of organic superconductors held in 1969. We took a lot of flak, particularly from Bernd Matthias and his cohorts for the fact that there were no such materials, and how could one hold a conference on a material that didn't exist? But it was a good meeting, and what was particularly good about it, was that we were able to get Ginzburg to come to it. For many years, the proof of the existence of organic superconductors was that they brought a person from Moscow to Honolulu.

In looking at a photograph taken of the group at this symposium and thinking about what has happened since then, I've noticed there are a number of interesting people who were at that meeting. Fred Gamble was one of these people.

He discovered superconductivity in the layered compound, tantalum disulfide intercalated with organic materials. This was, perhaps, the first important step towards the organic superconductors and to systems of lower dimensionality.

Mort Labes was also at this meeting. Mort talked about $(SN)_x$ at that meeting, and I thought he was crazy, but since then Rick Green discovered that the polymer $(SN)_x$, of sulfur and nitrogen, with no metallic elements present, was, in fact, a superconductor. It was the first polymeric superconductor discovered. This too was a major milestone on the road of progress that has occurred since then.

Allen Hermann was there; he was younger then! He presently holds the record for the highest temperature superconductor, the thallium superconductor with a transition temperature of 122 K. Don Murphy heads the team at AT&T that did the work on the Fullerenes - the carbon compounds. He was there, too. Felix Bloch was there, and Vitaly, too. Harold Weinstock of the Air Force Office of Scientific Research was there, I think, but, I don't think you gave us any money! You weren't there? Oh, that's right! But I hear you are giving money now!

The odd thing about this meeting was, that at that time there were no such superconductors known. Felix Bloch, who gave the introductory talk on superconductivity, commented on this, yet was a strong supporter of our work. He pointed out that the "Past is but Prologue", - an inscription on the National Archives Building in Washington. When a cabbie was asked what this inscription really meant, he explained that, "You ain't seen nothing yet". I think it's worth looking at what has happened since then, to see how right he was.

At the time we are talking about, 1969, we only had conventional superconductors. Shortly after that was the discovery of superconductivity in $(SN)_x$. Then a few years later, 1980, the first organic superconductor. This field has now taken off and the transition temperatures are up to 13.5 K, I think, or something like that. With the discovery of the ceramics, of course, the field literally took off. Then more recently, there has been the remarkable development of the discovery of the fullerenes. But to repeat "you ain't seen nothing yet"! And maybe, we will see some good ones to follow.

I would like to talk now about our effort to understand whether the high temperature superconductors utilize this excitonic mechanism using the dynamic polarizability of the environment; whether this might contribute; or, if this is not the case, what is responsible for the high T_C superconductors. I will talk about a new phenomenon, involving the proximity effect, in tunnelling into high T_C materials. The experimental work has been done largely by Dr. Matt Holcomb in my laboratory. He was a student of Prof. Jim Collman's of the Chemistry department, and mine in the Physics department at Stanford. The samples which we are using, were provided to us by Dr. Wen Lee from IBM-Almaden, Research Center. Gideon Friedman, a graduate student, helped us with some of the theoretical work, and Catherine Caley worked on some related problems.

As those of you involved with superconductivity know, our understanding of the mechanism responsible for conventional superconductors comes largely from detailed work on tunnelling. For those of you not familiar with this, let me give you a very quick walk through on this subject.

If you take two metals, one a normal metal and one a superconductor, then in the superconductor there is a gap in the density of state that develops below T_C. When you bring the normal metal in contact with the superconductor, with a barrier in-between, and try to pass current from one to the other, you find that there are no states available in the superconductor because of this gap. As a result, no current can flow until you have shifted the Fermi energy of one by applying a voltage, so that the normal metal states overlap with states above the gap of the superconductor, only then do you get an onset of current.

If now you take the derivative of this curve, then you get a curve that mimics the density of states that you see in the superconductor. In addition, you find variations from this ideal picture at higher voltages that reflect the presence of the phonons responsible for the superconductivity. In fact, it is this phonon structure which has shown unambiguously that it is the phonon mechanism which is responsible for the superconductivity of all conventional superconductors. The high T_C materials, on the other hand, have not been so easy to handle. These have insulating, disordered layers on the surface, and because of this and other experimental problems one has not been able to obtain clear-cut curves for these superconductors.

So what we tried to do was to use a different technique, in which we coated the superconductor with a benign metal that does not oxidize readily, and then use this sandwich, which has been called a proximity effect sandwich. This consists of a high T_C superconductor, a film of silver on top of it, and then a contact on top of this which is usually a tin-coated copper contact, or a bismuth-coated copper contact. It can thus be a sandwich of bismuth, silver, and thallium -(2223). We measure the dV/dI response of the junction as a function of the applied voltage V using a lock-in amplifier technique.

The material we are using is Allen Hermann's high T_C compound, the Tl-2223 with a transition temperature of about 122 K. These are thin film samples oriented with the c-axis perpendicular to the film. Because of the high transition temperature of this superconductor, it is possible to use for cooling a micro miniature refrigerator which we have been involved in developing over the last ten years. These are Joule-Thomson refrigerators made of several layers of glass which are etched photolithographically to form a miniature, counter-current heat exchanger. If you pass high pressure nitrogen through it, the nitrogen liquifies by expansion, and the cold stage cools within a few minutes to 77 K. The cold substrate is free of vibrations and can be controlled in temperature to high precision.

The importance of this technique -- and it might also be of interest to the people at the other end of the scale in astrophysics -- is that these refrigerators are extraordinarily quiet. You can do electronic measurements on them and in fact, we do tunneling measurements on them, and we have not been able to detect any vibration during operation at all. Similarly, in optical experiments, crystals mounted on the cold stage have been used for four wave mixing. So, if any vibration is present, it's amplitude must be less than a hundredth of a wavelength, or thereabouts.

The refrigerator uses the Joule-Thomson process. They are usually run using a cylinder of gas rather than a compressor. The tunnelling experiment is done with the sample sitting on the refrigerator. We have a little circuit board with three cat's whiskers that contact the sample, and a thermal link to the individual wires that keep them at the same temperature as the cold stage.

An important element in doing this experiment is that one can control the temperature of the sample to about ten milli-Kelvin over periods of the order of several hours. This turned out to be important for the discovery of the effect I will describe shortly.

I'd like to show you a typical tunneling curve. We compare this with what is probably the best data on high T_C superconductors that had been published in this area prior to our work, showing the differential conductance versus voltage of a particular junction. One junction was coated with lead and one was coated with gold, and this experiment was done between 4 K and 10 K. The curves are not symmetric and they are softened by some type of thermal broadening. Many people have done such tunneling and have run into the same problems due to impurities on the surface, which makes it difficult to get reproducible junctions.

Now, with the tunneling set-up that we have used, at 78 K, the results that we have obtained are shown next. You can see the striking difference between what I showed you beforehand. Here we are scanning from minus 250 mV to 250 mV. What you see is an array of interference fringes. But the most striking feature about this is that these lines, and, in particular, the first one, is about 0.5 mV wide. It is half a millivolt wide at a temperature of 78 K. In fact, if you go to 96 K, you can see the same thing with a line width which, if you think of it as being thermally broadened, has a thermal broadening of about one degree, so there is something new here. Whatever the effect is that is involved, it does not involve the quasi particles. It must involve a condensate somewhat like the Josephson effect for the location of the first line is determined to within a few microvolts, and appears to depend only upon the difference of the chemical potential of the two metals and on the magnitude of the superconducting gap. In fact, we will argue that it involves twice the superconducting gap.

In doing this experiment, the reason it was so important to hold the temperature to within a few milli-Kelvin was that the observed line moves almost ten line widths in 1 K. So if you have a small drift in temperature as you have in many of our competitors' refrigerators, you will find that the line is completely smeared out and all you see is noise. Holding it with this very good temperature stability, it was possible to see the effect rather nicely.

We show a scan from about 96 K down to 74 K. You see several interesting features. First, starting at about 90 K, the first peaks appear. Then you see the appearence of interference fringes. Then quite abruptly, at about 75 K, they disappear and below this they are totally gone. At that point, a tiny dip develops at the zero voltage position. We notice also that structure exists up to at least about 200 mV and there is a bump that moves out beyond 250 mV.

One might have thought that the spikes are caused by what we call a phase slip phenomenon, where you pass a current between two superconductors and then you have breakdown, and the cycle is repeated with increasing current. Spikes like this have been found before. However, if you apply a magnetic field transverse to the sample, you find that it takes only a few hundred Gauss, about 400 gauss, to destroy the fringes. This is not simply a phase slip problem. It looks like the destruction of phase coherence which then destroys the peaks.

I show again the greatly enlarged curve that we obtain for the narrowest line. This, taken at 77 K has a thermal width of 1.5 K, if you think of the line as thermally broadened. So the problem is, how can one understand the existance of such a sharp line. Let me go through a little bit of the mathematics to attempt to explain the importance of this phenomena. What we have is not a tunneling contact, but rather a metallic contact. We have a metal in intimate contact with another metal. It is what Sharvin described -- and what we now refer to as a Sharvin contact. One can think of such a contact as being an aperture through which electrons are simply injected into the other medium. This gives rise then to a resistance which is a geometric resistance due to the number of carriers that can emerge from that hole.

Now Michael Tinkham, and his group at Harvard, have calculated the details of the behavior across such a junction. One can show that the current which flows depends upon a certain transmission factor, multiplied by the difference of the Fermi functions of the quasi-particles on either side of the junction. As you increase this difference of voltage, the current increases. If, then you take the derivative of this current with respect to the voltage, there is only one place where the voltage appears, in the function. It is in the Fermi function of one side and hence you get the derivative of a Fermi function. So that the differential conductance, the reciprocal of which is the differential resistance, is proportional to the transmission factor folded with the derivative of a Fermi function. The derivative of a Fermi function has a width of 3.5 kT. At 75 degrees, this is about 25 mV wide, so this would completely mask any structure in the transmission factor if one was dealing with such a quasi-particle distribution.

This has suggested to us that in our sandwich the bismuth contact is not in the normal state but has superconductivity induced in it by the Tl-2223 via a long range effect through the normal Ag film.

One can think of this as the silver being infected with a superconducting "virus" which infects the bismuth and makes it superconducting but leaves the silver normal! It's not so crazy to think of it in that particular way. The picture is the following. The gap in a superconductor is the product of the pair amplitude times an interaction potential. The Gorkov function is large in the thallium superconductor. We claim that this Gorkov function propagates through the silver and into the bismuth, and in the bismuth, this function is also large. In the silver, the pairing interaction is vanishingly small. While in the bismuth, it is extremely strong, because bismuth has one of the strongest pairing interactions of all the elements. So if you infect the bismuth with the pairing correlation of the Gorkov function, it will exhibit a gap because the interaction acts like an egg beater and mixes up the pairs giving rise to a gap.

So if this exists, then one can look at what happens when one applies a voltage across the junction. If you think of the bismuth tip as a region in which there are bound pairs, and now you apply a voltage across that tip, you can accelerate one member of the pair, breaking the pair. That costs you twice the energy of the gap. You break the pair and accelerate one member across the silver to the Tl-2223 superconductor. For this to become a quasi-particle in the superconductor it must Andreev reflect at the superconducing interface giving a reflected hole. In each case, what you find is, an electron comes across the junction, forms a pair, and because you only provide one electron, you pay for it by creating a hole propagating back across the silver. The hole goes back and may then eliminate any other electron, and in this way transports a pair from one side to the other, or may itself Andreev reflect on the Bi-condensate creating an electron that interferes with the original creation electron. So depending upon the momentum or the phase difference around the loop, you can get constructive or destructive interference in the current.

I'll give you a physical picture in a moment, but if you go through the calculations for this, one finds that in order to get a higher resistance, you must have destructive interference of the pairs as they go around this loop. The condition for destructive interference is the usual one, $2\pi\ (n + \frac{1}{2}) = 2\kappa d$, where d is the film thickness and $2\kappa d$ is the total phase change round the loop. That's the phase shift around the loop. By simple calculation of acceleration of the particle, one can evaluate κ, which is related to the applied potential after you've paid for the cost of breaking the pair. So this indicates that you'll get destructive interference for a series of voltages above that necessary to break the pair. For a silver film of thickness, 2,000 angstroms, you get a periodicity of 7.2 mV for the peaks. The experimental measurements for a series of films merging from 500 Å to 2000 Å in thickness agree well with this relationship.

The effect has been seen with film, with a thickness of as much as 2,000 angstroms, while on the other hand, the coherence length of the superconductor we are dealing with, is about 15 Å in the a-a' plane and 3 Å in the C direction. Our effect extends over a distance 10-50 times larger than what we've usually thought of as being the range of the proximity effect.

What I would like to do in the last part of this talk is to show you how in fact the conventional theory can be adapted to explain this phenomenon. But first, let me say that the phenomenon that we appear to be seeing is simply the pair analogue of Newton's rings in optics. We are looking at the interference of the pair wave function in the silver, and we are varying the wavelength of this function by varying the applied voltage.

Let me amplify on the theoretical model. If you inject from your contact an electron into a good metallic junction of a normal metal with a superconductor, then you run into a peculiar effect. If it is a low energy electron, it sees that there are no states in the superconductor to which this electron can go. So you might expect then that there would be a resistance to the flow of current from a normal metal into a superconductor. That doesn't make sense, though, because the superconductor is a good conductor. What happens in fact is that the electron produces a pair which can propagate in the superconductor. But it doesn't have a pair to give, so it has to create a pair plus a hole, which it can. The hole comes back. This corresponds to Andreev.

Now, it was pointed out some 20 years ago by De Gennes and Saint James that these Andreev reflected holes and particles form a bound state in the normal film. What perhaps is not so well appreciated is that this is a pair, the electron and the hole which is a backward going electron, and the net effect of this is that it is a pair which is trapped below the gap in the normal metal.

The wave function of that pair looks like the following. The lowest bound state with index η equal to zero, has an amplitude that is approximately constant in the normal metal, and then there it is attenuated as you go into the superconductor because it cannot propagate. The second, with η equal to one, has one node and has the opposite sign to the first at the free surface of the normal metal. All states of even η are positive and those of odd η, negative on this surface. For each of the bound states, the pair amplitudes are all in phase in the immediate neighborhood of the superconductor. But when you are at the free surface of the metal, the different states alternate in sign. To get the pair density, we must add these amplitudes and then square them. So you get destructive interference between the two states of different sign on the free surface. As a result, one finds that in the immediate neighborhood of the superconductor, constructive interference give a large amplitude and the proximity effect is large, but as you go deep into the normal metal, and finally reach the free surface, destructive interference between the states reduces the pair amplitude to zero. This is the basis of the usual argument why the proximity effect does not propagate through the normal metal.

But the cuprates that we are dealing with are not normal metals.

We're dealing here with highly anisotropic materials. Silver, on the otherhand, is isotropic, and it is in contact with a c-axis oriented thallium film, the Fermi surface of which is more or less cylindrical; it is like a square cylinder. When you satisfy the boundary conditions on this surface, in order to Andreev reflect, the momentum parallel to the interface must be conserved. Because the energy depends only upon the momentum in the a-a$'$ plane for the thallium superconductor, the only time that you can get Andreev reflection is in a narrow range of incident angles.

This then puts a constraint on the possible value of the angle. So when you integrate over the angle of incidence, theta, the only terms that contribute are those in a narrow region. If you take the full temperature dependence into account, of the coherence length and the quasi-particle excitation, the bound states give a significant contribution to the pair amplitude at the free surface of the normal metal.

If you now compute the amplitude as a function of temperature, one finds for a film which is some 50 to 100 times the usual proximity effect thickness, say 200 nanometers thick, you find that the Gorkov function at the free surface becomes large. Let us return then to our earlier results.

Let us look at the curve that we had previously shown of the interference fringes as a function of temperature and return to the question whether there is a different pairing interaction in these high T_C materials. Let me remind you of the work of Scallapino and Shrieffer many years ago, on the phonon mechanism in conventional superconductors. The gap function was found to be a function of energy. It grows with energy and then reverses sign right at the maximum value for the phonon frequency. The imaginary part of the gap grows to a peak, oscillates a little, and then goes to zero.

Now, this would indicate that the gap function which determines the reflection coefficient for Andreev reflection should go to zero at about 70 mV, or at least becomes complex. It should change its phase. This suggests then that the curve of the differential resistance versus voltage should be crossed by a line, free of peaks, which would reflect the highest phonon frequencies at about 70 millivolts, plus the energy of the gap, or would be at 90 millivolts, or so. There should be a line which would mark this change of the reflectivity. But you see nothing. There is no indication of a such line anywhere near that energy.

So it suggests that the gap function does not pass through zero, but rather that there is another mechanism at higher energies which raises it and keeps it from doing so. So here is perhaps a hint that there is some type of electronic interaction present in these materials. I don't know whether it is magnetic or whether it is of the excitonic type, but it seems to indicate there is some additional term present.

To finish up, let me say what we are trying to do now. This is to study the reflectivity of silver films backed by a high temperature superconductor, because one can show that the presence of the gap in the superconductor should influence the density of states on the surface of the silver. So it may be able to give us a clue as to the gap structure at energies well above those that we can observe with tunneling. You cannot tunnel at too high a voltage because of the voltage breakdown of the junction itself. So our hope is to be able to see from these optical experiments a reflection in the gap, of the presence of some type of electronic interaction at higher energies. This will then relate back to what Ginzburg and I have discussed many times previously and what we have been searching for, for these many years.

I'd like to thank you very much for your attention.

DR. CLARK: Questions?

PARTICIPANT: Why do you use the thallium compound? Is it poisonous?

DR. LITTLE: It is the layered Tl-2223 compound. But thallium is not that poisonous. If you don't heat it, you don't drive off the thallium, and it has a superconducting transition temperature which is very high. We have also done the experiment on the bismuth-lead-2223 compound, as well. I think thallium has been somewhat overrated as being so dangerous. There's a material which is used in just about every organic chemistry laboratory, for infrared studies they use windows of thallium bromide. They don't label it as thallium, but it has a lot more thallium in it than this material. You don't put it in our sputtering apparatus. Dr. Wen Lee at IBM has a special system that does that, so he takes the risk, we don't!

PARTICIPANT: Why did you use a system like that instead of something like silver?

DR. LITTLE: The reason we used bismuth was that we were trying to go to the highest possible voltage to see the electronic interaction at high energies. Bismuth has a very small number of carriers, so if you apply a given voltage across it, then the current is small. We found that it worked with tin, too, but it doesn't work with most other materials that have oxide on their surfaces. Aluminum doesn't work, lead doesn't work either. We have not tried silver on silver yet.

PARTICIPANT: What was the magnetic effect that you alluded to? What's the physical basis of it?

DR. LITTLE: We thought that bismuth must behave like a conventional superconductor but in which there is an induced gap. In the BCS theory, you have a self consistent equation which bootstraps the gap. In this case, you are being fed by correlated pairs from the high T_C material. So the bismuth is simply responding. It is like a driven oscillator rather than a spontaneous oscillator.

So we thought that in the bismuth, the critical field might be on the order of a few hundred gauss. We applied a magnetic field both in the normal direction and in the transverse direction, and found that the interference fringes go away. I think the proper understanding of that is that the terms that contribute to the gap, the pair function, come from those states which propagate at a given angle with respect to the normal. But that is a cone of states. So when you apply a magnetic field to this, you advance the phase of one set of states and retard the phase of the other, so that between these, if in the flux enclosed area, when this becomes comparable to a flux quantum, then the interference effect should go away. That's about the magnitude of the effect.

We don't understand it fully. We simply observe that a small magnetic field destroys it, but that the T_C of high T_C material is virtually unaffected. So it seems to be weak superconductivity that we've seen, even though it exhibits a gap which is 100 K or more in size.

PARTICIPANT: Could you characterize it with a critical field curve?

DR. LITTLE: We haven't done that. We would have to do a series of scans to do so. As you can see from the temperature scan, the structure does follow the usual critical curve. It perhaps moves faster than BCS, but in these experiments, we didn't have the samples adequately thermally anchored to the cold stage. In more recent experiments we see that the onset of the interference fringes, instead of being at 90 K, in fact, appear as soon as the film becomes a superconductor. At 119 K, the first peaks begin to appear. It's absolutely flat above T_C.

PARTICIPANT: Do you co-deposit the Ag film?

DR. LITTLE: No, it's done subsequently. But we back sputter the thallium with an argon discharge, for 30 seconds before depositing the silver.

RADIO GALAXIES, QUASARS AND SUPERLUMINAL RADIO SOURCES

K. I. Kellermann

Like the previous speaker, I am going to assume that this is a general audience, so I will start at the beginning.

In the early 1930's, Karl Jansky used a simple radio antenna to study the source of interference to short-wavelength trans-atlantic telephone communication. In the process, he discovered radio emission from the center of the Galaxy. This important discovery made with a very simple instrument changed astronomy and astrophysics in several very fundamental ways.

First, up until that time, all astronomical observations had been made in the narrow window of the visible spectrum, between 3,500 and 7,000 angstroms. Jansky's radio observations opened up a whole new part of the electromagnetic spectrum, from wavelengths of the order of a hundred meters to millimeter and even sub-millimeter wavelengths. With the introduction of the space age, astronomical observations now extend to the infrared, the ultraviolet, X-ray and now even gamma-ray parts of the electromagnetic spectrum.

However, the radio observations are still unique in several respects. Because it was the first opportunity to look at the universe through a new part of the electromagnetic spectrum, a lot of very important discoveries were made at radio wavelengths--discoveries of new kinds of objects that were previously unknown, either because they are invisible or at least inconspicuous at optical wavelengths. For example, radio galaxies, quasars, pulsars, neutron stars, interstellar molecular masers, radio bursts from the sun, gravitational lenses and the microwave background radiation were all unknown prior to their discovery by radio astronomers.

The quasar 3C273, for example, is a fairly bright star, and is easily observed with a small telescope. But 3C 273 looks just like any other star of the same brightness, and it was unrecognized as a quasar until it was studied by radio astronomers. Similarly, effects of the cosmic background radiation had been previously observed in the infrared, but the effect was unrecognized until its dramatic discovery by Penzias and Wilson in 1965 at radio wavelengths.

Even though we are now able to study the universe throughout the whole of the electromagnetic spectrum, the radio observations are still unique. Radio waves pass through the atmosphere relatively undisturbed, and radio astronomers can work effectively from the ground. By contrast, the atmosphere is opaque to infrared, ultraviolet, X-ray and gamma ray radiation. Astronomical observations at these wavelengths must be made from space where it's very expensive. Radio telescopes are built on the ground where it's not only much cheaper, but where it's possible to modify the instrumentation either to keep up with improving technology or to fix things when they don't work. That is very much more difficult, or at least very much more expensive to do with telescopes in space as we have recently learned from the problems facing the Hubble Space Telescope.

Karl Jansky's antenna worked at 20 MHz or about 15 meters. It was speculated that if the radio emission observed by Jansky was due to thermal radiation from dust in the galaxy or free-free emission from an interstellar medium, then at shorter wavelengths, the intensity should be greater. In fact, the reverse was the case. When Grote Reber attempted to observe at short wavelengths he was surprised to find that the radiation was weaker, not stronger. We now recognize these early radio observations as the beginning of *non-thermal* astrophysics. Prior to Jansky's pioneering observations, essentially everything that was known in astrophysics was due to thermal phenomena. We now recognize a wide range of non-thermal phenomena in the universe, of which the non-thermal radio emission from galaxies and from quasars is the classical example.

For a long time, the mechanism for non-thermal radio emission was hotly debated. Vitaly Ginzberg and his colleagues in the Soviet Union were among the first to recognize the application of the synchrotron radiation mechanism, or what they called magnetobremsstrahlung in interpreting the celestial radio emission. Synchrotron radiation had been known for a long time; the mathematics had been worked out in the early part of the century and applied to particle accelerators after the war. But it was in the Soviet Union, largely by Ginzburg and his collaborators, who were able to wade through the mathematics and put the theory in a form that could be related to observations and interpreted in terms of the physical conditions. They showed that the power law radio spectrum and the power law distribution of cosmic ray particles were related by the equation

$$\alpha = (1-\gamma)/2$$

where

radio flux density is proportional to $(\text{frequency})^{\alpha}$ and

$N(E) = KE^{-\gamma}$.

As the sensitivity and resolution of radio telescopes improved during the 1950's and the 1960's, many radio galaxies, and later quasars, were discovered. Radio galaxies and quasars are very strong sources of radio emission. They may have luminosities up to the order of 10^{46} ergs per second, several orders of magnitude more than the integrated optical radiation from starlight.

By 1960, it was generally accepted even in the West that we were observing synchrotron radiation. However, understanding the source of the radiation mechanism is only part of the problem, since synchrotron radiation is very inefficient. The characteristic time scale, τ_c, for energy loss by a relativistic electron radiating at frequency, f, is given by

$$\tau_c \sim B^{-3/2} f_{GHz} \text{ years.}$$

For typical values of f ~ 1 GHz and B ~ 10^{-5} gauss, the characteristic lifetime is more than 10^7 years. So radio sources radiate for a long time, and it takes a large reservoir of energy to provide the high observed luminosity. The total stored energy is very great--about 10^{60} ergs in the form of relativistic particles and magnetic fields.

The energy problem is further exacerbated if the radio source expands from a small volume co-located with the underlying energy source. If the cloud of relativistic plasma expands adiabatically and if magnetic flux is conserved, the radio emission will decrease roughly as the fifth power of the size. So if you extrapolate 10^{60} ergs in a 100 kiloparsec source backward to a few parsecs, you get an absurd requirement on the initial energy supply.

High resolution radio images of radio galaxies and quasars reveal thin jets extending from the center to the outer radio lobes. These jets are thought to carry energy from the central engine to the outer regions without significant energy loss. But, for many years, not much was known about the detailed structure of the radio sources, partly due to the inadequate resolution of radio telescopes. In fact, for a long time, textbooks of astronomy often argued that the resolution of radio telescopes is inherently poorer than that of optical telescopes, simply for the reason that radio wavelengths are much longer than optical wavelengths, by a factor of 10^5. Thus, for a given resolution, a radio telescope, must be bigger than an optical telescope by a factor of 10^5.

Actually, as it turns out, this is not the case for two reasons. First, at optical wavelengths, the resolution is not limited by diffraction, but by turbulence in the atmosphere, or seeing, to about one second of arc. At radio wavelengths, path length fluctuations in the atmosphere are small compared to a wavelength and do not destroy the image.

Secondly, at radio wavelengths, it's much easier to build diffraction limited instruments. At wavelengths of the order of a few centimeters, the precision required to maintain coherence is of the order of a millimeter, whereas, optical telescopes require much greater accuracies to achieve the same resolution.

In a sense, the history of radio astronomy has been one of exploiting the progress of technology to achieve ever and ever better angular resolution. The largest conventional radio telescopes are about 10^4 wavelengths across at their shortest operating wavelengths, so have resolutions a bit better than a minute of arc, or only slightly better than the unaided human eye.

But, radio astronomers can obtain much better angular resolution not by building bigger antennas, but by building large arrays of smaller antennas. The Very Large Array (VLA) in New Mexico, contains 27 antennas, each 25-meters in diameter and can be configured to give radio images equivalent to a telescope 35 km in diameter. At its shortest operating wavelength of one centimeter, the corresponding resolution of the VLA is only a few tenths of an arc second, or better than that of any other ground based telescope.

Due to the incompletely filled aperture, arrays like the VLA have high sidelobes or spurious responses compared with parabolic dishes. Because the geometry of the array is well known, it is possible to deconvolve or CLEAN the so-called DIRTY image to remove the effect of the side lobes to give very much improved image quality.

The CLEAN image, however, is still degraded by the effects of the atmosphere. Path length fluctuations in the atmosphere which amount to a few millimeters, which is a small but still significant fraction of a wavelength, produce distortions and blurring of the image. Because the number of antennas (N = 27 for the VLA) is small compared to the number of independent interferometer pairs, ((N(N-1)/2 = 351) for the VLA), the number of unknown phase errors over the antennas is much smaller than the actual phase measurements of the source itself, and it is possible to iteratively solve for both the phase errors and the brightness distribution of the celestial source.

Although this operation is very intensive of computer time, radio images from the VLA as well as from other large synthesis radio telescopes in the UK and in the Netherlands show the detailed structure of radio galaxies and quasars. Typical images show two radio lobes symmetrically located about a compact central component which is often identified with the active nucleus of the parent galaxy or quasar. The radio lobes are characteristically separated by a few hundred kiloparsecs. Often a thin jet is seen extending from the bright central core toward one of the extended radio lobes.

It's now generally accepted that the source of energy for the extended radio lobes is in the associated active galactic nucleus or quasar, and that energy is transported from the central engine to the outer lobes along the jet. The compact radio sources located at the active center are so small that the synchrotron radiation is self absorbed, that is it becomes optically thick below a critical frequency, ν_c, given by

$$\nu_c \sim 8\ B^{1/5}\ \theta^{2/5}\ S^{-4/5}\ \text{GHz.} \tag{1}$$

For typical values of B ~ 10^{-5} gauss and a cutoff frequency, $\nu_c \sim 1$ GHz and $S_{max} \sim 1$ Jy, the characteristic angular size is only on the order of one millarcsecond. This is a very small angle way beyond the limit of even the largest conventional array such as the VLA.

To study the compact sources found in active galactic nuclei and quasars where the energy generation occurs, one needs very much higher angular resolution, or a radio telescope bigger than the VLA by a factor of ten to a hundred. It is not practical to build such large arrays by directly connecting the individual antenna elements such as in the VLA. Instead the data is simultaneously recorded with high-speed tape recorders at each of the antennas, synchronized by accurate atomic clocks. The individual tapes are then brought to a central playback facility where they are simultaneously played back to give the data used to synthesize the images in the same way as the conventional arrays. This so-called technique of very long baseline interferometry (VLBI), gives angular resolution better than a thousandth of a second of arc--a resolution much better than any telescope operating in any other part of the spectrum, in space or on the ground.

VLBI images of the central cores of radio galaxies and quasars show that the compact nucleus breaks up into a bright core and a small jet-like feature--only a few parsecs long--which is always located on the same side of the central core as the much larger extended jet and which points toward the extended jet.

In essentially every source that has been observed--in every radio galaxy and quasar the jets are lined up. This means that the relativistic jets which extend hundreds of kiloparsecs away from the central core, are collimated within a very small region, only a few parsecs, or less, in extent. Also, each source must contain a preferred axis which remains fixed in space over the lifetime of the source--or at least 10^7 years.

If we examine these compact cores over a period of time, we find that they are all variable. Their flux density may change by factors of two and three over time scales of the order of months to years. Trivial light travel time arguments then suggest that the source dimensions must be less than one light year. Many of these variable radio sources have very large observed redshifts so they are thought to be very far away. Thus, they are both very powerful and very small--under 0.001 arc seconds. This means this that the surface brightness--the luminosity per square arc second is extraordinarily high. So high in fact, that the radiation field is so intense, that the relativistic particles would lose more energy by inverse Compton scattering than by synchrotron radiation, and the radiation would escape at X-ray wavelengths, not radio wavelengths. Some workers used this as an argument in favor of a non-cosmological interpretation of quasar redshifts, since if the quasars were allowed to be closer than indicated by their redshift, their angular size could be larger, and the inverse Compton catastrophe avoided.

VLBI observations made over a period of time show that the individual components in the compact cores move with surprisingly large angular velocity. If the quasars are at cosmological distances, the corresponding linear velocities are five to ten times the speed of light. For a while, this was used as an argument against the cosmological red shifts and interpretations were suggested in terms of gravitational lensing or even speculation about radiation from tachions. The correct interpretation was described in a paper by Ginzburg and Syrovotsky in the 1969 issue of Annual Reviews of Astronomy and Astrophysics. In this prescient paper, they described in some detail the radiation from relativistically moving sources, where the motion is oriented nearly along the line of sight. Because the radiating source is almost catching up with its own radiation, any event appears to a fixed observer, to occur in a very much shorter time, and this can give the illusion of so-called super luminal motion. For a source moving with a velocity, v, in a direction, θ, with respect to the line of sight, the apparent transverse velocity is given by

$$v_{app} = \frac{v \sin\theta}{1-(v/c)\cos\theta} \qquad (1)$$

If the radiating plasma is moving close to the velocity of light, and if the direction of motion is close to the plane of the sky, then the apparent velocity is close to c. But, when the motion is close to the line of sight, the apparent velocity can be very much greater. The maximum apparent velocity occurs where

$$\sin\theta = 1/\gamma$$

and is $v = \gamma c$, where $\gamma = (1-v^2/c^2)^{-1/2}$.

The problem with this interpretation is that well over half of all compact radio sources that have been observed in detail show superluminal motion. The probability that any source selected from a randomly oriented sample will be favorably oriented to give this apparent effect is very small, and is given by

$$P = 1/2\gamma^2.$$

So for $\gamma \sim 7$, the a priori probability of observing a superluminal source is only about one percent.

Since the radiation from a relativistically moving source of radiation is beamed along the line of sight, in any flux limited sample, sources which are favorably aligned will be preferentially observed. Arguments of this type have led to the so-called unified models of radio galaxies and quasars, which interprets the different morphologies that are observed among these objects, or the difference between radio galaxies and quasars, or the difference between very luminous sources and faint sources, not as intrinsic effects, but just as the effect of viewing angle. For example, if a relativistic beam is moving along the line of sight, then the core will appear very bright with respect to the outer lobes. On the other hand, if it is oriented in the plane of the sky, then the outer lobes will be strong relative to the core. This has led to a whole industry of trying to explain all of radio astronomy and even all of astrophysics, as just the result of simple orientation effects.

These unified models have had a lot of successes, but there are also a lot of problems. For example, 3C 273, is the brightest quasar in the radio sky. That can be understood since it is a superluminal source, so the radiation is beamed toward us and appears enhanced. But, it is also the brightest object in the optical sky, as well as in the infrared and X-ray sky. So if it appears so bright at radio wavelengths due to relativistic boosting, perhaps relativistic boosting is important at these other wavelengths as well.

However, there are some problems with this interpretation as well. 3C

273 is also the brightest quasar in the sky in terms of its optical emission lines. The emission lines are not Doppler shifted; therefore they are not relativistically boosted.

A further problem is that the relativistic beams must be intrinsically two sided in order to supply energy to both extended radio lobes. But observations always show one-sided jets, presumably because we only see the side that's oriented toward us. We don't see the jet pointed in the other direction since it's Doppler boosted away from the observer.

But what about the extended jets which are always oriented on the same side of the active nucleus or the quasar as the compact jets. If the one-sided appearance of the compact jets is due to Doppler boosting, then the extended, intrinsically symmetric extended jets must also be relativistic. Some astrophysicists have trouble accepting bulk relativistic motion which extends hundreds of kiloparsecs from the energy source. I think there are two schools of thought. One says, if a jet starts out with bulk relativistic motion, it will stay relativistic. Others argue that turbulence and shock waves would break up a relativistic jet and it could not propagate out to those large distances.

Whatever the detailed explanation, I think one thing is clear. Relativistic effects do play an important role at least at radio wavelengths, and Vitaly Ginzburg was one of the first people to appreciate the important role of bulk relativistic motion in astrophysics.

I recall that when I met Dr. Ginzburg during my first visit to Moscow in 1965, I also met Leonid Ozernoi who is sitting here today. Ozernoi, then a student of Ginzburg, was concerned about understanding the complex spectra of radio sources. He argued that he could explain the detailed shape of radio source spectra with multiple "humps" as the result of differential Doppler shifts of relativistic components flying apart with different projected velocities along the line of sight. I remember walking around the streets of Moscow with Leonid. In between visits to tourist sites we discussed relativistic motion. I argued that due to differential Doppler boosting, we would see only one component, the brightest, but Leonid claimed that differential evolution would compensate the differential Doppler boosting.

I don't know if Ozernoi's detailed model was correct or not, but certainly he was a lot closer to current ideas than I was. That was 25 years ago, and I apologize for not believing you, Leonid. You were right!

VLBI images have very high resolution, but relatively poor image quality. The reason is that the antennas are not optimally located to form good images. Instead they are found at existing radio observatories which were built to be convenient to some university or to escape interference from man-made noise. Also, most of the antennas that are being used for VLBI do not work well at the shortest wavelengths where the angular resolution is best.

By the end of this year, we will see the completion of the Very Long

Baseline Array, or VLBA, which began in 1985 with support by the National Science Foundation. The VLBA consists of ten antennas located throughout the United States from the Virgin Islands to Hawaii. Each of the antennas will be controlled from a central control point located in New Mexico coincident with the VLA control center. The magnetic tapes recorded at each antenna will be sent back to the control center where they will be correlated and used to form high resolution high quality images of a variety of celestial radio sources. But still, that's not quite good enough to study the active cores of some radio galaxies and quasars. Some radio sources are observed to be smaller than the limits set by inverse Compton scattering. This would provide convincing evidence for Doppler boosting and bulk relativistic motion. But this requires angular resolution better than can be obtained from Earth-based interferometers.

About a decade ago, radio astronomers began to discuss the possibilities of space-based VLBI. But, in both Europe and in the United States, we were unable to convince our respective space agencies of the merit and importance of orbiting VLBI or OVLBI as it has come to be known. Funds for space astronomy are necessarily limited, so only those projects that are deemed to be the most important get funded. OVLBI is in competition with X-ray, infrared, UV, and gamma ray astronomy. As discussed earlier, radio astronomy has done very well from the surface of the earth. We don't need to go into space to survive. At other wavelengths, where the atmosphere is opaque or greatly deteriorates the image, there has been great pressure to locate instruments in space. So with finite funds, radio astronomers in this country and in Europe have been unable to develop OVLBI.

However, our Russian colleagues at the Space Research Institute in Moscow have been more successful. Under the leadership of Nikolai Kardashev they were able to develop the so-called Radioastron project which is designed to place a ten meter antenna into high earth orbit extending 70 to 100 thousand kilometers from the Earth. Working with ground-based radio telescopes around the world, Radioastron will give an order of magnitude increase in baseline over earth-based interferometers.

VLBI, by its nature, has been an inherently international science. Unlike many international science projects which have been formulated primarily to share costs or to obtain government support, VLBI requires the collaboration of antennas and radio astronomers from around the world for its scientific success. Much of the advanced instrumentation needed to optimize an OVLBI mission is not available in Russia, so collaboration with Western countries is desirable. Fortunately, Russian and American radio astronomers have had a long history of collaboration in ground-based VLBI and we looked forward to extending this cooperation to space VLBI.

Although the instrumentation needs were modest in terms of cost, particularly compared with the high cost of the spacecraft, the antenna, and the launch. Until recently the equipment was considered "sensitive technology" and not available for export to the former Soviet Union. So we were unable to get even modest support to participate in Radioastron.

I am not sure whether it was due to the beginning of the changes that are now occurring in the former Soviet Union or the departure of Roald Sagdeev from the Space Research Institute, but several years ago our radio astronomer colleagues in Moscow also fell on hard times, and the support for their work began to dry up. Fortunately, they were invited by Vitaly Ginzberg to come to his Lebedev Institute. Ginzburg and Andrei Sakharov wrote a letter to Admiral Richard Truly, the then Administrator of NASA, to urge that the United States participate in the Radioastron mission.

Somehow this letter ended up on the desk of Vice President Dan Quayle, who is head of the National Space Council. Either the Vice President or his advisors decided that there was some political advantage in spending a few million dollars in support of space VLBI so suddenly we had support from NASA to participate in the Radioastron project, which is now scheduled for launch in 1995. But, we have also had increased bureaucracy and control typical of large international space ventures, unlike our earlier collaborations between Russian and American scientists in ground-based VLBI.

I think every time I have met Dr. Ginzburg, either in Moscow or in this country, he always explains that he is not an astrophysicist, but a physicist. Nevertheless, he has not only made a number of contributions to theoretical radio astronomy, but more recently he seems to have reverted back to the beginning of his career when he participated as an observer in a 1948 Solar eclipse expedition. He has become involved again in observational radio astronomy, and in the crucial role of obtaining financial support from the Russian government. Remarkable as this is under the present economic circumstances, it is even more remarkable that he has been able to raise financial support for observational radio astronomy in this country as well!

For that, we thank you very much!

EXOTIC PROPERTIES OF HIGH T_C MATERIALS AND THE GINZBURG PARAMETER

V. Z. Kresin

The first scientific talk in my life I gave at the famous Ginzburg seminar, and today I feel myself much younger because of such memories.

Before discussing the famous Ginzburg parameter, I would like to talk briefly about another phenomenon which is called the Ginzburg seminar. Vitaly Ginzburg has been running this famous seminar for almost 40 years. It's a unique event. The seminar takes place each Wednesday morning, and several hundred people are coming to the Lebedev's Physics Institute where Ginzburg is head of the theoretical section. The seminar lasts two hours. Part of the seminar is similar to a journal club; one can hear also a lot of original talks.

For example, I remember a time, when the BCS theory was created. There was one preprint of the theory in Moscow, and Ginzburg was the owner of this preprint (There wasn't any copying machine). The paper wasn't published. All of us, young students, had a unique opportunity to learn about the famous BCS thanks to three Ginzburg's seminars.

It is a difficult task to give a presentation at this seminar. There were many occasions when a speaker understood that he missed a real opportunity not to give his talk. There are many sharp questions, particularly from the Chairman.

As I mentioned, the seminar takes place each Wednesday, regardless of Ginzburg's presence. But there is a big difference between a seminar which he is running, or if he is absent. Unlike Vitaly, I can make such judgment, because I was attending the seminar in his absence, but he never did. There is some kind of chemistry about this man. Everything is more interesting, is much more clear, not boring; the seminar is an entirely different phenomenon in his presence.

Let me turn to the main topic. Many years ago, in 1958, the All-Union Low Temperature conference was held in Tbilisi, Georgia. I was a young student, and it was the first conference in my life. I remember a brilliant talk given by Ginzburg at this conference. Ms. N. Ginzburg also participated in the Meeting.

Vitaly Ginzburg was talking about liquid He. Usually we are stressing a similarity between superfluidity and superconductivity. But Ginzburg was focusing on the difference between them. The difference is very simple. Heat capacity has a sharp jump in superconductors, but if we study liquid helium we don't observe such a sharp jump. We have so-called lambda singularity, so the picture is entirely different. He was trying to explain the reason for the difference.

Ginzburg stressed the importance of fluctuations. Because of fluctuations, there is a difference between liquid helium and superconductors. For superconductors, this factor is reflected in the famous Ginzburg parameter. It contains the ratio of critical temperature over the value of the Fermi energy. For ordinary superconductors the Fermi energy is large relative to critical temperature; therefore, the Ginzburg parameter is very small. As a result we have a sharp transition.

Superconductivity was born for a second time after the discovery of high temperature superconductivity. Everybody knows a famous dependence of critical temperature vs. time. We obtain similar dependence if we plot the number of publications on superconductivity as a function of time!

After the discovery, Dr. S. Wolf (NRL) and myself start working in the field. We were trying to evaluate the major normal parameters of the oxides. First of all, we came to the conclusion that Fermi surface exist in this materials. But what is exotic in these materials is the uniquely small value of the Fermy energy. It turns out to be almost two orders of magnitude smaller than for usual metals. We are dealing with the Fermi "puddle", rather than with the Fermi "sea".

The Ginzburg parameter has became a very important quantity. Our friend, Guy Deutscher published the paper "Ginzburg Criterion in the High T Oxides". He concluded that a small value of the Fermi energy along with large value of T_C leads to a large value of the Ginzburg parameter. As a result he came to conclusion the critical behavior of high T_C oxides should be similar to superfluid helium. This theoretical prediction was confirmed experimentally; therefore, the difference between liquid helium and this new high T_C materials is not so large, as for conventional materials.

The Ginzburg parameter is proportional to the ratio of the energy gap over the Fermi energy. This ratio shows which portion of the electrons forms Cooper pairs. For the conventional superconductors this ratio is very small. It means that only a small portion of electrons is involved in pairing. In the high T_C materials this parameter is much larger. We're dealing with entirely new physics when a lot of carriers form the paired states.It means a small value of the coherence length.

As a result, it is difficult to perform the tunneling spectroscopy, which is a crucial experimental technique. At the same time, a small value of the coherence length leads to a number exotic propertiues. Among them the so-called plateau effect, zero-bias anomalies, residual microwave losses, depression by Pr-substitution, and a linear term in heat capacity.

All these phenomena can be explained from some unified point of view. Short coherence length in the presence of different structural units (planes and chains) leads to a two-gap superconductivity; it has never been observed in conventional materials. The larger energy gap corresponds to planes, and the smaller one to the chains.

As a result, the surface resistance in the low temperature region is a sum of two exponents. If temperature is low enough, then R_s is dominated by the smaller gap.

We should distinguish between intrinsic and an induced superconducting states. The S-N sandwich displays the proximity effect. In the transition metal we are dealing with two bands. A remarkable property of the high temperature oxides, for example, Y-Ba-Cu-O ,that they are characterized by both channels.

One can derive the expression for the critical temperature, and it allows one to explain the "plateau" effect.

If we remove oxygen, it affects mainly the chain sites. As a result, we developed local magnetic moments on the chains. It is well-known that the magnetic moment acts as a pair breaker. As a result, one can develop so-called gapless superconductivity on chains only. It makes a great impact on the spectroscopy, but T_C is not affected. This effect leads to residual losses.

In the end I'd like only to make up a couple of remarks. First of all, I would like to thank Alan Clark for organizing this remarkable symposium and for inviting me to be here. I'd like only to ask him to organize another symposium in 25 years from now. Please, invite me again, and I'll finish everything I wasn't able to say today.

Secondly, life is very tricky. I represent here the Office of Naval Research, the oldest department which is in charge of development of a basic science. Many years ago I got an invitation from Bill Little to participate in the Hawaii symposium. This symposium was sponsored by the Office of Naval Research. I wasn't able to make the trip. But Vitaly Ginzburg did, so his affiliation with the Office of Naval Research started a long time ago. That's why I'd like to ask a good friend of mine, Stuart Wolf from Naval Research Laboratory to say several words.

DISCUSSION

DR. WOLF: I actually feel very honored to be on the stage here, because I personally don't know Vitaly Ginzburg very well, but Vladimir asked me to say a few words, so I will do that.

I just want to make an announcement. There will be another talk by Professor Ginzburg at the Naval Research Laboratory next Thursday. It's going to be at 3 o'clock in the afternoon, and I'd like to invite everyone. We have a very big room, so I'd like to invite everybody here to come and spend that afternoon with us. The title of the talk is "Lev Landau, the physicist and the man". It's going to be a very interesting topic. I hope you all could come.

It turns out that Professor Ginzburg recently celebrated his 75th birthday. So this is really his jubilee year, so I have a little present for him. It's the uniform he should wear when he comes to the Naval Research Laboratory. He's sure to be able to get into the gate. It was quite difficult to make the arrangements, but we have done it. He will be actually spending a couple of days with us next Thursday and Friday.

I'd like to read a little poem. I've written a very short one, so you don't have to bear with this too long. It's called The Ginzburg Diamond Jubilee.

The Ginzburg Diamond Jubilee

As we certainly join in welcoming you again to our nation,
And are extremely glad to join you in this celebration.
Your contributions to science are so broad and diverse,
The chronicling of them requires no less than pentameter verse.
Your work spans the gamut from the stars in the sky,
The secrets have yielded to your mind's eye,
To low temperature physics, superconductivity to be sure,
Where you've stimulated the search for high T_C more.
This short line is meant to be just a little gift,
For the 75 years you've given the world such a scientific lift.

Thank you. That's all I have to say.

PARTICIPANT: You say your model can explain why Pr suppresses T_C. Can you say that in just a few words?

DR. KRESIN: Pr removes carriers from the plane. As a result, the critical temperature is going down. It means that, indeed, the chains are intrinsically normal. If chains are intrinsically superconducting, you can still measure some T_C. It is an experimental support that the chains, indeed, have induced superconductivity.

Dr.CLARK: What other experimental manifestations might we expect from a very high fraction of pairs?

DR. KRESIN: For example, an increase in a positron lifetime below T_C is due to the large value of the Ginzburg parameter.

DR. LITTLE: Vladimir, I didn't quite understand the picture of the Fermi energy being this little puddle, because if you look in the band structure of these materials, your little puddle is like a lagoon which is connected to a great big ocean. There are many other bands, so it's not a one band model. Isn't it really connected to these other bands?

DR. KRESIN: First of all, if we take the La-Sr-Cu-O, it has several similar energy bands. For this material we can measure experimentally the Fermi energy, which appears to be small. For Y-Ba-Cu-O it is relatively larger (about 0.3 eV), but still much smaller than for conventional materials.

DR. LITTLE: The problem is that you discount certain electrons. You are ignoring the fact that there are many bands that overlap one another. The last band is the one that you are focusing on.

DR. KRESIN: For La-Sr-Cu-O the energy bands are similar. Of course, we don't consider the filled energy bands. We count on the bottom of the non-filled band (number of states inside of the Fermi surface). The corresponding quantity is small.

GAMMA RAY ASTROPHYSICS AND EARLY RESULTS OF THE COMPTON OBSERVATORY

J. D. Kurfess

It's a great honor for me to participate in this symposium today. Unlike most of the other speakers, I never had the pleasure of meeting Prof. Ginzburg before today, so this is a double pleasure for me.

Like so many people in the field of high-energy astrophysics, my introduction to Professor Ginzburg came through the seminal book that he wrote with Syrovatskii, "The Origin of Comic Rays". That book was written in the early 1960's when gamma-ray astronomy was in its infancy. At that time cosmic ray physics had a history of 50 years or more, and the flux of cosmic rays at the top of the atmosphere was well measured. Much was known about cosmic rays -- the general elemental abundance distribution, the energy distribution, etc. They pointed out in their book, however, that one could also predict, based on the cosmic ray flux observed at the earth, and propagating those cosmic rays through the interstellar medium, that cosmic rays would undergo nuclear reactions which produce pions, and which then decay to high-energy gamma rays. So an observable flux of high-energy gamma rays, with typical energies of 100 MeV gamma rays was predicted.

The field of gamma ray astronomy was almost assured, although not easy because of the very low fluxes of photons available. Indeed, gamma-ray astronomy is a vibrant endeavor and we've made a lot of progress in the last 30 years. In my presentation, I will briefly discuss the development of gamma ray astronomy, but I'll really focus on NASA's Compton Gamma Ray Observatory (GRO), which was launched about one year ago. I will highlight two of the very exciting topics that have been investigated by experiments on GRO: gamma rays from active galactic nuclei and the cosmic gamma-ray burst phenomenon.

A lot has transpired over the 25-30 year history of gamma-ray astronomy. As an example, consider the progress we have made with solar flares. The first detection of gamma rays from solar flares was made on a balloon flight in the year 1959. Solar flares are a source of cosmic rays as well as gamma rays. The gamma rays result from charged particles being accelerated in the flare region, presumably interacting at the footprint of these magnetic arches and undergoing nuclear reactions. Gamma-ray astronomy provides an opportunity then to use nuclear spectroscopy as a technique to study some of the detailed processes going on in solar flares.

The richest solar flare spectrum was observed by the Solar Maximum Mission satellite from a flare on 27th of April, 1981. The inferred gamma ray spectrum obtained from the flare included a very rich nuclear spectrum from which about 15 nuclear line features have been identified. Such high quality spectra permits the study of the accelerated particle spectra, abundance determinations, and a wide range of physical processes in the flare region. Now we want to extend these capabilities to astrophysical sources.

In the early 1970s, the first observations of cosmic gamma ray bursts were reported. These events were discovered by small detectors in space which observed very large, but brief increases in the counting rate every few days. The events typically last from a fraction of a second to tens of seconds, and are characterized by a continuum energy spectrum with a several hundred keV e-folding energy. Recently, however, some bursts have been detected up to tens of MeV. In addition to the continuum spectrum, some bursts exhibited apparent absorption features at tens of keV, and there are also reports of red-shifted positron annihilation line radiation. These latter observations give support to the idea that bursts originate on neutron stars.

The explanation for these absorption features is that we're dealing with X-ray emission from a neutron star with a very strong magnetic field, typically 10^{12} gauss. The precession of an electron -- in this case, the energy to flip the spin from aligned with the field to the opposite direction is roughly 12 kev in a field of about 10^{12} gauss. An absorption or emission feature at these energies is taken as evidence that we're seeing emission coming from a neutron star with a magnetic field of several times 10^{12} gauss, typical of what one would expect for neutron stars. However, as I shall discuss later, the origin of gamma ray bursts is still unknown.

Two satellite experiments were launched in the 1970s to study very high energy gamma rays above 50 MeV. The first was the U.S. satellite SAS-2 which undertook the first high-energy survey of the Galaxy. This was followed by COS B, a satellite launched in 1975 by a European consortium. COS B also concentrated on mapping the galactic plane during its 6+ year mission, but also detected one extragalactic source, the nearby quasar 3C273. The galactic emission is characterized by a ridge of high-energy gamma ray emission along the galactic plane and concentrated toward the galactic center region. This is largely due to the interaction of cosmic rays in the interstellar medium producing pions which decay to high-energy gamma rays. Several discrete sources were also detected including the well-known Crab and Vela pulsars, the first two radio pulsars that were found to be sources of gamma radiation. These are neutron stars with rotation periods of 33 milliseconds and 88 milliseconds respectively. The third strong source, 'GEMINGA', has been an enigma. Although it is one of the strongest gamma-ray sources in the heavens, it was not detected at any other wavelengths, and remained unique in that respect for almost 15 years.

However, only yesterday, it was announced that, based on ROSAT X-ray satellite data and some GRO data, GEMINGA has also been determined to be a pulsar, with a period of about 237 milliseconds. So a long standing puzzle has been, at least partially, resolved.

Now I will discuss briefly my supernova contribution to the symposium. A major objective of gamma ray astronomy has been the confirmation of several aspects of nucleosynthesis in supernovae. We believe that most heavy elements are synthesized in supernovae events and that several radioactive isotopes are produced. Some of these emit gamma ray lines that should be observable. This was remarkably and convincingly proven with the observation by the SMM mission of cobalt-56 decay radiation from the supernova 1987A.

SN1987A is the nearest supernova in several hundred years, and is located in the neighboring galaxy, the Large Magellanic Cloud. This observation of cobalt-56 confirmed what we were quite sure had to be the case, that the exponential decay of supernova light curves is powered by the energy from the radioactive decay of cobalt-56, for during much of the decay phase the decline of the luminosity agrees quite well with the half life of cobalt 56.

One other very important research area has been the observation of positron annihilation radiation from the Galaxy primarily towards the central region of our Galaxy. This will be the focus of Reuven Ramaty's talk, so I won't dwell on that, but will move right along to the Gamma Ray Observatory mission.

GRO is a NASA mission that we started working on almost 14 years ago. Finally, we arrived in April, 1991 when GRO was launched by the space shuttle Atlantis. Two days later, the 35,000 lb. satellite was deployed by the Atlantis crew. Things were going along pretty swimmingly until the operation to deploy the TDRS high-gain antenna, at which point the antenna boom was stuck. The U.S. had not had an astronaut out on an EVA for over five years, although an EVA was planned for the following day. So astronauts Jerry Ross and Jay Apt went out and gave the boom a shove. That was all it took and the boom was released, GRO was deployed, and things have been going quite well since then.

There are four experiments on the Gamma Ray Observatory. My team at the Naval Research Lab is responsible for the Oriented Scintillation Spectrometer Experiment, which covers the nuclear line astrophysics region from about 50 keV to 10 MeV. An instrument called the COMPTON telescope operates in the 1 to 30 MeV energy range. It uses the Compton scattering process to produce images of the gamma ray sky with a fairly broad field of view. I should point out here that all of the GRO experiments use the Compton scattering process as part of the detection process for gamma rays. Therefore, several months after launch the observatory was named the COMPTON observatory after Arthur Holly Compton, who was the discoverer of the gamma ray scattering effect named after him and was one of the early U.S. Nobel laureates.

The highest energy experiment on GRO is EGRET, which uses a spark chamber and calorimeter to detect high-energy gamma rays in the range from 20 MeV to about 30 GeV. Let me point out here that the COMPTEL and EGRET experiments both have very broad fields of view. During the first 15 months of the mission a complete survey of the sky will be obtained using those two experiments.

The fourth experiment is called the Burst and Transient Source Experiment (BATSE). It focuses on observations of transient phenomena, particularly the cosmic gamma ray bursts. The BATSE experiment is composed of eight modules located on the corners of the spacecraft. This provides full coverage of the unocculted sky for some of the BATSE detectors. By comparing rates in different detectors, they're able to locate gamma ray burst sources to a few degrees on the sky.

The very first observation that was undertaken by GRO was the Crab nebula because it's almost a standard candle for gamma ray astronomy. This is the 33 millisecond pulsar that had been studied by SAS-2 and Cos-B. What was really demonstrated is the synergy of these four GRO experiments. We've now been able to measure the gamma-ray spectrum coming from this 33 millisecond pulsar over five to six decades of energy. It is fit extremely well by a single power law. Now we can look for the weak bumps and wiggles in the spectrum, but it is remarkable that a single spectrum fits that data quite well. The excellent spectrum should give the theoreticians much to ponder.

Now I move on to the two topics that I want to highlight, the active galactic nuclei and the gamma ray bursts. But in the process of doing that, I'll mention one other supernova. Just a few days after the launch of GRO, there was a supernova discovered in the constellation Virgo, SN1991T. It turns out that SN1991T is the closest Type 1 supernova that has occurred in 20 years. So a target of opportunity was declared to observe this supernovae. The supernova is only one degree away from the nearby quasar 3C273. As I mentioned, COS B had seen only one source beyond our Galaxy, and that was the nearest quasar, 3C273, at a distance of 900 million light years.

This target of opportunity provided the GRO experiments, particularly EGRET and COMPTEL, an early opportunity to observe the region of the sky that included 3C273. The EGRET experiment expected to see 3C273 right away, since it was known to be a strong gamma-ray source. But no 3C273. Instead, they were surprised to find there was another bright source in the field of view, and they were able to identify this as an even more distant quasar 3C279, at a distance of about a three billion light years!

What was fortuitous about the target of opportunity was the variability observed in the high energy gamma ray emission from this quasar during this two-week observation period by EGRET. The intensity started out at a modest level, grew almost linearly for about a week, and then abruptly returned to a quiescent state in another 2 days.

This might remind you of some of the time variability that Ken Kellerman discussed in the radio data. But the gamma-ray variability occurred on time scales of a day or two, which requires that the source of this emission have a light travel time of only a couple of days. Presumably it's very near the core of 3C279, which is believed to contain a central black hole. We think we are seeing gamma-ray emission originating from the footprints of the jet.

One thing that we learned is that the apparent luminosity of the source is dominated by the gamma rays. Now, this may be somewhat misleading, because, as Ken Kellerman was pointing out earlier, we think that we're dealing with is a beamed source. 3C279 is a known superluminal quasar, so this is a source where the jet is pointed toward us. The apparent gamma ray luminosity would require a total luminosity from 3C279 on the order of 10^{48} ergs per second, just an incredible amount of energy (about a million, billion times the luminosity of the Sun). This can probably be reduced by one or two orders of magnitude by invoking a beaming mechanism, but still, it's an incredible amount of energy that is being released. With the very rapid time variability, it can't be associated with anything very far away from the black hole.

Now that EGRET has completed two-thirds of their sky survey, they have detected 14 new high-energy gamma ray sources, extra galactic in origin, that are associated either with quasars, or in three cases with BL Lac sources. BL Lac sources are a similar type of an object where we think that the jet is aligned directly toward us, even more than in the case of quasars.

So gamma ray astronomy at this point is contributing a lot to the study of these active galactic nuclei. A couple of things I'd like to point out here. The red shift is the measure of the distance to some of these objects -- it turns out that 3C273 is actually one of the nearer ones. Several of the QSOs appear to be coming from sources at a red-shift of about 2, putting them at tremendous distances of 8,000, 10,000 megaparsecs, near the edge of the observable universe.

The detection of 3C279 was the first remarkable discovery that came out of GRO. This was followed by some very exciting results on gamma-ray bursts. Gamma-ray bursts are very brief but very intense flashes of gamma rays observed by space instruments. They exhibit remarkable variability in terms of the temporal structure and there's a lot to be learned from the time history of these bursts. The bursts can have very simple or complex time structure and they last from just a few milliseconds to several hundred seconds. For example, some bursts are followed by an extended period of time (tens of seconds) of no emission, only to be followed by another burst which we find is coming from the same source.

The BATSE instrument observes an average of about one gamma ray burst per day, so they have observed about 300 gamma ray bursts at this point in the mission. BASTE has detected a burst which exhibits the shortest duration that has been observed, with a minimum time structure of only 0.2 milliseconds. We're clearly dealing with a very compact object. Many bursts exhibit very complex time structure. Other bursts show very smooth structure. So whatever the origin of these gamma ray bursts are, one has to account for this tremendous variability. Something else about these bursts that I think is kind of mind boggling. One would think we're dealing with a single type of a mechanism and there couldn't be tremendous latitude in what we're seeing. But the distribution of the number of bursts versus duration shows that burst durations extend all the way from ten milliseconds up to well over 100 seconds. So there are over four orders of magnitude in duration that one has to account for in attempting to model the mechanism that is producing these bursts.

Before GRO was launched we thought we understood the origin of gamma-ray bursts. The logic there came from a couple of different arguments. One had to do with the number-intensity distribution of gamma ray bursts. Consider plotting the integral number above a given intensity as a function of the intensity of the bursts. For an isotropic, homogenous distribution, the number of events that you would expect in a spherical shell at a distance R goes as R^2. That is reflected in a number-intensity distribution that should vary as of the intensity ,S, as $S^{-1.5}$. That is what is seen for the more intense bursts. But there is a break in the distribution at some intensity which corresponds to reaching the edge of the homogenous distribution. This had been observed before, but in the previous data one could not convincingly exclude the fact that this may have been a detection threshold effect. The BATSE experiment is ten times more sensitive than previous experiments. Therefore, the expectation was that the weak bursts observed by BATSE would show a break in the log N-log S distribution corresponding to seeing the disk of the Galaxy. The idea is that with the limited sensitivity of previous experiments, they were seeing bursts out to a distance somewhat smaller than the several hundred parsec thickness of the galactic plane, i.e. we were seeing the isotropic distribution of nearby burst sources in the local spiral arm of our Galaxy. As we got more sensitive instrumentation, that is, the BATSE experiment, we would see out to greater distances and start to see the disk population show up, much like the Milky Way is seen in the night sky. For these weak bursts, the slope of the log N-log S distribution should become minus one. But at the same time we should also see the galactic plane reflected in the spatial distribution of the bursts.

However that is not what is observed. In the most recent distribution obtained by the BATSE experiment the log N-log S distribution shows quite a departure from homogeneity. But the bursts remain isotropic on the sky, even for the very weakest bursts. There is no hint of a concentration along the galactic plane. The BATSE team has tried many statistical tests, and everything is consistent with an isotropic distribution on the sky.

So where are these bursts coming from? One possibility is that they are still neutron stars, not from the disk but from an unknown galactic halo population. It could be the same population that might account for the dark matter in the galaxy. BATSE can place a lower limit on the size of this halo of 40 or 50 kiloparsecs. This confounds the theorists who must explain how this population of neutron stars could form such a large halo.

Another possibility is that they are quite local, fairly close to our Sun. If they're within 100 astronomical units, a typical burst luminosity is 10^{26} ergs. But what could they be? It has been suggested that they could result form collisions of comets. There are many comets in the Oort cloud, a collection of comets at distances of 100 to 1,000 AU. But how do you extract gamma rays from such collisions, and how do you extract gamma rays with extremely high efficiency, for gamma ray bursts are not seen at any other wavelengths.

If they are from a galactic halo, that extends the energy requirements up by many orders of magnitude, so now we would require 10^{42} ergs per event. Since typical durations are from a fraction of a second up to several tens of seconds, you realize that the power released is mind boggling.

As the new BATSE data come in, people are being forced to take cosmological models much more seriously. If one places the burst sources at distances of a thousand megaparsecs, the energy required is on the order of 10^{51} ergs. What does that energy correspond to? Well, the models people are applying here have to do with neutron star-neutron star collisions, neutron star-black hole collisions, or perhaps the collapse of neutron stars to black holes. Again, you must take a substantial fraction of the energy available and convert it into gamma rays, with very high efficiency and very little radiation at other wavelengths.

I hope that I have conveyed the excitement of the GRO instrumentors and some of the puzzles arising from the initial results coming from the COMPTON Observatory. I expect we'll find a lot more. I expect that some of future results from GRO will relate directly some of the issues which Prof. Ginzburg has conributed to for so many years; i.e. the Origin of Cosmic Rays.

Thank you.

PARTICIPANT: Are pulsars gamma ray sources? e.g. Geminga.

DR. KURFESS: The story on Geminga is that from the early days of high-energy gamma ray astronomy, from the SAS 2 and the COS B observations, Geminga was known to be one of the three brightest sources in the sky. The COS B map provided a location for Geminga to about one degree accuracy. Those fields were searched, and there was a candidate X-ray source that was identified about ten years ago. Recently, Jules Halpern from Columbia Univ. and Steve Holt, using a ROSAT observation, found periodic emission from the X-ray source that had the suggested association with Geminga. They found periodicity at 237 milliseconds. Shortly after they had made that discovery, the EGRET team on GRO looked at their data and were able to confirm that the 237-millisecond pulsar was also the gamma ray source GEMINGA. They have also been able to measure a spindown rate for the pulsar from which they estimate the age of the pulsar to be about 100,000 years, perhaps a bit more.
This suggests that it's a relatively nearby pulsar, closer than either the Crab or Vela

pulsars.

PARTICIPANT: Are all galaxies gamma ray active?

DR. KURFESS: The standard model, called the AGN paradigm, is that all active galaxies have massive black holes at their cores. The list of EGRET AGN's now includes 14 in a flux limited sample. We're seeing this many out of a total list of 8,000 or 10,000 optical AGNs, even though EGRET has detected some of these sources to distances of z=2. It looks like EGRET will be able to observe 20 or 25 of these, maybe a few more. The ones we're seeing are the ones where the jet is directed toward us.

It does imply that there are a lot of active galaxies out there with jets beamed in different directions that would contribute to an overall gamma-ray background in the universe. Now, at lower energies, in the MeV region, it is probable that the emission is not so tightly beamed or not beamed at all. There is a real problem with a universal cosmic gamma ray background and an enhancement in the MeV region that really isn't understood.

PARTICIPANT: If the X-ray bursts are spread out in a halo, do they arrive later than the visible sources which are found in the plane? What's the difference in the mechanism of their distribution?

DR. KURFESS: Well, I think there are a couple of different thoughts on that. One possibility is that for some reason, there's a population of old stars that were in an original halo, and a lot of these have gone through their evolutionary cycle and are now neutron stars, and we're seeing them out there. Another possibility is that they are neutron stars from binary systems that were expelled into the halo when their companion star evolved though a supernova phase. This is known to happen at some level, but whether you can get them out into a halo that's 50 kilo parsecs in diameter is another question.

SOME PUZZLES OF THE NORMAL STATE OF HIGH T_C OXIDES

L. P. Gor'kov

First of all, I am very glad to say my thanks that I am here, because I would be unhappy to miss this Ginzburg symposium. So, thanks to the organizers for inviting me. Usually when I'm asked who was my teacher, I answer it is Landau. Really, Landau was my main teacher, but everybody counts the number of people whom he considers also as a teacher, and among them, Vitaly Ginzburg is the first. It happened in a particular way.

I developed, once, many years ago with Ginzburg-Landau equations from microscopic theory. I was sure the results were correct because of the beauty of the microscopic theory and the beauty of the Ginzburg-Landau scheme. When Vitaly started to push on me just to do a comparison with experiment, I was quite surprised. Of course, I was snobbish and of course, I simply didn't know how to start it. He was pressing on me for a while, and when he saw that I'm not going to do anything, he just did it by himself and published it. This was a hard lesson. Since then I do my comparison with experiment by my own, but this was a good example, when I was able to understand how you should do things like that.

Now, I would like to say a few words about the new materials, in five years, that we know about in high-temperature superconductivity. I don't know if Ginzburg really believed that it's possible to have high T_c. I guess he did. But I must say, I didn't. There were no reasons why such a superconductivity should exist -- well, no obvious objections, but the nature was telling us, you have so many compounds already, they are in the same scale, so probably to increase the temperature you have to find some sophisticated mechanism. My approach was completely different. I was trying to understand what sort of phenomena people do have in the number of materials where TC is extremely low.

But I guess it was extremely important, and Vitaly and Bill Little were pushing all the time experimentally, justifying how important it is, that this is really one of the key problems. This encouraged and stimulated experiment, and I think I have to say that I wasn't right. But I think nevertheless for both of us, it was completely unexpected that such high temperatures can be obtained so soon.

So when such materials appeared, of course the immediate problem was, everybody was interested to understand it. There was, anytime when you get exciting results, a number of proposals how to get this superconductivity mechanism. But the most natural way to get a survey of new superconductivity was just to use the technological Ginzburg-Landau scheme and see what happened. To a large extent, it was a discouraging search, because there was nothing, no pronounced features.

The main features of the BCS theory still remain. You remember in the microscopic theory of superconductivity, the key part is just the creation of Cooper pairs. The typical size was such a big size, and the number of electrons per volume of such a pair is extremely high, somewhere between ten to the nine, ten to the 12. Since temperatures were much higher here and Fermi velocity is also less then it was in older superconductors, the main question was just to check whether we have Cooper pair type interaction.

The key experiments for this are with measurements of flux quantization, when the superconductor is able to screen and fix a flux quantum, which is measured in this quantity, and with signals about participation of two electrons in this pairing. Our point was about some effect. When you apply voltage across a barrier and therefore there is difference in chemical potential, so in principle pair from chemical potential here could jump there, but in this case it would be a quantum of electromagnetic radiation, which is just two ev. This was also absurd.

So as I said, there was nothing especially new. The main features of this theory remained. Cooper pairs are definitely here. So based on the study of superconductivity itself, it was difficult to make a judgment. There were differences. Most of the new superconductors are based on so-called conducting copper planes that are highly isotropic. We can discuss how big are Cooper pairs, because they are isotopic, they are pancake shaped, they are short in perpendicular layer dimensions. But they are still large enough, so the number of electrons per unit Cooper pair is still big enough. Of course, it's not such a huge number as it was in older superconductors. This is the famous Ginzburg number which Vladimir Kresin was talking about.

This number measures how close to transition temperature can you interpret superconducting properties in terms of -- Ginzburg-Landau theory. People agree or disagree. According to my estimate, it is probably ten to the minus two, ten to the minus three. You saw previous estimates of ten to the minus one earlier, but it depends on the test.

Important is that at least quite reasonably, a region near T_c can be understood in terms of Ginzburg-Landau approach, which is supported by our understanding of what is going on in BCS theory. I am not going to give quantitative description of interaction, because in my opinion there is a lot of strange particles which exist in new materials, and my intention is just to give you a smell of this puzzle. I am talking mostly about this system of materials, and what is said here. So if nothing is special in superconducting properties, we should probably look for key properties in normal state.

This was easy to do in older superconductors, because when you had even TC on a scale of ten degrees Kelvin, this is still reasonable to suppress superconducting state by applying magnetic field. So applying magnetic field, you could restore normal metal behavior. Here it is impossible. It will be possible, but still these are huge fields, at least 100 tesla.

So we have to work with properties above TC, and try to find out if there is something unusual there. It took a rather long time, but actually there is a collection of surprises which I just try to summarize right now. If you remember, at the early discovery of high TC, it was discovered as a result of doping just with oxide compound is not conducting. Instead, it is a magnetic insulator with high temperature, with very high activation gap on the scale of one, two eV. So superconductivity here appears as a result of doping, not a huge doping, one sixth, but still it gave about 40K.

Next was the discovery of these chain compounds. I mention right now just oxide compound with oxygen seven. Everybody of course has forgotten about these old-fashioned materials and started to investigate these extremely complicated structures. Here we have TC on the scale 91K. Right now, it is possible to have transition temperature with bismuth based materials around 120, 125.

What was strange here? People started to check what is going on when you substitute with yttrium. The difference is in this case with elements. They have magnetic moment. The magnetic moment is always destructive feature for superconductivity. Their presence it is just decreased drastically. First this was a great surprise to me, at least, but substitution of yttrium didn't change T_c at all. This is really very strange, because this rough structure shows with one copper or two plane, there is a neighboring plane, and between there is an yttrium atom, and it is really unclear how is it possible to localize electrons just in the plane, because all distances are on the same scale in size. So it is extremely interesting and remains unclear how nature allows you to have electrons localized just in planes and not interact with atoms.

Anyway, this was the first result. When we understood that these are probably somehow very two-dimensional materials. Now we know copper-oxygen planes are a common feature in all these compounds. This means that we have to take into account that it always is anisotropic, and this problem of the dimensionality becomes important because superconductivity in a sense is a three-dimensional phenomenon. You can apply it only if you are able to have a current in equal directions.

Important point is actually superconductivity. Probably everyone has heard that in two dimensions, fluctuations are strong, and fluctuation can destroy phase transition. Superconductivity belongs to the class of transitions which can exist like in the case of helium II. It can exist in two dimensions, just the phase transition. When you go down in temperature, when it decreases temperature, it can only increase as a new or balanced state.

Now, on fluctuations, I was talking about the magnetic superconducting fluctuations. Are we strong in this case? We shouldn't be strong if we have the

Ginzburg number small enough. When people realized this, good samples became available and people now started to investigate in more detail some superconducting properties. The final proof as most of us believe for BCS superconductivity was a discovery of the so-called Hablis-Victor peak. Hablis-Victor peak is when you measure the relaxation rate for nuclear spin, how it depolarizes due to interaction with surrounding electrons. Important is that this density of state feature in BCS, with the singularity of energy equal to delta. It appears immediately near T_c, and you see this singularity in an increase of the relaxation rate.

So what is known when this peak is absent in new materials? Is it a new mechanism? We don't know, it's possible, but there could be alternative explanation which would involve some strong coupling explanation. This result could be an indication in favor of d wave pairing, for instance, as now many people are trying to prove by different means.

Now, I would like to come back to the normal state properties. This is an attempt to show that if you just take nuclear spin and apply magnetic field, it is polarized by magnetic field. But there are forces which can change polarization of spin. This is mostly due to interaction with electrons in the usual process. You have an interaction with this nuclear spin, but some of electrons in metal can pick up this spin due to Fermi interaction, so as a result you get depolarized spin. It is clear that the probability for such a process is proportional to number of such electrons which are above Fermi surface. This is temperature and this is normal metals, this is always a constant, with a so-called interaction proportional to temperature.

However, when people started to get able to measure the relaxation rate above T_c, this is a schematic for T_1 relaxation rate over time pressure. It should be constant, as I said, and it is constant for oxygen if measured for oxygen or for yttrium. But it is not constant for copper. The magnitude differs very seriously, and temperature dependence is very unusual for metals. So this is a good time to remember that actually most of these compounds somehow have magnetic properties. This is a magnetic compound, this is antiferro-magnet with temperature on a scale of 500 K.

Suppose we have local moments somehow in this system. When we can understand the difference between behavior of oxygen isotopes and copper isotope, if there are local moments, then in this case copper would experience this magnetic ordering, if there is a magnetic moment. So local fields here on copper site are large, but this effect would cancel because it changes size, on oxygen site and the same on yttrium site. So assuming these would be an explanation, but it immediately involves a picture that somehow magnetic moments are present. But we know that this oxygen seven is a metal, it is not antiferro magnetic, it's actually a superconductor.

So we have a problem which was emphasized from the very beginning with magnetic properties, which is just the same as strong interaction. It was stressed from the very beginning, and people use it in different explanations. Anderson was pushing forward, Bob Schriefer based his spin-bag picture for superconductors on this idea. But we still do not have a final solution for all this. What we probably know from the experiments is that magnetic fluctuations are somehow important. This group just suggested an explanation which I already explained. But I have to mention one important point.

It seems that for magnetic phenomena, two dimensionality is very important. However, superconducting transition can happen as a phase transition in 2D, because disorder at such a transition means just phase for superconducting order parameter. This Belizinsky-Koster theorem proposes that there is a transition in two dimensions for superconductivity. But for antiferro magnetic transition, standard magnetization can exist only at zero temperature. If you increase the temperature with spin waves, immediately destroy the long-range magnetic order in two dimensions.

So we have reasonably small fluctuations for superconducting transition, but antiferro magnetic transition should be important for these materials if moments are present. In these oxide compounds, we really see how this two-dimensional fluctuation would develop.

Just to conclude with the different role of two-dimensionality for superconducting transition because one can survive in two-dimensional case and the other cannot.

Now, I was thinking mostly in terms of oxide compounds. Now we can come back to the idea of doping. The first thing people started to do with one-two-three materials was to investigate the phase diagram here, this temperature as a function of oxygen concentration. Here is oxygen six, here is oxygen seven, and experimentally it is known superconducting transition decreases, which you see by neutrons. Superconductivity appears starting from oxygen seven, goes down, it is around 90 here, it is between 50 and 60 here.

But is going here, nobody knows, because there is no actual control -- not good enough control on the oxygen. In addition, I would like to mention an oversimplified picture. I said in two dimensions you can have fluctuation. Ground state you can have only at zero temperature. But if you have layers which are coupled together, when in this case you can get a dimensional type transition, and this is what is shown here. Nobody said us that there is no play which spins somehow on plane here, so there could be also spins, because just the onset of superconducting transition shown here is a three-dimensional phenomenon showing that in planes you already have very developed magnetic fluctuation. So even if you look from this side of oxygen and trying to think what is going on in this direction for higher oxygen, you are somehow forced to think what happens with spins when we proceed along concentration ranges.

Now the next diagram, this is for doped materials where this superconductivity has been first discovered. Here we have a very similar picture. The difference is this magnetic state which as I said is a real strong insulating state with energy gap. This is a very steep decrease of three-dimensional ordering, but spins are here. You can see in this compound, samples are available of big size. You can see large fluctuation of local moments here. Showing some localization process, people usually speak about spin glass or not, because concentrations are still rather small. What happens next? The increased concentration already at .05 you start to see superconductivity, superconductivity increases with gradual increase of X. When there is so-called optimal concentration where superconductivity is 40K, now it is suppressed and here we have metallic state, but there is no superconductivity. So when you started with one-two-three materials, you could think from the metallic side, you can think about Fermi surface, you can think about them as metals, and just to look for a mechanism which could explain the high transition temperature, but you remain with the language which uses metallic type description.

But here, something unusual happens, because as I said here, this insulator is just repeated once again. In one-two-three there is no problem in a sense, because copper two shifts and no explicit difficulties with the ordinary description of metals. But in this material, there is an explicit problem. If there was no interaction between electrons, you would get a good metal with half filled band. Instead, as I said, it is the insulating state with very pronounced localization of magnetic moment. When you add a minor amount of doping with strontium ions, you very quickly get conduction and metallization and superconductivity.

So how do we think about doping? Usually there is some deeply localized levels, and they are responsible for magnetic moments in this one. When you have some conduction bands which are empty, but when you dope it by strontium, a number appears here. So when you have reasonably good homogeneity of your samples, this system is to show metallic behavior, and actually this is a picture borrowed from superconducting description.

This idea of doping works very well as measured by Hall effect. Hall effect as you know measures just numbers of carriers, number of holes proportional to concentration, when this whole coefficient is proportional to one over X. By one over X, by this blue line is shown one over X behavior. What is seen is, at the beginning you have with one over X behavior approximately at this range, even below .1, activation starts, and it becomes obvious that this simple explanation based on doping stops working, and probably with concentration, when people already have the beginning of metallic properties.

This is the question what sort of Fermi surfaces we have. In metal, we would have big Fermi surfaces, and if we just take this so-called optimal, two minus X just for X, .15, we would treat them as a Fermi surface which is rather big, and size number of holes here is to be proportional to one minus X. So one minus X for strontium, this means five or six. Our possibility is that you use this doping concept, you have small Fermi surface here, and we are conducting. So Fermi surfaces here will be proportional to X.

Now, you remember, everybody was surprised by the difference in behavior in one-two-three materials of relaxation rate. It behaved in a one way for oxygen 17, but it was different for copper. Now in the same experiment, measurements of relaxation rate have been repeated for this material, and it showed just the same behavior, practically the same except just minor difference in numbers, just drastic difference in magnitude between relaxation of copper an oxygen.

So if I take it literally, if I believe in the explanation I get for this phenomenon in one-two-three materials, I should accept that the Fermi surface already at this composition is rather big. But I can, instead of talking about strontium doped materials, I have another material which I can do rather easily. I can take two copper or four, and dissolve oxygen in it. It is just a solution. If I just start count numbers or holes which oxygen introduces into my compound, it's one to one correspondent. So I can consider this compound from two points of view. One is, I just consider it thermodynamically, it is just a solution of oxygen, so there should be a stability gap. On the other hand, I can consider it as doping forces and see is there any comparison between these materials and this one. Delta here usually on such a small scale, .03.

So here is a comparison again. What do I have for phase diagram for strontium doped materials, and this is for oxygen doped materials. Here is for superconducting state. Here is again metal like we have here, but superconductivity tempers here in response to this optimal transition temperature. In such a range of concentration, we have a phase separation. This phase separation means that oxygen moves in space and creates a new phase. Just taking delta here means nothing, because it should separate into two pieces, one phase here, a phase here as is shown here. A metallic phase and this one an insulating phase.

PARTICIPANT: Where is superconducting phase?

DR. GORKOV: Yes, this is a superconducting phase. But now it is interesting just to note what is this delta. Delta is .06 means that you have in this phase one oxygen atom. They are 15 unit cells approximately, maybe less, maybe more. It says that you have just this size, which is not interesting. And we had the same for yttrium based materials. Because there are so many fissures, very common in strontium doped and oxygen doped materials, there should be some mapping. The main difference in properties when you investigate the behavior of a system with number of holes is just due to the fact that oxygen is paired but strontium is not, so when you increase concentration of strontium, here you have thermodynamic transition. But when you increase concentration of strontium, you somehow are not in equilibrium state.

I guess my time is over, and I have to finish. Let me just mention that actually, there is a lot of puzzles like this. I wanted to show you once again that the problem of do we have metallic phase with phase transition is not finished. Recent neutron data probably can give a proof, do we have small pockets or do we have large Fermi surfaces.

Now it is once again a summary of what I said about doping. Doping is not clear. Since we do not understand how it takes place in two-one-four materials, I really am in trouble to understand how it works for these one-two-three materials, where you have chains and thallium based materials. I was trying to emphasize what 2D superconductivity is, when I said that for magnetic phenomenon, fluctuations are important because magnetic moments are there, but two-dimensional magnetic transition doesn't exist. There are many others like infrared, linear sensitivity and so on which are not resolved yet.

I wanted to finish saying the following, that I guess it will take probably a long time to resolve all of this. I strongly doubt if it's possible to establish a mechanism of superconductivity. The simplest way, how to simplify our analysis of superconducting purposes is still Ginzburg-Landau theory, and I think as somebody already mentioned, in the next 25 years we will probably know the answer to all of this. But we actually see transitions increase much sooner, which was so unexpected to have a transition temperature on a scale of 100K.

Thank you for your attention.

PARTICIPANT: I just wanted to point out that there has been some recent work looking at strontium copper oxide in the region above the 1.5 doping. There is a phase transition in samples that have been let come to equilibrium.

DR. GORKOV: Strontium, you say?

PARTICIPANT: Yes, and there's a transition that occurs at a doping of roughly .2 with a very sharp drop in T_c. The metallic phase is actually at very low T_c.

DR. GORKOV: Well, this would suit me, but if you are able to get an agreement between Carver and Jorgensen, this would be great. What is important, for oxygen you have a transition.

PARTICIPANT: You passed over one item. You say magnetic phenomena fluctuations are crucial for superconducting activity. Is that a statement of belief or of fact?

DR. GORKOV: That is a statement of belief, actually. I was trying to pursue that magnetic phenomena are seen all the time in all materials. They are seen experimentally and they should be seen as difference in relaxation between copper and oxygen.

So it is seen even in oxide compounds. When you go to one-four compounds, you really have magnetic insulator, and minor amounts of dopers may produce metallization. When you perceive that given concentration from higher temperature down to decreased temperature, you see band neutrons. So fluctuations are present there. This would be rather strange if there is no exchange interaction between conduction electrons and localized -- so actually, this was the question which was first formulated by Anderson. Can we split magnetic degrees of freedom from electronic degrees of freedom, and this is the issue which we are trying to resolve both experimentally and theoretically. I'm not trying to do it in the same way as he does, but it seems it's not an empty question.

GALACTIC ANNIHILATION RADIATION

R. Ramaty

I'd like to thank Dr. Clark for inviting me to honor Vitaly Ginzburg. Of course, I did not have the pleasure or opportunity to relate to stories back in Moscow, but it turns out that you did play quite an important role in my life, in an indirect way. I actually grew up on your book, as far as astrophysics is concerned. This is how I learned high-energy astrophysics, from your book with Serovatsky, The Origin of Cosmic Rays. This was mentioned by Jim Kurfess also.

In fact, my thesis was on modulation of cosmic rays, and what I did was to use ideas from this book to calculate interstellar cosmic ray positron flux, and then compared with local observations and tried to learn something about the modulation of cosmic rays. I won't get into this at all, but just to point out that that motivated my interest in positrons in astrophysics. In fact, my talk today is on positrons, but not from cosmic ray origin.

Another sort of information about you was the fact that Leonid Osivnoy works in our laboratory. I talked to him a few days ago, and he actually did confirm the Ginzburg seminar that we heard about, and also the fact that you consider yourself more a physicist than astrophysicist, very modestly. Finally, he told me something that wasn't mentioned today, namely, the famous Ginzburg equation is good for a physicist, but that for an astrophysicist one is equal to ten. So with this introduction, what I would like to talk about is galactic annihilation radiation, in other words, positrons annihilating in the galaxy and in sources. It really has little to do with cosmic rays, but it's a story that I've been following for quite a number of years. Recently, there is also some new data, so the story is actually quite interesting.

I will narrow down to one topic, namely, galactic annihilation radiation. Just to tie into the previous talk, we can look at data from the COMPTON observatory that Jim Kurfess discussed, with OSSE spectrometer, which is a sodium iodide spectrometer. It's not an ideal spectrometer because of the energy resolution, but it has other good things. A measurement of the galactic center shows the half MEV line. There is a continuum due to processes which we don't fully understand. This continuum may have something to do with cosmic rays. But then on top of this continuum, there is a second continuum due to the annihilation of positrons, namely, positrons form positronium and then orthopositronium decays into three photons and instead of producing a line produces this extra continuum.

So this is just to show you what we are dealing with. I realize most of the audience is really in superconductivity, so I have to make sure that you understand what I am talking about. This is basically what I'm talking about. I'm going to discuss much more data, but basically, this shows a rather typical and also recent observation.

Now, the search for galactic annihilation radiation has been going on for a long time. There is a lot of work dating back to the early 1970s. These are fluxes in the half MEV line, measured at various times. Most of these observations were done from balloons. A few of them were done from satellites. I will describe some of these as we go along. These very early observations were done with primitive detectors at Rice University by Hames. There were lots of problems. The line did not come out at exactly 511 kilovolts, and to this day we are not sure exactly what is going on with these data points.

Many of these observations were done subsequently with germanium detectors. For example, this is the HEO 3 satellite which was launched in 1979. There were four germanium detectors on board, and these were the observations. There are other observations here also. Here is the OSSE observation I just showed you a moment ago, and this observation is using a balloon from Australia. It is only about two or three weeks old. So we have this entire database, and as you can see, there is tremendous confusion; what is going on?

First of all, you can see that the fluxes are widely different. For example, measurements done only 11 days apart, the flux is quite different. So there is first of all the question of field of view. Now, these detectors in many cases have very broad field of view. For example, this detector is actually not so broad, 15 degrees field of view. This latest observation has about 18 degrees field of view, but then there was this huge 130 degree field of view on the solar maximum mission satellite. The solar maximum mission was up for nine years, from 1980 to 1989. It did not observe the galactic center continuously because it was a solar mission. It observed the galactic center once every year for about two or three months. So this shows some kind of average at selected times every year. But again, there are fairly big discrepancies.

Then there are observations with similar detectors, or actually the same detector, which show real time variations. This is also done in the same detector, so there is some time variation. We'll get back to this. So there is as you can see some confusion going on when we just look at the data like that.

However, the first thing that we can do is order these data according to detector field of view. So this is the same data as before, but now ordered as a function of detector field of view. Here is this SMM observation, 130 degree opening angle, and here is the OSSE observation. We cannot talk about the single angle. Jim Kurfess went through that rapidly, but the OSSE detector on the COMPTON gamma ray observatory has an 11 by four degree field of view. So I plotted this very small field of view at 11 degrees.

You can see that there is a general trend of the half MEV flux increasing with detector field of view, although there are internal variations that we will have to sort out as we go on this afternoon. But there is this general trend, which of course means there is some sort of distributed emission or diffuse emissions throughout the galaxy, and as one opens the detector field of view, one gets larger and larger fluxes.

Now, let me show you a few examples of further data so you get a better feeling of what is happening. Here is the fairly recent observation done with a germanium detector on a balloon. This is flown in Australia because the galactic center goes overhead exactly. Now we can see that the line is actually quite narrow. The width of this line is only 2.9 kilovolts. It is by the way precisely at 511 kilovolts. The error in the line center in these observations is on the order of a quarter kilovolt out of 511 kilovolts, so it means that annihilation takes place in a medium which is essentially moving at low velocity or not moving at all. In fact, the positrons are annihilating at a large distance from any kind of compact object, because otherwise we would get a gravitational red shift. So we get a nice precise line at 511 kilovolts.

This is pointing toward the galactic center, as you can see. This observation also to observers Teagarden and Lowenthal -- I will have to apologize that many of the names are not written down, their instrument itself is mentioned in the references here. When the detector was pointed away from the galactic center then the flux was perhaps there, perhaps not, but actually quite weak in any case. So there is a line from the galactic center, probably with a fairly narrow longitude distribution, because if one goes away 25 degrees from galactic center, the flux is much lower.

Another observation from a galactic center. In the present talk I will not exploit this fact, but the reason I'm showing this is that there are probably some interesting things going on. This is just another observation, again a germanium detector finding a line which is actually narrower. This is still a debated issue whether in fact the width of the line has changed or not, but this observation which was on a different time, to the lower flux and perhaps a line with a narrower width. So there might be even changes not just in the flux, but also in the shape of the line.

I already showed before that if one goes away from the galactic center, the flux diminishes. This is OSSE data. What I showed you in the first Vu-graph was a spectrum which showed the line with the process going in continuum. That is this flux. For whatever it is worth to people who don't work in this area, the flux in this case was about 2.7 times ten to the minus four. But the OSSE also observed fluxes away from the galactic center. You can see, the arrow bars are quite big here, basically indicating there isn't much flux and they barely observe it. Just to reinforce the notion that this emission is fairly strongly peaked towards the galactic center. The GRIS observation I showed you before was carried out at -25 degrees. Again, the flux was quite low, and you will see in a few moments another picture for that. But again, you can see this concentration of flux toward the galactic center.

So what can we do with this kind of data? We have to somehow model it. Contrary to the previous talks, I will actually try to develop a model here for all of these observations. I think one of the simplest ways to approach this is to say that we are dealing with two components. There is the steady diffuse emission, again based on the fact that as we open the field of view we see more and more flux. There is variable emission from at least one source, perhaps more sources at or near the galactic center. That is based on the time variation. You will see later on, there is even more direct observation, but at the moment, let me just make these two assumptions that there is steady diffuse emission based on the opening angle and variability or variable compact source based on the variability.

So let's talk about a diffuse source before we go to the variability. The basic constraint on this diffuse source is as you've seen. It's that it has to have some kind of distribution over the sky such that it produces low fluxes for narrow field of view detectors in the plane. In other words, if we move the detector away from other galactic center, the flux goes down, the emission is strongly peaked towards the galactic center. Obviously, if we have really good detectors, by now we wouldn't have to play with all these things, but because of the nature of the data given to us, we just have to approach it in a fairly simplistic way.

But anyway, the diffuse component must be peaked toward the galactic center, but at the same time it must produce a fairly large flux when looking towards the galactic center to account for this SNM data that I showed you before. If we look at the galactic center with a broad field of view detector, we see a large flux. So we have to come up with some kind of spatial distribution which is at the one hand peaked but also large enough.

So one searches for what is possible, and it turns out that using the distribution of galactic novae (this is a distribution worked out by Higdon and Fowler in 1987 based on observations of other galaxies) we found that it's possible to produce distribution of diffuse emission which is reasonably consistent with the data. What is also interesting is that this distribution of galactic novae is also similar to the distribution of galactic type one supernovae in space. It's peaked toward the galactic center, but it has quite a lot of emission in latitude and in longitude so that it produces this large flux that SMN has observed. This type one supernova as you will see later on could in fact be responsible for a large flux of positrons that are needed to account for this diffuse emission.

Now, if we make this assumption, we actually use other distributions also. But I will not go through all the details. Let me just use this one distribution. We have to somehow normalize. We don't know how many positrons these supernovae are producing. That's what we are interested in. So we are normalizing this distribution to those off-center points and to the SMN galactic center point, and in fact the picture I showed you before is just that. This distribution here, this diffuse component, is normalized to account for the off-center points, and it's also normalized in such a way to account for that large SNM flux which you cannot see from this picture, which then leaves a certain amount of flux for this off-center point.

Of course, there is latitude here, and it would be possible to raise this. This is what the big question is: was this flux due entirely to this diffuse component, or was it due to perhaps some point source in the galactic center? It turns out there are many more observations of the galactic center looking for the half MEV line from OSSE that have not been released yet. When I get to give this talk again a year from now, I'm sure I think I will have more data points. But at the moment we have to work with just these data. As you can see, there is a certain hint there that there is emission over and above this diffuse component from the OSSE data.

Now, if we go to those balloon observations, this is the same balloon instruments, the germanium spectrometer, the GRIS gamma ray imaging spectrometer. It's a detection system developed at Goddard by a group led by Bonner Teagarden. By now, they have four flights from Australia observing the galactic center. Here are those four flights. This was in early 1988, this was in late 1988.

These are the data, and this is the diffuse component I mentioned before, normalized as I mentioned before to the off-center observation. It's known by the same way as in the previous Vu-graph, and clearly at least this highest point requires extra emission. There is no question about that, that the highest point does require extra emission. Perhaps these points could be somehow accounted for, obviously this point can be, but that point up there does require some extra emission.

Before I get to the point source itself, let's continue talking a little bit more about the diffuse component. There is a diffuse component. What is it due to? Once I go over that, we'll go back and talk about this point source or perhaps collection of point sources.

Once we have normalized this diffuse component, we can actually derive a number. This is the number of 511 kilovolt photons produced per second in this diffuse component, about 10^{43} 511 kilovolt photons. To get a positron production, one has to take into account the fact that some annihilation is via positronium. I mentioned at the beginning, it's not a big number. This changes to about 1.6. If somebody wants to ask about this later we can talk about that.

Now, there is a guaranteed source of positrons in the galaxy, in addition to cosmic rays. There is a guaranteed source, and that is aluminum 26. Aluminum 26 is a radioactive isotope. It decays at a million year lifetime into magnesium 26, and in so doing emits a gamma ray line at 2.8 or 9 MEV, and 82 percent of the time also positrons. This line has been observed by more than one instrument, including GRO most recently. One can use this line to estimate how many positrons are coming just from aluminum 26. It turns out it's actually a fairly small number, so other sources in addition to aluminum 26, which is a guaranteed source, must produce something on the order of 1.4×10^{43} positrons per second. So this is what we need in order to produce this diffuse emission.

So what are the possible sources for the diffuse emission? Here is a list, obviously not complete. Here is aluminum 26 itself. There are some obvious sources. We mentioned cosmic rays a number of times. We know how many positrons cosmic rays are producing. We see the gamma rays that the cosmic rays produce, and we can calculate that. It is quite small.

This is what is needed. This is an older Vu-graph. We can see 2.1 times ten to the 43. Before I said 1.4, so the numbers change a little bit as we go and reanalyze things, but basically this is what we need. Cosmic rays are just not producing enough. There are other sources. One can think of a gamma ray burst.

This Vu-graph was made before all the recent excitement over gamma ray bursts, as was mentioned in Jim Kurfess' talk. I still think that gamma ray bursts are probably not the dominant source. There are other things. Pulsars perhaps contribute. We may be wrong in this calculation here, but I think that in addition to aluminum 26, the dominant sources are cobalt 56 and scandium 44.

Cobalt 56 was in fact directly detected from supernova 1987A. That was a type two supernova. That means there was a tremendous amount of hydrogen envelope on top of the imploding neutron star. Whatever positrons are produced from cobalt 56 will annihilate in that supernova and will not populate the interstellar medium, so that's not the source for us in this case. However, from a type one supernova there are plenty of positrons produced, there's much less envelope, and it's enough for a small fraction of the positrons to escape. I won't bore you with the numbers, but this is the fraction of positrons that one somehow calculates to escape from the envelope into the interstellar medium. These are the positrons that can contribute. Something on the order of an escape fraction of .05 in fact can produce enough.

This is another radioactive chain. This starts out as titanium 44, scandium 44, calcium 44. Here the lifetime is only 55 days. Here the lifetime is about 30 years. This is also an important source, and as I mentioned before, the rest are not terribly important.

Another way to look at this, what these isotopes produce, is this picture from a recent paper by Chen and Linglefeld. Let's just look at this side of the graph, so that you get the feeling and also appreciate the importance of what this means. This is the current rate of iron nucleosynthesis. Now, you have to realize that all the iron in the galaxy comes from cobalt 56. So by measuring the positrons we learn something about how much iron is manufactured currently, because we are talking about currently. Once these positrons get out into the interstellar medium, they only live there about 100,000 years on the average, so we're dealing with the present clearance rate of nucleosynthesis in the galaxy.

So this is nucleosynthesis rate in the galaxy in solar masses per hundred years. Based on theoretical models and also on rate of supernova explosions, depending on the Hubble constant, one would expect that this current state of nucleosynthesis is between .3 or .4 solar masses per 100 years out to one and a half solar masses per 100 years.

This is the escape fraction that I mentioned before. Without boring you with all the models, there are two crucial numbers here. Either the positrons are totally trapped in the expanding ejecta, in which case they only survive after everything has opened up, or they move freely in that expanding ejecta. So you have a big range here. Up to ten percent escape and down to .003 escape.

So to make a long story short, the current rate of iron nucleosynthesis consistent with all of this is a little bit less than one solar mass per 100 years. Its escape fraction is somewhere here, .01. In fact, this dividing line is between where cobalt 56 and scandium 44 dominate. Of course, aluminum has been subtracted, and we did not bother about all the other sources that I mentioned. They make small contributions.

There could be things that are wrong with this story. I don't guarantee it a hundred percent. But I think, given everything that we know now, we have a pretty good understanding what the diffuse component is. The galaxy is full of positrons. At least 50 percent of these positrons come from the current nucleosynthesized iron 56. The rest comes from scandium 44 going to calcium 44. Of course, there is much less calcium 44 than iron 56 in the galaxy, but the reason that they are comparable is because the calcium decay chain is much longer lived, so the positrons have a better chance of getting off into the interstellar medium.

So obviously, what we want to do and OSSE is actually doing right now, is to map out this diffuse emission and see more about the real nature and the real distribution. But this is what I can say so far.

So now let's get back to the point source. Now we know something about the diffuse emission. We subtract this diffuse emission from the data, and this is what we get when we subtract. We were careful to do the errors right. Again, this is done with Dr. Chen here and a graduate student, Jeff Kiebow. This is now the data that you saw in the beginning with the diffuse components subtracted. So this is presumably due to some variable point source, and here it is.

There are good measurements. I think there is real evidence here for some emission from this variable source. Then in 1980 it presumably disappeared, although it could have reappeared here; there was not enough measurements. In 1988, there was a period of activity here. These are the GRIS observations I showed you before. I'll get back to the source in a minute. This is actually quite important.

This is the OSSE observation which may or may not have a point source distribution. It's very close to zero, but it's hard to tell. This is this new GRIS observation, two weeks old. I'll get back to these upper limits in a second.

So there is something going on, and we probably have some source of narrow line emission. It's a narrow 511 KEV line emission. What happened recently -- and we have to say thanks to the French and your countrymen working at ICCI who have brought us Sigma and Granat -- and this is what has produced quite a lot of extra excitement in the field.

This is a general map of the galactic center, just to get the feeling of what we are dealing with. This is longitude and latitude. Note the source 1E 1740.7. It's a little bit less than a degree from the galactic center. These are various other X-ray sources. Any of these in principle could be the source of this variable emission. Maybe some of them are; I really don't know.

Now, narrowing a little bit down the angular size, this is the Granat Sigma results from a paper by Sutnyavidol. Here is the galactic center itself, Sagittarius A. Here is the source 1E 1740. It's quite strong in the spring and fall of 1990. Then in 1991 -- this is X-ray energy, 40 to 110 kilovolts -- in the spring it actually disappeared. So it's variable in X-rays. If one looks only in the 300 to 600 kilovolt range, in one day in October, 1990 it was very strong and there was nothing else.

In fact, this is summarized again from the paper, I'll call this source the 1E source. 1E stands for Einstein Observatory. Here is normal state, these points. This is a low state when it wasn't there, and on this one date it produced this feature, this source. It looks very much like annihilation radiation. This is actually the count, but the peak energy is not at 511 from this source. But it looks like something is taking place there. So maybe this is the source.

When people saw that, everybody was very excited. Maybe this is the source that we are looking for. Now let's see, is this the source, yes or no, and what can we say about it? Small summary of first of all this 1E source, as far as annihilation radiation is concerned. The line width is very broad, 240 kilovolts, admittedly with large error bars. The line center, admittedly there are errors, but it's not at 511. The flux is large, as you will see. I'll get back to the UV cycle in a moment.

This narrow line source, we've never seen it as a source. We just infer it from the variation. But its line width is very narrow. It's precisely at 511 kilovolts, and the flux is also variable, but it's not that high as this one. Now, cycle is a very difficult thing to establish, based on a very few observations. These are really estimates that, because time is running out, I will not try to justify them. It seems to me that this source is on -- this narrow line source is on more often than this 1E source, which was only once in something like ten to 20 observations. That's why I'm saying ten percent.

I should say right now that I have another sentence on that in the summary. Another source, Nova Musci, which is not in the direction of the galactic center. It's an X-ray nova which appeared in 1991, also showed a feature like this. There might be another source as well, and perhaps even Signus S1 has something similar to this. But as far as the galactic center source is concerned, this is the most important source.

So the two sources are not the same. This hypothesized narrow line source that I'm talking about may be connected to this 1E source, but it's not the same thing. What also happened fairly recently after the discovery of this 1E source was work by Mira Vellethal and even before that by Bali and Leventhal, who identified the molecular cloud aligned with the 1E source. The 1E source is actually here. These are radio sources. There are two radio sources that were discovered in the VLA, and most recently a jet was located here.

Now, this is a molecular cloud -- on the gamma ray of course much larger, on this order. So it's possible that this 1E source is somehow related to a molecular cloud. This is actually interesting, because if it's related to a molecular cloud, then we can in fact -- at least we can try, but I think we have a chance then to explain these differences, in other words, the difference in width, the difference in alignment and even a difference in duty cycle we can try to explain. It's thought that this 1E source is in fact a black hole. It's thought; it's not proven, of course.

Now, in order to do that, the idea is then that the positrons are released from this black hole, a fraction of them get out. The black holes are powered by accretion and the accretion is generally forming an accretion disk. So one possibility is, the positrons by a mechanism that I have no idea -- at one time I thought I knew, but now I don't think I know, so I don't want to say how the positrons are produced -- but a fraction of these positrons get into the accretion disk and produce this broad line, just by thermal broadening, maybe by Keplarian motion also. So this broad line, slightly red shifted because of the gravitational wrenching, is produced in the accretion disk. I won't say more about that, but perhaps three times as many positrons or some fraction actually get out into this cloud. That's a much easier physics to treat, and we have done that.

Now we inject positrons into a cloud. I don't know how much time I have. This physics is not so difficult. So if we start off with positrons about one MEV and put them in a gas and they lose energy, and once they got down to a fairly low energy around maybe 100 electron volts, they can charge exchange with neutral hydrogen and produce positronium, which then decays into two or three photons, depending on which state the positron is formed.

This is a very rapid process. This happens essentially instantaneously. The lifetime of a positron is also very short. But then a fraction of a positron can actually pass over the threshold for positronium formation and become thermal positrons. If the medium is neutral and has no dust in it, then only these two channels are important. Then the positrons which have not formed positronium in flight, all they can do is annihilate to the hydrogen directly.

This is a very slow process, compared to the first one. So if you inject these positrons, what happens is, some time passes because of energy loss, and then one gets a big peak of annihilation from the first process, and there's a long tail annihilation from the second. This is what happens in a neutral gas. I won't bore you with an ionized gas because probably we'll run out of time.

So this is just what this figure shows. Delta function of injection. If we inject at T zero into a cloud of density of ten to the five, a cold cloud, totally neutral, the different curves here are for different incident energy of the positrons. We don't use positron beam, but actually a Gaussian, just so things spread out a little bit.

This is the peak of the Gaussian. So here is this first peak that I mentioned before, and if the energy of the positron goes up, it takes a longer time for the positron to slow down and the peak is moved down this way. But let's look at this peak. So we get this first peak from the charge exchange in flight, and then we get this long tail from annihilation of the positrons which have already thermalized.

Now, this has also consequences on two things, on the positron fraction and the number of positron that annihilate via positronium. This hasn't been totally exploited yet, but in the beginning, because everything goes through that charge exchange channel, we have 100 percent positrons annihilating via positronium, and then when we get that long tail it drops down to a very small fraction. The line width, interestingly enough, changes also. This has not been exploited yet, but I think we pretty soon will find uses for it.

It turns out, when the positrons form positronium flight by charge exchange, that positronium moves quite rapidly. So in the beginning, the line is quite broad, about six kilovolts. But then once they thermalize, the width of the line is determined by the atomic motion of the electrons with which the positrons annihilate. So the line width actually becomes quite narrow. These are for ionized gasses that I won't go into. So there's a change also in the line width.

So one can play all these games. Rather than inject the positrons that build the function, one can also inject continuously for awhile. This is injecting the positrons continuously. In this case, the positrons are injected into the cloud continuously for a year, which is this line, and then stopped. So then it starts rising here, and because they were injected only for a year, it comes down and we get this long tail just as before. If the positrons are injected continuously for three years, it looks like this, and if they are injected for ten years or for infinity for that matter, it will eventually achieve steady state at that level.

This by the way is normalized to the real conditions that we think might exist. So really, the model that we are trying to push is one in which we take the positrons from this 1E source, and put them into the cloud. This can explain a number of things. It can explain why the line is very narrow. It can also explain a duty cycle, because this whole emission lasts much longer. As far as the width is concerned, the situation is a little bit confused, but it's possible that there are these changes in width that I mentioned before that may have to do with these things. Again, if you want more details on this, perhaps we'll leave it to the questions.

But just to get back to that table I showed, where I show the differences between the 1E source and this assumed or postulated narrow line source, could simply be due to injection into the cloud. This has an interesting prediction. Obviously, that point source is variable, but at some low level it should be there essentially always. You see, this low level of emission actually lasts for a long time, for ten years. So actually, my prediction at this point would be that this variable point source should be there essentially always at the low level and sometimes at high levels, as indicated by the observations.

So let me summarize then and start from things that are definitely observed and strong conclusions, and things which are more tenuous.

First of all, there is absolutely no question that galactic E-plus annihilation radiation was observed. In gamma ray astronomy until very recently, every observation had to be questioned. I don't think there is any question that whenever you pointed to a galactic center, you will see annihilation radiation.

We have a diffuse component. It requires something on the order of ten to the 43 positrons per second in the galaxy. This may be off by a factor of two or so. The most likely sources are cobalt 56 and titanium 44. Aluminum 26, which is also guaranteed to be there, produces something like 15 percent of the total. Other things like cosmic rays, pulsars, gamma ray bursts. We could be wrong, but they produce less.

There is still the question of these black holes. Do they populate general interstellar medium? It's hard to tell. There doesn't seem to be a need for them, but perhaps there are other things. These may not be the most important sources, although the spatial distribution is good, especially cobalt 56 which is produced in type one, which will have this nova distribution, which we found to be nicely in agreement with the data.

Now we have the diffuse component. There are point sources. I mostly talked about this 1E source. I mentioned Nova Musci, there might be a couple more. These are real, they have been observed. They produce broadened red-shifted features, presumably black holes producing pairs in some unknown process, maybe photon-photon interactions very close in, it's difficult to tell. The broadening I think is due to thermal broadening in the accretion disk. The red shift may in fact be due to the gravitational red shift of the black hole, which is very exciting if we can begin to measure that.

I did not mention it, but the Nova Musci, which has nothing to do with the galactic center because it's way off, there is another line feature at the 170 kilovolts very clearly in the data, which is due to the back scatter of 511 radiations. Almost for certain this accretion disk, in addition to producing this broad line, also back scatters photons and produces another line feature.

So these are very real objects, and a very interesting field of research. We have a source of variable 511 lines which has never really been seen directly. By that, I mean actually imaged. But from the variability of the line observed with broader field of view detectors, we conclude that this source exists. Then this has to be reconciled somehow with the 1E source, and what we are proposing is that the reconciliation is via this molecular cloud.

Now, the question then is, back to the question of the black hole candidates. What powers these black holes and what causes the variability? Well, it has been suggested both by Mirabelle and Bali that actually the black hole is powered by accretion from the cloud itself. It doesn't have to be, because black holes can be with a binary companion which would power it.

Now, if it's powered by the cloud, then one can at least speculate about the mechanism which produces the variability. These are just inhomogenieties in the cloud. There is no question that the cloud is very inhomogeneous. As the hole goes through the inhomogeneities, the rate of accretion will change. So there might be periods when the rate of accretion is high, just because there is more gas to interact with. Other periods when the rate of accretion is lower, and then the source is in a low state.

The variability of this line on the longer time period is probably on the order of a year, it appears. The velocity of the hole through the cloud just to produce enough accretion has to be on an order of ten to 20 kilometers per second, just to create enough. By the way, these holes are thought to have masses on the order of ten to 50 solar masses, at the most. So in order to produce this variability, the spacing between clumps in the cloud, if you want to think about it that way, is actually quite small, if that is the mechanism, perhaps on the order of ten to the 15 centimeters. But this is just one possibility.

So this is my summary, going down from things which are really definite to things which are questionable and still to be found out.

What to do in the future. I think I would like to see identification of the narrow line source, in other words, observe with good angular resolution the source of narrow 511 KEV emission, and see is it indeed coincident to the 1E source or not. Obviously, one could continue further monitoring these black hole candidates, discovering other ones. We would like to map the diffuse component. After all, this is an important galactic component, is related to the current rate of nucleosynthesis and what kind of spatial distribution it has.

Fortunately or unfortunately, it's peaked at the galactic center, so everything is very confusing. Where it's peaked also there are one or more variable sources, so my whole talk has to do with disentangling these two components.

Finally, one should be going into high resolution spectroscopic studies to observe the positronium and the width of the line. I've alluded to these questions, but I didn't speak to them in detail. But I think in the high spectroscopic mode, one will find out very interesting things such as gravitational red shift, motion of positrons through clouds and things like that.

So I'll stop at that point. Thank you.

PARTICIPANT: Could the source be Saittarius A?

DR. RAMATY: It could be, except for the fact that Sagittarius A shows no hard X-ray and gamma ray activity. Other than that, it could definitely be Sagittarius A. In the past, we had an argument based on the production of positrons. The size of the source was quite small. I don't know how strong that argument is, but the Sagittarius A star does not show any high energy activity, so it's difficult to associate with it, whereas the Einstein source does show high energy.

WHAT PROBLEMS OF PHYSICS AND ASTROPHYSICS SEEM NOW TO BE ESPECIALLY IMPORTANT AND INTERESTING?

V. L. Ginzburg

First of all, I would like to thank cordially all the participants who came, and especially I am obliged to all the speakers and organizers, Alan Clark and Edgar Edelsack. Also, I think it is very bad when somebody takes part in the organization of his own celebration or something like this. That is why in spite Alan says it, I would like to stress again that I have had absolutely nothing to do with the idea of this seminar and its organization. I am not guilty.

Now, about my talk. I would like to speak about some partly educational enterprise in which I was engaged. A lot of the problems of physics and astrophysics seems now to be especially important and interesting. I will speak about these.

This title, "What problems of physics and astrophysics seem now to be especially important and interesting", I know from practice, is very dangerous indeed. Why? Because I heard a lot of accusations. What is important and what is not, it is immodest to say this, is the first accusation. The second accusation is, does a special and important problem exist in reality, and what purpose to select it. So I must stress that first of all, this accusation or quasi-accusation is mainly due to misunderstanding. It's in the beginning of all this a pedagogical idea. In fact, I wouldn't make a secret that I pay more attention than simply pedagogical to this. But in the beginning, it was really pedagogical.

The point is that I believe that everyone who have something to do with students or young physicists have noticed that many young physicists have very narrow horizons. I was astonished with this. For instance, he knows some very fine things about quantum electrodynamics, et cetera, complicated material and very fine things. But asked what is the structure of a neutron star or even the nature of superconductivity, what is the mechanism, they do not know. It is very strange.

At the same time, the breadth of the horizon is very natural. Physics is exciting, interesting, but even from a pragmatic point of view, even from the point of view of obtaining new results and have new ideas, it is also important to have wide horizons. So I ask myself, what can be done in this direction.

So many years ago, I organized when I was teaching in Gorky and later in Moscow special lectures about these most important problems which exist. From this came a paper published the first time in 1971, so I have engaged in these things more than 20 years. The last edition in English was published in 1985. The title is Physics and Astrophysics, the Selection of Key Problems. Also, I proposed a new edition. But permit me to say an old joke. The old joke is that "the permanent thing in the former Soviet Union is temporary difficulty". Due to this temporary difficulty, it is not published yet in spite that it is already two years in the publishing house. But I brought to the United States the xerox copy, and I hope it can be published after two years. I don't know, it has to be translated, and it is 600 pages. It is not only this, it is some other material, too.

I can also mention that in Physics Today, in May 1990, some comments were published, and I would like now to explain my method, my approach to advocate them. In any case, to popularize physics and physics problems I make a list of most important and interesting questions. Of course, this list is not only the questions, but also comments to the questions.

So the method is as follows. I have a lot of problems, and comments to these problems. I am absolutely sure that it is very easy in general terms to explain to physicists, not quantitatively, but qualitatively these things. For instance, about neutron stars. It's enough to say many very simple things about neutron stars. It is very easy to explain why the magnetic field is so strong, why inside it is superfluid and superconducting, all these things. It is no need to write many pages. It is easy to do in short terms.

So here is my approach. But of course, it is impossible to speak about everything. There exists a very good formula: everything about something, and something about everything. But practically it is impossible to have very many problems. So I must do some selection. Everybody who uses this approach must select something. That is the difficulty.

The young people, I don't know their reaction. I know that many of them use this book, but I don't know the reaction, because they are rather shy. About colleagues, the reaction I would say was without much enthusiasm; thus, especially here is one important question. My list ignored many problems. You can say that everything in physics is interesting. People can be not interesting, but physical problems are interesting. But I must select. As a result of this selection, many things are absent. To some of my critics, the absence of some problem in the list on which they work themselves means that the list is deficient. My one old friend, he mentioned to me that "If you had published this paper about the most interesting problems before you were elected to the Academy of Science, you would never have become member of the Academy".

My beloved problem in physics is radiation of uniformly moving sources. It is Vavilov-Cherenkov radiation, Doppler effect, transition radiation, transition scattering, et cetera. But of course, this problem is not on my list, because it is not mysterious, not especially important for the technology. So I do not follow this rule that what is interesting to me is interesting to everybody.

Now, the proof of the pudding is in the eating. I would give you the list. This list is divided in three parts: macrophysics, microphysics and astrophysics.

Macrophysics

1. Controlled nuclear synthesis (fusion).
2. High temperature superconductivity, superdiamagnetism.
3. New subtsances (metallic hydrogen, etc.).
4. Certain problems in solid state physics (see below).
5. Second-order and similar phase transitions (critical phenomena).
6. Surface physics.
7. Liquid crystals, very large molecules (polymers, fullerenes).
8. The behavior of matter in very strong magnetic fields.
9. Rasers (x-ray lasers), grasers, and ultra high-power lasers.
10. Strongly nonlinear phenomena (chaos, turbulence).
11. Super-heavy elements, exotic nuclei.

Microphysics

12. Particle mass spectrum (quarks, gluons), quantum chromodynamics.
13. Unified theory of weak and electromagnetic interactions, W and Z bosons, leptons.
14. Grand unification. Proton decay, neutrino mass, magnetic monopoles. Superunification, supersymmetry, superstrings.
15. Fundamental length. High and superhigh-energy particle interactions.
16. Nonconservation of CP invariance. Nonlinear phenomena in vacuum and superstrong electromagnetic fields. Phase transitions in a vacuum.

Astrophysics

17. Experimental verification of the general theory of relativity.
18. Gravitational waves.
19. Cosmological problem. The connection between cosmology and high-energy physics (microphysics).
20. Neutron stars and pulsars.
21. Black holes, cosmic strings, textures.
22. Quasars and galactic nuclei. Formation of galaxies, hidden mass (dark matter) and its detection.
23. The origin of cosmic rays and cosmic x-ray emission. Gamma ray astronomy at superhigh energies.
24. Neutrino astronomy.

Starting now with macrophysics you will see, for instance, controlled nuclear synthesis, fusion. It is a problem which is possibly not so mysterious, but so important to mankind, so nobody would disagree that it would have to be on any list of this kind.

The second problem, high-temperature superconductivity. It was on my list even in 1971, but because high-temperature superconductivity now is somewhat achieved, everything on this list must be a function of time. When so a problem is solved, you must cross it from the list; but I think that high-temperature superconductivity, it is not time to cross from the list. You have seen from today's talks that many things are not clear, so it is in the list. But I a put new point, is there room-temperature superconductivity? I will speak about this at the end of my talk.

Now, superdiamagnetism. What is superdiamagnetism? It is the search for substances with very high diamagnetism. It is possible in principle to have diamagnetics whose absolute value of susceptibility is two orders of magnitude higher than existing diamagnetics.

New substances. For instance, metallic hydrogen. I cannot comment. Of course, when I give a special lecture on this subject, I can comment on every point as it is done in my booklet and my paper. But I cannot do it here because it would be impossible in a short time.

Now, certain problems of solid state. There are quantum Hall effect, metal-dielectric transition, charge and spin density waves, some things like this. Second order and similar phase transitions, critical phenomena. There are a lot of important examples, beginning from helium two and helium three, et cetera. (Now, helium two I have in mind superfluid helium and helium three is an isotope.)

Surface physics. Liquid crystals, very large molecules. Here I add Fullerenes, carbon 60. Now it is unavoidable to have on the list. The behavior of matter in very strong magnetic fields. We heard today on the pulsars and that most probably, gamma bursts originate in neutron star surfaces. It was shown on the screen peculiarities in spectra that really exist. It is unavoidable in this case to have neutron stars, because I don't know any object on which you can have magnetic field strengths of 10^{12} oersteds.

The matter in such strong fields behaves quite differently. For instance, iron. Iron in such a strong field will be a very thin needle with two iron nuclei. It is the lowest state of iron in such a strong field. So to understand the surface of a neutron star, you must know this.

The next is X-ray lasers, gamma ray lasers and ultra high power lasers. The ultra high power, I have in mind the power which is several orders of magnitude higher than in existing lasers. Now comes strong nonlinear phenomena, turbulence, and attractors. The last is heavy elements and exotic nuclei. Of course, nuclear physicists would object, but I believe first of all that nuclear physics is now nearer to macrophysics than to microphysics. Also, in selection it is possible here to add something, and I selected super heavy elements. I mean in atomic number larger than 110 or 109, so it is beyond 109, 110. Exotic nuclei, for example super-deformed nuclei, or nuclei with larger density.

Now, the next is <u>microphysics</u>. We have a lot of particles, but we don't know why the mass of the mu-lepton is 200 times higher than the mass of electron. Quark-gluon plasma, quantum chromodynamics, the dynamics of the quarks are all problems. For instance, I wouldn't be as surprised if the Nobel Prize soon will be given to people who show that there are only three types of neutrino. It is great achievement, but it is very difficult to know who; it was a team of hundred people. How they selected who, I don't know, but they managed to do this.

Now, the Higgs boson is the last very important particle needed in weak interaction-electrodynamics unification. In this moment, it is the main problem to observe. According to last figure I know, the mass of the Higgs is larger than than 57 Gev. One of the purposes of the superconducting supercollider is just to have the creation of this Higgs particle.

Grand unification. This means unification of weak and electromagnetic interactions with the strong interaction. As a result, proton decay, neutrino mass, magnetic monopole are all part of this super unification, that is, unification of superstrings, fundamental links, high and super-high energy particle interactions. I do not claim that I know all these things in depth, but some things I know, and it is not so difficult, I believe, to explain to physicists what is meant for every point here. So, these include fundamental lengths, high and super-high energy particle interaction, non conservation of CP variants, non-linear phenomenon in vacuum and in super strong electromagnetic fields, phase transitions in vacuum, all of which are very important for cosmologists.

That is all for the microphysics, and now astrophysics. By the way, in the Soviet Union, astrophysics is somehow divided from physics. Here in the United States, when you see in Physics Today the achievements at the end of the year, astrophysics is part of physics and it is part of astronomy. This division is not important. My first point here is experimental verification of general relativity; of course it is physics, but it is here because general relativity is used in astrophysics. Now it is here gravitational waves, the problem of generation is very difficult. Here in the United States and in Europe are now built detectors. Unfortunately, you have to wait several years to use very large interferometers to observe gravitational waves. It will be a new source of astrophysical information. We have radio frequency spectrum, and Dr. Kellerman today spoke about the achievements, how wide is the spectrum. But we also have several other channels of information. Cosmic rays is one channel, possibly not the most useful mechanism. Neutrino and gamma rays, and in principle there are also others, but possibly not so important.

Now for cosmological problems, the connection between cosmology and high energy physics -- everybody knows something about this -- is very popular. Neutron stars and pulsars, black holes, cosmic strings, texture, like black holes, and other singularities in the universe -- and galactic nuclei, formation of galaxies, hidden mass, the dark matter problem and the possibility of detection of dark matter. I believe this is one of the most significant problems affecting astrophysics now. Now also the origin of cosmic rays and cosmic X-rays and gamma ray emission, including gamma bursts, for instance. Everybody, every physicist must know what is a gamma burst, what is the situation. Gamma astronomy and super-high energy gamma astronomy is also a very interesting thing. A few years ago, it was proposed to be observed gamma radiation with energy 10^{14} ev and larger. Now there are no signals, so it is two possibilities. It is a function of time or it was an error, but in any case it is a very hot problem.

So that is my list, in fact. Of course, as I mentioned already the division into microphysics, macrophysics, astrophysics is quite conditional and also the 24 problems. Why 24? I wouldn't insist that it is 24. It is possible to add many things. For instance, when you have some discussion in physics today, they say why you don't include formation of stars? All right, I agree, please include formation of stars. Laser cooling, tunnel microscopy, atomic force microscopy, and many others. You see, it is my approach, if we speak about the educational point of view, it depends from the teacher. Everything can be on the list, but I believe that my list can be added to, and I hope there will be some discussion later on. But in principle, I include the things which have mystery and are very important from some point of view.

Now, one more remark. Here there is no biology. It is not because I do not understand that biology is great. I would say that I like physics very, very much, but nevertheless I would put biology even before physics, because such things like the origin of life or the functioning of the brain are great problems. But first of all, it is impossible to embrace everything. The second point, I don't know those problems and what I can say? In this case, I cannot give you a sensible commentary, so I haven't included them in the list.

It is possible to speak about what is the background of every problem. I would like to say very few things are needed to explain the background for instance about fusion. To say what is the reaction, thermonuclear; what is the method, inertial or new catalyzers. But some remark about cold fusion I also make. Cold fusion is not existing. But the progress of science is better served by publication of wrong results, which can be verified by other workers, than by withholding publication of results until confirmation. I say this in connection with cold fusion.

Now, about superconductivity. Of course, in the lecture I explain a lot of things about superconductivity, about superdiamagnetism. But I didn't do it today, but nevertheless I would like to say something about high temperature superconductivity. The point is that beginning from 1964, it was a dream to have high T_C. Now we have high T_C. There are questions to explain, et cetera, but I believe strongly that the place of high T_C before observation is now room-temperature superconductivity. The question is, what is the highest possible critical temperature for superconductors. I speak about real superconductor, real material. Neutron stars are superconducting, but for a neutron star the critical temperature is

10^{10} kelvin, because it is nuclear forces. I will not speak about this, I speak about real materials and even not about metallic hydrogen. Metallic hydrogen probably has a very large critical temperature, and according to some calculations metallic hydrogen can be superconductor even at room temperature. I speak about substances like cuprates, like some others, which are not available now but can in principle be available.

This question is open, the question about the highest possible critical temperature. But it is a long struggle, and my opinion and the opinion with whom I work, that there are no limitations of critical temperature, no sensible limitations. Everybody agrees that with metal you cannot achieve many thousand degrees, but I mean that something like 300 Kelvin, 500 Kelvin for metals or other compounds, there are no intrinsic limitations which prevent achieving this goal.

Now, a very important point. The problem of mechanism of high temperature superconductivity which was discussed partly today. Let me assume that the mechanism is electron-phonon mechanism. Then it is rather clear that it is impossible to have transition temperature larger than the Debye temperature. So from this point of view, if the materials we know now are electron phonon-materials, it would not be possible to achieve room temperature superconductors. So it is absolutely important to know the possibility to have room temperature superconductivity. But if the mechanism is electronic, for instance, or magnetic, or even RVB (in spite I dislike this RVB) some limit is established and in these cases it is a possibility.

I believe the mechanism is excitonic, but who knows? But the principal equation that if it is electronic, even magnetic, I don't like magnetic, but nevertheless if it is magnetic, it is possible to have high temperatures. It is not a prediction, what I am saying. A prediction is something real. It is just a dream. It was dream about high T_C, now it is some dream about room temperature.

In conclusion, I believe that if it is possible to say, to start an enterprise with this list, with the possibility to answer the question how to educate young people, it is justified to say it is interesting. And to say the truth, I think it is interesting not only for the young people. For everybody it is interesting to look about possibilities in physics, about hot spots. What is very strange is in my publication, I several times ask, please, give us your list. I do not insist that my list is better than any other. Let us compare the lists, let us discuss the problem. But nobody likes to do this. It is not an easy job. Somebody would accuse him of something. Many people criticize me, let us permit him to criticize. I in some sense throw the glove now. Please, give your proposals and it's possible somebody will propose something other. I wouldn't insist that my list is the best.

So now you see the situation. Once again, thank you very much for your attention and for coming here.

DR. CLARK: First we'll entertain questions to Professor Ginzburg about his talk, then I will ask the other speakers what their list looks like, and then we'll open it up to discussion. Are there any questions to Professor Ginzburg on his talk?

PARTICIPANT: Magnetic monopoles, do they exist?

DR. GINZBURG: I don't know. Theoretically it is possible. The inflation cosmology somehow explains why it is not observed. But people look, and if it could be discovered that would be a really great achievement. So I hope that nobody would cross it at this moment from the list.

PARTICIPANT: You spoke briefly about superdiamagnetic systems. Could you say a little bit more about it?

DR. GINZBURG: What I have in mind, diamagnetism even of graphite is small; for graphite, as I remember, has a susceptibility of 10^{-4}. Usually in metals you have 10^{-6}. In superconductors, it is minus one over four pi ($-1/4\pi$). It is conditional; therefore superconductor is not quite diamagnetic, but formally superconductor is the ideal diamagnet. So you clearly can have susceptibility of -10^{-2}. It is a possibility. Nothing in principle forbids you to do this.

How to achieve this? I don't know, but it is not by chance that I put this in the list, because there are some ideas about which materials can have such very large diamagnetic susceptibility. Everybody knows that we have electric dipole, magnetic dipole, even two type of magnetic dipoles in principle, because the second type of magnetic dipole is the dipole from two magnetic monopoles. This dipole is not equivalent to normal magnetic dipoles because inside the magnetic field has different direction. But there exist also so-called toroidal moment. I wouldn't explain in detail. It is something like this: take solenoid and put it in a torus shape. Inside you would have magnetic field. If there are no azimuthal currents you would have no magnetic moment. If it not charged, you would have no electric moment. But it is something, it is some electromagnetic system, and this electromagnetic system is just a toroidal system.

Why is the susceptibility so small? It is because usually the paramagnetic effect is stronger. These toroids have no magnetic effect, so paramagnetic effect for toroidal system is specially suppressed. So there are some ideas of such materials (and I can give the literature) in this special class of magnetics. Among them it is possible to have diamagnetism.

I am happy that here, when we discussed the question, it is lively interest by the people; so it is a very interesting problem in physics!

DR. CLARK: Do you have any comment about the early observations in copper chloride of possible superconductivity?

DR. GINZBURG: Thank you very much, yes, I have. I believe it was something. Possibly just the idea of superdiamagnetism came from this, because it was magnetic observations. In case you have larger and larger magnetic susceptibilities, it is in principle two possibilities. I think most probably it was some superconductivity, but superdiamagnetism is also a possibility.

I will tell you an interesting story. The man in the Soviet Union who was engaged in this preparation of copper chloride, but first of all not everybody know about what we are speaking. It was observation here in the Soviet Union that copper chloride (in the United States also cadmium sulfate) have a large diamagnetic moment, which is still there at 200 degrees kelvin. It was possibly high temperature superconductors, but in resistivity there was nothing seen. The other possibility was that it is superdiamagnetic. Of course, if you have nothing in conductivity, it is not against superconductive nature, because it is possible to have inclusions.

But the man who was doing this in the Soviet Union, after this discovery, he was able to expand his laboratory, new equipment, et cetera. But he told me that his main goal was to avoid oxidation. In fact, most probably if it is superconductivity, it is due to oxidation. But what to do?

It is interesting. I spoke with Chu, a well-known specialist in superconductivity. He said, yes, he observed himself that something was in this old observation, but he told me, I am so busy with some other things. It is interesting that in science, the fashion may play a large role. It is now fashionable to do this, and nobody would like to do something else. I already spoke here at NIST about thermoelectric effects in superconductors. I cannot understand why people don't work in this, it is very interesting. But it is not fashionable. In America, there are some people who don't give you money!

CONCLUDING DISCUSSION

DR. CLARK: Since there are no other questions directed to Dr. Ginzburg, I'm going to ask the speakers, do they have any comments about the things on Professor Ginzburg's list, and would they add any of their own?

DR. LITTLE: I'd just like to ask your thoughts on the following: In view of the fact that you have had this broad understanding of a huge range of things in physics, the fashion in physics has been in the other direction, for people to become narrower and narrower. There was a time not too many years ago when most physicists read the whole of Physical Review. Now they usually subscribe to Physical Review Letters, and that's about all.

Do you see any way in which you can change the trend so you can make people, or encourage students at least to develop the kind of breadth that you've shown?

DR. GINZBURG: I don't know what it is; a statement or a question.

DR. LITTLE: It's a question, whether you have any way that you can encourage younger people to develop this broader range, because they seem to be encouraged to follow a narrower way. I know your seminar did very much to fight against this, but are there other ways of doing it?

DR. GINZBURG: How to do this? First of all, not to believe the theoretician, that is first very important point. I'm a theoretician myself, and of course I love and respect theoretical physics, but not about what is possible or impossible. To be against fashion, not to believe in what is fashionable or not fashionable. Even Phys. Rev. Letters, which I respect very much, is not the last word in science.

DR. CLARK: Dr. Kresin, do you have anything you would add to that list?

DR. KRESIN: I think what is most interesting is the overlap between different fields, like physics and astrophysics, between physics and biology ,etc. Speaking of superconductivity, we never have had much chemistry in this field.

In addition, I think that properties of small metallic clusters is a very interesting direction.

DR. CLARK: Professor Gor'kov?

DR. GOR'KOV: I prefer the old-fashioned Landau approach, when you just get a broad enough education for students for them to be able to get in a new area which is interesting for them. I'm afraid I would ask my students to read about 24 subjects. I guess they simply do not have time to work.

DR. GINZBURG: Permit me to make one remark. I would like to stress that I agree with you. It is impossible to address everything, and to learn all these things, of course not. By the way, I also think like one of Landau's students, but even to me he was very, very broad. But nevertheless, I don't permit you to examine me about all 24 subjects. And I would answer you for everybody.

DR. RAMATY: I would like to mention a topic that was not discussed at all. That's the sun. There are two broad areas of study. I think the interior is extremely exciting, what is going on with solar neutrinos right now. There is a beginning of understanding.

What is perhaps less known to this audience is the atmosphere. Solar atmosphere is a magnetized plasma, very active. It has an 11-year cycle, produces flares and other transient phenomena, and I think there is some very interesting plasma physics going on there. But what's even more important is, that it has some relationship to our day-to-day life because it can affect the earth, the climate and so on. So I think the study of the atmosphere of the sun is also important. I'm talking about things that I myself am interested in, so that's why I'm mentioning that.

I don't know anything about these except what I read in the newspaper, but it seems to me that once we go from the sun, getting back to earth, questions like global warming and ozone depletion are fantastic topics. I don't know what we can contribute. I know that I cannot, but I think these are very important questions.

Leaving the sun alone and getting to education, I agree with what you said. But there is one more point, and that is the education of the general public. Because what you are talking about is that physicists should know what physics is, but it's a very important problem also for the general public. In fact, in our country and in other countries also, there is a great fraction of the population that is not only undereducated, but underprivileged. We have seen what happened in Los Angeles, for example. I really don't want to sound like a politician, but I wonder if there is something we can do, in trying to bring in some of these people, to tell them the excitement of physics, and maybe that's going to raise the whole social situation a little bit more.

DR. CLARK: What you're saying is that a grad student in physics doesn't have time to be out on the street. We are now ready for open discussion. Any questions, comments?

PARTICIPANT: This is maybe a question, maybe a comment. To me, it seems that the problem of young people being focused is that physics is taught as if it was a trade. You learn how to operate a machine, you learn how to do certain kinds of calculations or something, then you go get a job to do that. You don't learn to ask the question, what is the right calculation to do, or what is the right measurement to do. Do you think that that has something to do with the problem that you are presenting to us?

DR. GINZBURG: In fact, I don't understand. The best answer in such cases is no comment.

PARTICIPANT: Maybe I can rephrase it. Nowadays, when I want to get a general appraisal of my health, I go to someone who specializes in internal medicine, and he tells me he thinks of himself as a general contractor. He is somebody who looks at the whole system, and then when he finds something wrong, he sends me to a specialist. But the specialists never look at anything but that one narrow part of your body. Maybe what we need is a new area of physics called the general physicist who looks at the whole body, but is not a specialist.

DR. GINZBURG: I propose a formula, something about everything, and everything about something. But this general physicist would be something about everything.

DR. CLARK: Obviously the Ginzburg seminars had lectures that were very influential on our young people. How can we do that here?

DR. GINZBURG: Very easy. First of all, it is usually where I lived, because it was going many years. Next and very important, the seminar has to be Wednesday at 10:00 in every weather, independent of the weather and independent of my presence (I have two deputies). So the people come to this. The people met every week. There are two hours of some discussion and literature and papers, but also they meet. So it was first of all systematic, every week. If it is not every week, it is bad, because two weeks is too long a period. And systematically you should go problem after problem. Now because it has some reputation, people come and ask, may I put on a seminar. If it is interesting, I put it on, sometimes I make a mistake. Many speakers are bad and cannot explain even if they know something, so there are weak points of this.

But the main intention is, in such a large institution as this is, it is very easy to organize such seminar, have some active people who always came. The worst thing is to come only with topics interesting to you. It is impossible. You must come to everything. It is easy to organize, if the senior people of an organization decide it is useful, they can easily organize it so young people will come also. But if all the senior people wouldn't come, of course, it would be bad. That is all I can say. The system works.

DR. KELLERMANN: Do you spend the whole two hours on one topic?

DR. GINZBURG: No, no, of course not. The usual thing is as follows: In the beginning we use topics from the literature. Unfortunately we do not have enough literature, but in principle because it is easy for everybody to look at a journal, so all talk about the news. If it is important news, we have more time or less time, and this is a route to communication, because we have two hours. But if it is a special interest, you must have a flexible system.

DR. KELLERMANN: And people are prepared beforehand with specific pieces of literature?

DR. GINZBURG: No, no, no preparation. They come and they ask questions. If it is clear that it is not enough, you can next time be flexible.

PARTICIPANT: I would like to address the so-called young generation. I always admired the people of Professor Ginzburg's generation. Also, I developed some sort of inferiority complex when I met with these people. What is happening, why are those people so broad and I am so narrow, why can't I embrace such a wide range of physics. But I think the reason is, the physics itself changed. I don't know how many years separate us, but probably 30, 40 years. The physics in these 30, 40 years became very complicated. It's very developed. At this moment, you really can't embrace all those things. You can't work at the same time in solar flares and superconductivity and be proficient. This is the reason why the young generation is so narrow, I think.

PARTICIPANT: I can add one point. That is also an important point, but another one is the system itself. I hear many young people telling, I am concentrating on one area, I want to do something that is very close by, but I cannot try that because my grant goes only for two years, I have to make a product for that purpose. If I change and run out of time, then I'm not going to have any product.

PARTICIPANT: Part of the problem is what this young lady just alluded to, the way our system works in this country on a competitive grant basis. There is not enough time for flexibility. The Canadian system gives every professor a certain baseline funding, $25,000 a year, and it's enough to support a student and some equipment and play in the sandbox, if that's what he wants to do, explore some wild idea and not have to get a grant for it. Of course, if he wants to focus on a particular problem, there's a grant channel also. But at least the system allows a certain flexibility for people to go in a different direction.

Here, we're locked in pretty tightly by the competitive nature of how our system is structured. So to the funding people, I urge a certain amount of flexibility. I don't know how we introduce it.

DR. KELLERMANN: I think the problem is even worse for young people, because the system we have now, you even see advertising, Professor X has an opening for somebody to work on such-and-such a problem. At least up to post-docs, assistant professors, there's been no opportunity for people in the most productive part of their career to work on what they want, or what interests them. In order to get paid a salary, you have to work for somebody on a very specific job.

It used to be, post-doctoral fellowships, you get a position and work at what you want. You can't do that anymore.

DR. GINZBURG: Permit me one small remark. Excuse me for speaking so much, but I would like to stress in these quite different questions, to take part in some work. If I ask you even to prepare a doctoral seminar, I would understand, because to prepare a doctoral seminar takes, but to say two hours a week is a seminar, something would enter. It is no burden. So I don't understand this. An obligation of senior people is to give these young people this possibility.

DR. CLARK: We thank Professor Ginzburg.

(Much applause!)