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Institute for Technology and Strategic Research

THE FIFTIETH ANNIVERSARY OF THE FIRST PUBLIC ANNOUNCEMENT OF THE SUCCESSFUL TEST OF FISSION

PROCEEDINGS

January 17, 1989

The School of Engineering and Applied Science

The George Washington University
WASHINGTON DC

MASTER

P R O C E E D I N G S

CONVOCATION

TO

COMMEMORATE

THE FIFTIETH ANNIVERSARY OF THE
FIRST PUBLIC ANNOUNCEMENT
OF THE
SUCCESSFUL TEST OF FISSION

Co-sponsored by

Institute for Technology and Strategic Research
School of Engineering and Applied Science
The George Washington University

and

The Carnegie Institution of Washington

Tuesday, January 17, 1989
8:30 a.m. to 2:30 p.m.

The Cloyd Heck Marvin Center
The George Washington University
800 Twenty-first Street, N.W.
Washington, D. C.

These proceedings are the result of a convocation to commemorate the historic occasion of th announcement of atomic fission. The first public announcement was made by Neils Bohr attendees at the fifth Washington Conference on Theoretical Physics that was help on the campu of The George Washington University by the Physical Society on January 26, 1939. This meetir is unique because it announced the unlocking of energy of the atom which had been speculate about since the origins of scientific investigations and is now seen to be the dawn of the nucle age.

The convocation was sponsored jointly by the Institute for Technology and Strategic Research The George Washington University and the Carnegie Institution of Washington, and was mac possible by financial support from the U.S. Department of Energy, Kaman Sciences Corporatio the National Science Foundation and the Office of Naval Research--Contract #N00014-89-J-1072.

An exhibit was set up by the Smithsonian Institution of some of the objects in their physi collection relating to the discovery of fission. The exhibit contained artifacts such as a head Enrico Fermi; replica of Aston's mass spectrometer; replica of Chadwick's neutron chambe Geiger counter tubes from CP-1, cube of fuel from CP-1, embedded in transparent model reactor; neutron source used by Fermi in the 1930's; copy of a Strip-chart record of neutr activity of CP-1; sample of enriched uranium -235 produced at NRL; first sample of plutoniu -239; and Nier's mass spectrometer.

Copies of the Proceedings are available from:

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Video tape set--VHS--(two volumes) can be obtained through the Institute at a cost of $50.00, including postage. Requests should be sent to address above.

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PARTICIPANTS OF THE FIFTH WASHINGTON CONFERENCE ON THEORETICAL PHYSICS WHO ATTENDED THE FIFTIETH ANNIVERSARY CONVOCATION

CHARLES LOUIS CRITCHFIELD

LAWRENCE R. HAFSTAD

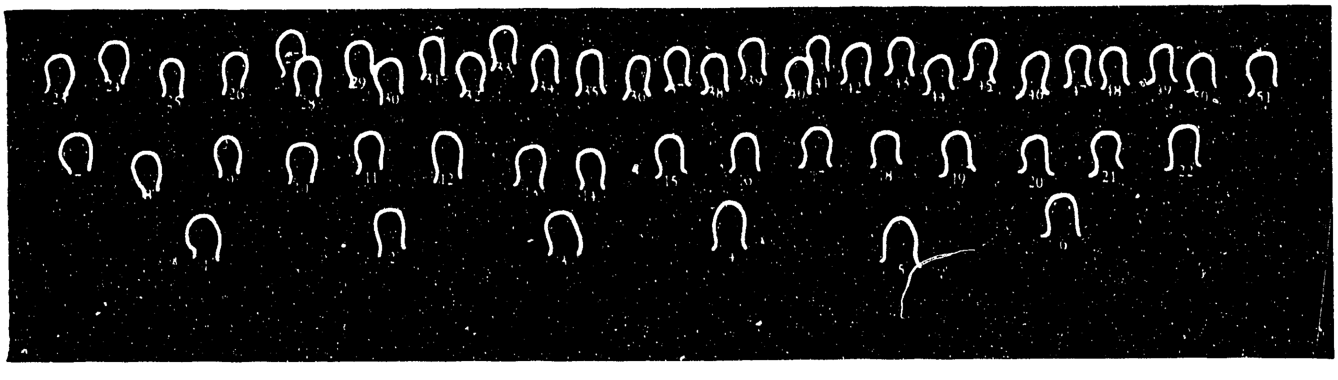
HAROLD JAMES HOGE

FREDERICK SEITZ

EDWARD TELLER

These were the participants in the fifth Washington Conference on Theoretical Physics, thrown into a turmoil when Niels Bohr (First row, fourth from left) interrupted the proceedings to announce that a month earlier, in Germany, the chemists Hahn and Strassman had split a uranium nucleus. The world was never the same again.

1 · Stern, Carnegie Tech, Pittsburgh
2 · Fermi, Rome, Columbia
3 · Fleming, Carnegie Institution of Washington
4 · Bohr, Copenhagen, Princeton
5 · London, Duke, Paris
6 · Urey, Columbia
7 · Brickwedde, National Bureau of Standards
8 · Breit, Wisconsin, Carnegie Institution
9 · Silsbee, National Bureau of Standards
10 · Rabi, Columbia
11 · Uhlenbeck, Columbia
12 · Gamow, George Washington
13 · Teller, George Washington
14 · Mrs. Mayer, Johns Hopkins
15 · Bitter, Massachusetts Institute of Technology
16 · Bethe, Cornell
17 · Grayson-Smith, Toronto
18 · Van Vleck, Harvard
19 · Jacobs, Massachusetts Institute of Technology
20 · Starr, Massachusetts Institute of Technology
21 · Hebb, Duke
22 · Squire, Pennsylvania
23 · Kuper U.S. Public Health Service
24 · Mahan, Georgetown
25 · Myers, Maryland
26 · Roberts, Carnegie Institution of Washington
27 · Critchfield, George Washington
28 · Baroff, U.S. Patent Office
29 · Bohr, Jr., Copenhagen
30 · Meyer, Carnegie Institution of Washington
31 · Herzfeld, Catholic University
32 · Lord, Johns Hopkins
33 · Inglis, Johns Hopkins
34 · Wulf, U.S. Department of Agriculture
35 · Wang, Peking, Carnegie Institution of Washington
36 · Johnson, Carnegie Institution of Washington
37 · Mohler, National Bureau of Standards
38 · Scott, National Bureau of Standards
39 · Vestine, Carnegie Institution of Washington
40 · Rosenfeld, Liege, Copenhagen, Princeton
41 · Seitz, Pennsylvania
42 · Diecke, Johns Hopkins
43 · Mayer, Johns Hopkins
44 · Hibben, Carnegie Institution of Washington
45 · Tuve, Carnegie Institution of Washington
46 · O'Bryan, Georgetown
47 · Hafstad, Carnegie Institution of Washington
48 · Cohen, Columbia
49 · Hoge, National Bureau of Standards
50 · Sklar, Catholic University
51 · Rossini, National Bureau of Standards



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FIFTIETH ANNIVERSARY OF THE FIRST PUBLIC ANNOUNCEMENT OF THE SUCCESSFUL TEST OF FISSION

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Dorothy Betts Marvin Theatre - First Floor

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		Maxine F. Singer, President, Carnegie Institution of Washington
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	Lecturer	Frederick Seitz, President Emeritus, The Rockefeller University *"Nuclear Science: Promises and Perceptions"*
11:45 a.m.	Introduction of Lecturer	Donald Gubser, Naval Research Laboratory
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Continental Room - Third Floor

1:00 p.m.	Luncheon	
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	Lecturer	Edward Teller, Honorary Director Institute for Technology and Strategic Research *"Toward a More Secure World"*

INTRODUCTION

The time is January 1939. We are at a Conference on Theoretical Physics being held in Washington, D.C., and sponsored jointly by the George Washington University and the Carnegie Institution of Washington.

No jet aircraft fly noisily overhead to disturb the speakers at this conference. And no speaker here mentions the application of physics to space exploration. Space exploration is yet but a wild dream of some of the science fiction writers of the time. A time, that is, when the Periodic Table consisted of 92 elements. The transuranium elements, important man-made elements beyond uranium (e.g. neptunium [93], plutonium [94], americium [95], curium [96], berkelium [97], californium [98], einsteinium [99], etc.) were undiscovered in 1939.

Yet, the pivotal events that took place during the 1939 conference and subsequent to it have forever changed our ideas about the world we all live in.

Of course, the physicists of the 1939 world were well aware of the investigations carried out by Lord Rutherford, Neils Bohr, Irene Joliot-Curie, Pavle Savic, Frederick Soddy, Enrico Fermi, Leo Szilard and others. All these people had attempted to unravel the mystery of nuclear structure and the origin of the nuclear species.

However, few scientists in January 1939 really believed that, in spite of the notable work of a few of their above-mentioned peers, the great amounts of energy stored in the atom could ever be harnessed and put to any practical use. Consequently, the attendees at the 1939 conference were amazed by the announcement made there by Niels Bohr. Bohr revealed that Otto Hahn, and Fritz Strassman of the Kaiser Wilhelm Institute in Germany, and Lise Meitner with Otto Frisch--both Austrian physicists in exile, Meitner in Sweden and Frisch in Denmark--had been successful in splitting the atom. He added that this discovery of a new kind of nuclear reaction, the fission reaction, had been confirmed both experimentally and theoretically.

Bohr's announcement immediately stimulated work on fission in many laboratories in the United States, where the discovery was further confirmed by February 1939. And, by March the pioneering research of Hahn, Strassman, Frisch and Meitner was publicized widely in scientific journals. This triggered the publication of a number of monographs on fission. In these papers the possibility of a nuclear chain reaction was discussed and it was theorized that the fission-produced energy could be used to cause additional fission.

These background papers set the stage for the discovery of the first transuranium element with the actual discovery resulting, in turn, from experiments aimed at understanding the fission process. Suffice to say here that over 200 of these transuranium elements, each of which has a known number of isotopes, all radioactive, have been discovered since 1939. Of course, plutonium--a transuranium element--found its first use in the manufacture of nuclear weapons. However, the more potentially beneficial practical use of plutonium-239 is as a nuclear fuel to generate heat which is converted to electrical energy. Other transuranium elements have demonstrated a wide range of practical applications for space exploration, medical research and the non-destructive testing and evaluation of materials, among other practical uses.

The rest of this story is history, but what a history! It deals with the fifty year period of time 1939 through 1989, which is but a mere moment on the time scale of man's quest for a rational understanding of his world. In such a historic "moment" we have witnessed a tremendous number of changes in the way we live due to the scientific and technological discoveries that have taken place during the past fifty years. Thus, we owe a debt to the 1939 conference that in no small way was directly responsible for such changes. It is in this spirit that this same conference is honored and celebrated by the 1989 symposium the proceedings of which are documented herein.

Don Groves
Research Fellow
Institute for Technology and
Strategic Research

WELCOME

Dr. Sam Rothman

Good morning. I am with the Institute for Technology and Strategic Research, and I would like to welcome you to this Commemoration of the fiftieth anniversary of the first public announcement of the successful test of fission.

We at The George Washington University are very proud to host this affair. As you know, the announcement occurred on January 26, 1939, fifty years later the world has changed appreciably, and we can see the impact upon international politics, diplomacy, science and other factors as a result of that particular test back in 1939.

We wish to extend a particular welcome to those people considered "founding fathers," who were here at the particular meeting in January 1939. Several of them are with us here today and, hopefully, you will have an opportunity to speak with them during the break, at lunch, or even perhaps after lunch. They will be with us for most of the day.

I now wish to present the Director of the Institute and Dean of the School of Engineering and Applied Science, Dr. Harold Liebowitz.

DR. LIEBOWITZ.

REMARKS AND INTRODUCTION OF
DR. MAXINE F. SINGER
PRESIDENT OF CARNEGIE INSTITUTION OF WASHINGTON

Dr. Harold Liebowitz

Thank you, Professor Rothman for starting the meeting and, also, being on the Organizing Committee for this eventful conference day. On behalf of the Institute for Technology and Strategic Research, I would like to greet you all on this eventful occasion.

The Institute was founded two and one half years ago by Professor Teller and myself. We were attending the eightieth birthday of another Hungarian, and we had begun to speak about how such an institute should take form. This Institute was formed to ensure that the University has strong inputs to our strategic research.

We believe that this Institute is unique, in that it has a very strong engineering and scientific input, as well as the softer sciences. Consequently, it has been organized with the cooperation of the other schools and colleges of the University, including the Elliott School of International Affairs, the Graduate School of Arts and Sciences, Columbian College, and the National Law Center. Certainly, when areas come up which are of interest to other schools, such as the School of Government and Business Administration and the School of Education and Human Development, the faculty and students of these division of the University also are available to participate in this Institute.

We are proud of the Institute because many academic institutions today are shying away from military research and nuclear work. We think it is only fitting for a university to offer the pros and cons, and to offer the platform that is necessary for unbiased evaluation and programs. And so, we have organized this Institute in such a way that we can undertake such subjects as nuclear energy: unpopular in the United States and the United Kingdom and a few other places but, on the other hand, going ahead in places like Japan and France. The Institute recently had a meeting on ultra-safe nuclear reactors, and results should be forthcoming soon. One of the conclusions reached at this meeting was that we should be looking to smaller nuclear reactors. I think that we can overcome many of the problems which exist today--cost overruns, and many engineering problems--which have turned people and including corporations away from investing in nuclear energy.

I am pleased that The George Washington University was the place where Niels Bohr made the announcement of his first successful test on fission. Professor Teller tells me that on that evening, it was announced that the Carnegie Institution was where that evening they carried out tests to confirm Niels Bohr's claims. Professor Teller says that George Gamow had some doubts about it, but was able to be convinced. You will hear much more about all of that from the "founding fathers"--or, as Professor Teller says, what he likes to think of it as "the survivors."

Those who are here today, and those who could not make it but who were here fifty years ago, truly are founding fathers: people like Fred Seitz, and Professors Teller and Gamow, and Herzfeld from Catholic University. If you have not seen the photographs of the people who were present at that time, there are 51 in the pictures.

I think it is important at an event like this to look back and think of these outstanding announcements, and how the world has come to look. That is why I made the few comments before about nuclear energy. I think that we have to put things in proper perspective, and I would say that we are glad that the scientific discovery did take place--it has changed the world, and the people who have been associated with that meeting have dedicated themselves to peace in this world. If I can get agreement from all of you that you will be around at the next meeting at the 100th anniversary, we will hold that meeting here also.

I would like to take this opportunity to thank Professor Müller, who has come from Zurich, and for being a part of the program. When I visited him and told him about this, he graciously consented to be part of this celebration, and I would like to publicly thank him.

It is a pleasure for me to introduce the President of the Carnegie Institution of Washington, Dr. Maxine Frank Singer. She has been a "doer." She has received high honors throughout her career: graduating Phi Beta Kappa from Swarthmore in 1952, and receiving her doctorate from Yale University in 1957. She was for a number of years at the National Institutes of Health, National Cancer Institute, and participated in a program here to present some of the latest updates in science to congressmen, and the implications that they should be aware of in carrying out their duties and responsibilities in the Congress. She became President of the Carnegie Institution of Washington in 1988, and also is presently scientist emeritus in the laboratory of biochemistry at the National Cancer Division of the National Institutes of Health.

She has been active on editorial advisory boards, and has received honorary degrees from a number of universities. She has been recognized by the Association for Women in Science with its award for contributions to science, and received the Director's Award of the National Institutes of Health. She has been elected to the American Academy of Arts and Sciences, the Institute of Medicine, the National Academy of Sciences, and received the Distinguished Service Award from the Department of Health and Human Services. Also, I notice that she received the Katharine D. McCormick Distinguished Award from Stanford University in 1983, and the Distinguished Presidential Rank Award in 1988.

These are only typical of the kinds of recognition that Dr. Singer has received. It is with great pleasure that I call upon her to address the group.

PRESIDENT'S MESSAGE

DR. MAXINE F. SINGER, PRESIDENT
CARNEGIE INSTITUTION OF WASHINGTON

Professor Liebowitz and honored guests, it is an honor and a privilege to welcome everyone here today in the name of the Carnegie Institution of Washington. We have come together to commemorate one of the great scientific events of a century that has been marked by profound discoveries about the nature of the universe and its components.

This event, the demonstration and confirmation of nuclear fission, was especially remarkable because of its significance for all aspects of life on our planet. It was also notable for the context in which it occurred. In January 1939, some of the world's nations had already felt the bitterness of war; and the rest were on the edge of a conflagration that would consume the whole planet. The fundamentally evil nature of the aggressor had already denied--and would continue to deny--universal tenets concerning the dignity and value of human life. Displaced people from all societal niches had already begun to wander the world in search of freedom and opportunity. Among these were many gifted physicists who found that freedom and opportunity in the United States. In spite of the circumstances, they, along with American colleagues and others from the then-seething European continent, came together here in Washington. They came not expecting drama, but to engage in that most essential of scientific activities, communication.

Thus, besides all the more obvious reasons we have to commemorate the 1939 Conference on Theoretical Physics, our celebration should underscore the essentiality to science of open, international scientific discourse.

Today, the scientific work of the Carnegie Institution and of its Department of Terrestrial

Magnetism--DTM, as it is known to all--some five miles north of us in the District of Columbia, is far removed from the nuclear physics that Merle Tuve and his colleagues concentrated on in the 1930s. But we remain deeply aware of this marvelous chapter in our history, a chapter which exemplifies in many respects the nature of the Carnegie Institution.

In founding the Institution in 1902, Andrew Carnegie had the then visionary goal of an Institution for discovery. His idea was that the Institution should seek out the exceptional individual and provide that person the means for engaging in unfettered research "for the improvement of mankind." Since that time, successive Boards of Trustees have been faithful to Carnegie's idea, and thus the focus of research in our departments has changed or remained the same, depending on the interests of the scientists.

For example, bold and ambitious programs in developmental and plant biology, in astronomy and earth sciences, continue; while the genetics laboratory at Cold Spring Harbor and the archeological excavations at Chichen Itza on the Yucatan Peninsula were turned over to others.

DTM was established in 1904, soon after the Institution itself. Its earliest program was to measure and study the earth's magnetism. Land surveys were conducted, as were ocean surveys; the latter by the *Carnegie*, a nonmagnetic sailing vessel whose global voyages ended in 1929 when the vessel accidentally burned in the South Pacific. By then, DTM's interests had already broadened. Gregory Breit, working with the young Merle Tuve, had demonstrated the ionosphere in 1925 using radiowave echoes. Tuve was the driving force behind the Department's growing interest in subatomic particles. He and the group he assembled built and enhanced a succession of instruments for accelerating particles, including a Van de Graaff generator in 1932 and an improved 5-million volt Van de Graaff machine in 1938.

As is still the case, funds for such undertakings were appropriated from the Institution's resources. Tuve had to convince the then President, John Merriam, of the soundness and potential of the newly proposed instrument. But the bias of the Institution was, and remains today, sensitive to the importance of instrument development to scientific progress. For example, one of our primary current concerns is planning the construction of a new optical telescope with an 8-meter mirror for our Las Campanas Observatory in northern Chile.

Its ability to bombard materials with high-energy beams of electrons and protons put the group at DTM at the forefront of atomic nuclear physics in the United States in the 1930s. Two reports published in 1935 stand firm today. They demonstrated that the nuclear force acting between two protons is identical to that between a neutron and a proton. Tuve, with colleagues Lawrence Hafstad and Norman Heydenburg, described the difficult but superbly executed experiment. Breit, who by then had left DTM, described the theoretical analysis with coauthors Condon and Present. These reports were the culmination of the goal set by Tuve and Breit a decade earlier: to observe the scattering of protons by protons in order to disclose the nuclear component of the force between the two particles. I am told by physicists that the two papers exemplify the extraordinary aesthetic beauty that can characterize great scientific achievement. And the scientific leadership inherent in these accomplishments made DTM and the Carnegie Institution the proper co-sponsors, with The George Washington University, of the annual Theoretical Physics Conference here in Washington.

The events leading up to the exciting days of the Fifth Annual 1939 Conference are well known, and are perhaps most wonderfully told in Richard Rhodes's book, *The Making of the Atomic Bomb*. Two observations about the story occur to me, a biologist. One is what little time elapsed between Hahn and Strassman's demonstration that barium isotopes are produced upon neutron bombardment of uranium, Meitner and Frisch's working out of the theory, and the demonstrations in Copenhagen, at DTM, at Johns Hopkins, and at Columbia of the co-production of nuclei of the expected energies. The pace of international communication and experimentation, both of which were needed for these events to take place within a few short months, has only more recently become familiar to biologists. Our prejudices would have suggested that such rapid communication depends on contemporary aids, like fax machines and BITNET.

The second observation is to note that the name "fission" was directly derived from the biological term used to describe the division of a bacterial cell into two cells. It was suggested to Otto Frisch by an American biologist working in Copenhagen, William A. Arnold.

For DTM, the ability to demonstrate the fission-associated energy release depended on Tuve's foresight in building the large Van de Graaff generator; on Merriam's support; and on the skill, energies, and enthusiasm of Richard Roberts, Lawrence Hafstad, and Robert Meyer.

But of course, such intersections are not accidental. They are rooted in the recognition that science is unpredictable and one must, thus, be prepared; and in the precept that guides the Carnegie Institution: to "encourage in the broadest and most liberal manner, investigation, research, and discovery, and the application of knowledge to the improvement of mankind." They are rooted in a commitment to the individual scientist and his or her imagination, taste, and capacity for originality.

It is in this framework that we can measure the continuing contributions of DTM and the Carnegie Institution. Roberts and Meyer were quick to demonstrate the emission of delayed neutrons from fission, suggesting the possibility of a chain reaction. Later, Merle Tuve would use his vast energy and talent and DTM's resources to initiate development of the proximity fuse that played such a critical role in the Allied countries' war efforts. And Vannevar Bush, who became the Institution's President in 1939, was responsible for convincing President Roosevelt of the importance of science to the nation's effective participation in the war. Bush organized the National Defense Research Committee, and recruited and coordinated the nation's leading scientists in the war effort. In so doing, he laid the groundwork for what has become a truism: that the scientific community must bring its expertise to national policy debates and decisions in peace and war alike.

After the war, the DTM staff shifted focus, realizing that original work in nuclear physics would require large groups, costly accelerators, and work on secret projects. They chose instead to engage in research more consistent with the Carnegie spirit. One group, led by Phil Abelson and Richard Roberts,. made extraordinary contributions in biology. Among other things, they laid the groundwork for techniques that are the basis for much of contemporary biology. Under Merle Tuve, Carnegie staff developed the image intensifier tubes for use with optical telescopes, and pioneered new approaches to seismology, an effort that continues to this day.

These and current activities at DTM are part of our Institution's heritage, a heritage that is enriched by our participation in the great events of January 1939. At the same time, they continue to move us forward, as is in the nature of science. The atmosphere in the Department continues to be free-wheeling and iconoclastic, epitomized in its famous 40-year Institution, the daily lunch club, where staff members alternate in preparing food for the stomach and food for thought for their colleagues.

The usual peace of the DTM hilltop is about to be disrupted for a while. We break ground within weeks for a new, modern building. Although the 80-year old original building is solid and useful, the expansion will permit the staff of the Geophysical Laboratory, now a few miles south, to join the DTM staff at Broad Branch Road. In this way the trustees hope to encourage the rich scientific rewards that can come from closer collaboration and communication.

I hope that I have convinced you of the continuing dedication of the Carnegie Institution to science--and to the way of doing science--that permitted the Institution to contribute so much to the events fifty years ago that we are here to celebrate. It is in the nature of science to look back, even while moving ahead. I am very grateful for the opportunity to look back with you as we begin this fine day.

Thank you.

INTRODUCTION OF
STEPHEN JOEL TRACHTENBERG, PRESIDENT
THE GEORGE WASHINGTON UNIVERSITY

Dr. Harold Liebowitz

Thank you very much, Dr. Singer, for a very scholarly introduction to this meeting. Not only do we wish to look back fifty years, but we certainly want to look ahead: not only at The George Washington University but, also, on a much more broad scale and in a global picture. The University has recently appointed Stephen Joel Tractenberg the 15th President of the University, and he started here August 1, 1988. The Board of Trustees has been very concerned with how the University will grow in strength and in a global role, given its Washington location.

The new President comes to George Washington University at a time when two tough problems face institutions of higher learning: tuition costs and minority access to post-secondary education. We are not only experiencing a decline in the numbers of students interested in science and engineering but, also, demographic changes as well. Whereas we used to draw primarily upon the pool of prospective students from the Northeast U.S., we will be drawing upon more Blacks and Hispanics in the future; and more heavily from other regions of the country. There is certainly great concern for ensuring a proper education for all, and I am sure that President Tractenberg will have a few comments on that.

Stephen Joel Trachtenberg has attended three Ivy League schools: Columbia University; Yale University, where he received his law degree in 1962 and then his Master's degree of Public Administration in 1966; and Harvard University. He is no stranger to the atomic energy picture, as from 1962 to 1965, he was an attorney with the New York Office of the Atomic Energy Commission. He has expressed a longstanding interest in the affairs of the Institute and in the subject of our meeting today.

He was a legislative assistant on the Hill, and he was also a tutor in law and a teaching fellow at Harvard University. From 1966 to 1968, he was Special Assistant to the United States Education Commissioner in the U. S. Office of Education in Washington.

He has received many awards and has been very active. We expect to see many changes here.

It is a great pleasure to call upon President Trachtenberg to address you.

PRESIDENT'S MESSAGE

Stephen Joel Trachtenberg, President
The George Washington University

Thank you very much, Dean Liebowitz. First of all, my remarks are brief and mostly in the nature of an extension of hospitality, and to say how delighted I am to have you all here on the campus at The George Washington University for this very exciting occasion. I do want to associate myself with Dr. Singer's remarks, which I thought were thoughtful and absolutely right for the occasion.

As she was speaking, a thought flitted through my mind; actually a fugitive reflection from my youth. My father was born in the Ukraine and grew up in a small town, and when I was a boy he used to amuse me by telling me stories about growing up in "the old country." One story that I found particularly interesting had to do with him and his best friend, Sasha, who heard that in a neighboring village there was something called an automobile, and the two of them proceeded to walk the 10 miles between where he lived and the next community to see this modern miracle. Years later, my father, sitting in front of a television set in Brooklyn, New York, watching a man walk on the moon, expressed great skepticism about the event and said, "You know, they can show you anything they want on television. It is probably not really happening."

I persuaded him that in fact, there was a good likelihood that it was happening because, I said, "Listen, if you can believe in television, you ought to be able to believe in space travel." He ultimately made the leap of faith. At the time, I was working for the Atomic Energy Commission, and because of the fact that the work we were doing was classified, I wasn't able to tell him that we were working on atomic powered space travel. Indeed, it strikes me as having come full circle to note that in my role here this morning I am serving, in a manner of speaking, as an agent of Glenn Seaborg who was the

Chairman of the Atomic Energy Commission at the time. I also find it fascinating that this is the 50th anniversary of nuclear fission. I myself just celebrated my 50th birthday. I draw no cause and effect from these two anniversaries, but I do have a sense of the remarkable and extraordinary things that can happen in one person's lifetime--and I like to think I have got a little room yet to go.

I hope that The George Washington University will continue to play a significant, indeed, seminal role in the initiatives that you will be devoting yourselves to today and further.

Additionally, I want to pick up on something that Dean Liebowitz talked about: namely, the social agenda to which The George Washington University is committed. Those of you who saw this morning's *Washington Post* may have read an article that reported on a talk I gave last night as part of the Martin Luther King Day ceremonies. Yesterday I announced that The George Washington University will be devoting approximately 7 million dollars to a new scholarship program, a scholarship program that ornaments our current commitment to minority youngsters from the District of Columbia. We anticipate that with this new initiative, which we are calling the "Twenty-first Century Scholars Program," we will draw the most outstanding youngsters from the District of Columbia's public schools to The George Washington University each year for the next decade, right up to the next millennium. I hope that a significant number of those young people will consider careers in the sciences, in physics, in chemistry, and in engineering, because I think it is imperative that we have new leadership in these disciplines that comes from all the racial and gender groups of which our society is composed. The George Washington University hopes to be a leader in that manner, as well as in research and in teaching.

I am pleased, on behalf of the institution to see you all here today, and I look forward to working with you all in the future.

Dr. Singer and Dean Liebowitz thank you very much for your remarks.

INTRODUCTION OF DR. FREDERICK SEITZ
PRESIDENT EMERITUS, ROCKEFELLER UNIVERSITY

Dr. William R. Graham

Most of us, at least of my generation, came to know Fred Seitz not through that conference, but through his books. Certainly, the *Modern Theory of Solids*, which he first published in 1940, the *Physics of Metals*, published in 1943, and *Solid State Physics*, released in 1955, were in fact the path to a whole area of science which has revolutionized the world itself. Few scientists or others are involved in even one revolution. Fred Seitz has been involved in several.

In more recent times he has received the National Medal of Science, the Vannevar Bush Award of the National Science Board, and many other national and international scientific awards. In the intervening years, he was the Executive President of the National Academy of Sciences from 1962 for the next 7 years; President of Rockefeller University for over a decade; and a member of many advisory and leading boards and committees in this country, which have helped shape the course of our freedom and our democracy.

Physics is not the course of logical deduction and consistent experiment that one might suspect if one read only the carefully crafted and logically deduced expositions that we present to the students of the subject. It is presented that way to help them understand it, not to trace the course of the history of physics. In fact, physics has really been a struggle between the conventional wisdom of the field on the one hand, and unique views which at first, at least, have tended to be largely rejected by conventional wisdom and the bearers of the conventional wisdom.

Galileo, for example, conducted what seems to us today to be a simple experiment, determining the acceleration of different weights in the same gravitational field. But he challenged over 1,000 years of conventional wisdom when he did that. That makes you wonder sometimes what people were doing in that intervening 1,000 years, until Galileo came to challenge the conventional wisdom of the age.

The history of physics is legion with people who have challenged that wisdom: Copernicus, Einstein, Bohr, Schrödinger, and on and on. Sometimes even they had trouble assimilating what it was they had discovered.

The same can be said of the applications of science and physics, and this is a field in which Dr. Seitz also has been deeply involved. It will be regarded, I think, as one of the great mysteries of this era by historians to come after us that today we depend for our national security and our own safety solely upon the ability to destroy others; and not only is that an expediency that we have had to put in place up to now, but this has been taken by many as the conventional wisdom of the subject--the right and proper approach to our national security.

Therefore, the dialogue in some quarters has come to being that the cause of peace is served only by our ability to destroy others; and our ability to protect ourselves against such destruction is viewed by some as a hostile act. Nothing could be further from the truth, and no one has served more strongly to rectify this misconception than Dr. Seitz.

Dr. Seitz was the President of the American Physical Society in 1960. In 1987, when that same Society, under new leadership, released a report on devices, largely beam devices to protect us from ballistic missile attack, Dr. Seitz challenged it. The report was, in my view, full of technical errors and, in fact, even internal inconsistencies. There was a debate on the subject. Unfortunately, as is the characteristic of some of the worst traditions of physics, the rebuttal to the paper itself was rejected for publication by the same journal--it happened to be a journal of the physics society which published the challenge to these beam devices. Dr. Seitz took great exception to that. And then when the Board of the Physical Society put out a much more sweeping condemnation of the usefulness and technical feasibility of our defense of ourselves, Dr. Seitz took great exception to it and publicly and before Congress challenged it in very strong terms and, in my view, very correctly and with great effect.

Today Dr. Seitz is Chairman of the Board of the George C. Marshall Institute. He is carrying on in the tradition of the leaders in science of the past, and what will certainly be the tradition of leaders in the future. He is exceptional as a man of ability, of vision, integrity and that rarest property of all, courage.

It is a pleasure to introduce Dr. Frederick Seitz.

LECTURE

NUCLEAR SCIENCE: PROMISES AND PERCEPTIONS

Dr. Frederick Seitz

The discovery of radioactivity at the turn of the last century and the discovery of fission at the end of the 1930s was accompanied in both cases by the emergence of socio-political activism on the part of a number of prescient individuals.

Some of the response was positive as well as prescient in the sense that individuals saw on the horizon the potential for an unlimited source of energy for the extension of industrial civilization on a world-wide basis. Others were concerned mainly with negative aspects. Indeed, some individuals have, for their own reasons, sought to generate public fear without offering any compensating form of balance or enlightenment. This bifurcation of outlook is not new in human history. The age of steam, the rise of chemical technology, the dawn of the air and space ages and the great discoveries in the field of molecular biology have generated similarly divided emotions and activities. Every emerging technology generates a mixture of hope and fear, to paraphrase the title of Alice K. Smith's remarkable book on the history of activism within the scientific community in the period immediately following the events of 1945.

Much of the history of this development is contained in Spencer Weart's book *Nuclear Fear,* but I shall add some personal touches. In particular, I would like to examine aspects of this activism by focusing on the roles played by two excellent scientists who were prescient and motivated primarily on the basis of humanitarian concerns.

The first is Frederick Soddy, whose life extended from 1877 to 1956. He is scarcely remembered at the present time although he was clearly the first person to appreciate the long-range potentialities associated with the discovery of transmutation of the elements. The second is Leo Szilard, the brilliant Hungarian-born physicist, who appeared in the world in 1898 and died in 1964. He was, of course, well-known to many in this room. Before discussing the work of the two, however, let me digress for a moment.

Carl Jung, the Swiss psychiatrist, was an early associate of Sigmund Freud but eventually broke with him and established his own school and pattern of work. When studying the behavioral patterns of children and older people who were well along in their careers, Jung discovered that the psychic tensions which could be traced to their subconscious did not conform in most cases to matters related to sexual activities so strongly emphasized by Freud. Instead, they required much more general analysis and, when needed, therapy. Among other things, Jung found when studying the drawings of young children that there are inborn symbols, concepts, and designs which must be of genetic origin and which go back to experiences in the early history of our species. Perhaps the most celebrated of these is the concept of the mandala, the design consisting of a circle which may have several forms of subsidiary decoration, and which children often draw spontaneously without appreciating the source of their inspiration. It is prominent in many forms of religious art.

To come nearer to home in the areas of the physical science, we can apparently recognize two concepts that seem to be embedded in the human subconscious and which surface in one form or another in each generation among individuals who are appropriately stimulated. One is the concept of the frequent visitation of beings from a different world, terrestrial or otherwise, who permit themselves to be seen only occasionally. Beings such as leprechauns and trolls held the field in the past. In our own time this phenomenon appears in the form of observations of unidentified foreign objects, or UFOs. There was a rash of claims of such visitations in the 1950s and 1960s and a new one has just emerged. The other concept is related to the possibility of the development of an agent or instrument that can threaten the survival of a large part or all of humanity and can lead either to our destruction or our salvation. This concept appears in various forms throughout recorded history. In the last century it emerged in the literature in the form of weapons made possible by discoveries which take place at the frontiers of science. It is, for example, a theme found in several forms in the clairvoyant books of Jules Verne--an author with whom Soddy was quite familiar.

I first encountered this form of psychic revelation at a relatively primitive level when I was serving in a technical intelligence office in General Eisenhower's headquarters in Europe near the end of World War II. The office was headed by H. P. Robertson, the expert on relativity theory who had been at Princeton University in the 1930s. A German private soldier who obviously suffered from a severe psychic disturbance came to the attention of our military staff. He was interviewed and a detailed report was sent to our office. He believed that he had witnessed, under unlikely circumstances, the test of a bomb by his own generals and scientists that was many orders of magnitude larger than anything then available anywhere, and which destroyed an area of many square kilometers. He described the incident in great detail. This was well before the July 16 test at Almagordo.

Let me pass to my main subject. In 1896, Henri Becquerel discovered by accident that a specimen of uranium-containing mineral in his laboratory emitted a penetrating radiation which could expose photographic plates in sealed packages. This was followed soon after by the isolation of radium--a minor constituent in pitchblende ore--which on a weight basis was an even more intense emitter of similar radiation. The nature and ultimate origin of the radiations was completely unknown, and became the object of a great deal of speculation. Coming as they did at the same time as the experimental isolation of the electron as a constituent of the atom and the discovery of x-rays, the new disclosures added immensely to the excitement current in the field of physics as it entered into a turbulent, revolutionary period.

By the summer of 1903, Ernest Rutherford and Frederick Soddy, working in close collaboration at McGill University in Montreal, had succeeded in demonstrating beyond serious doubt that the phenomenon of radioactivity --the term used to designate the effects found in uranium radium and a few other heavy elements, including thorium-- was intrinsic to the atoms of the species which exhibited it. Soon thereafter, in 1905, Egon von Schweidler, a colleague of Ludwig Boltzmann, introduced the present-day concept of the statistical nature of the disintegration process.

Incidentally, the somewhat accidental partnership of Rutherford and Soddy was a most remarkable happenstance. Rutherford, a few years older than Soddy, was a reasonably well-established experimental physicist, and Soddy was a newly graduated physical chemist with a strong interest in the history of chemistry. The experience of both was essential to the discovery that radioactivity arose from atomic disintegration.

There are two important social issues associated with these developments which I would like to mention. The first is related to professional attitudes. One might have supposed that the older, well-established chemists would have been especially intrigued by the discovery of radioactivity and plunged into the field. The opposite seems to have been the case. It is almost as if they hoped that the observations would go away. One suspects the issue was partly psychological in the sense that they were involved in other successful work; and partly related to the fact that, as well-established chemists, they saw a threat to the hard-won concepts of the immutability of matter. Fortunately, the field did prove creatively exciting to some of the younger chemists. Two young chemists deserve special mention.

First there is Frederick Soddy, whom I mentioned above. He arrived at McGill University from England in 1900 at the age of 23 as a very junior faculty member. He became fascinated with Rutherford's primitive attempts to unravel the fundamentals of radioactivity and joined him as a fully dedicated partner. Rutherford, then 29, had arrived at McGill two years earlier and had begun focusing on the observations related to the radioactivity of thorium. Incidentally, Rutherford openly stated in later years that the cooperation of Soddy was essential to the success of the early work. Soddy did not share in Rutherford's Nobel prize, but was rewarded later for his contribution to the discovery of isotopes, chemically identical atoms with different mass.

The other notable young chemist who decided to devote his career to the field of radioactivity was Otto

Hahn who, after completing his doctoral work in Munich in 1901 at the age of 22, first joined William Ramsey, the discoverer of helium, in London and then joined Rutherford at McGill to become one of the world's foremost radiochemists. His first important work carried out in Rutherford's laboratory was to refine in an essential way the analysis of the thorium chain of reactivity made by Soddy. His great contribution to the field, with Strassmann nearly forty years later, was, of course, to make sense out of Fermi's very rudimentary and only partially correct analysis of the effects of irradiating uranium with neutrons. Fermi correctly predicted the production of transuranic elements, but missed the fission process and several other consequences of the irradiation. Hahn reported by letter to Lise Meitner the phenomenon he termed the "bursting" of uranium. She discussed it with Otto Frisch, who then reported it to Niels Bohr, who in turn brought the news here in 1939 and stirred the community of nuclear physicists--not least those in the United States, as never before. Doubtless his early work on the natural radioactive elements stimulated him to focus on the confusing results that emerged out of Fermi's laboratory from their quick run through the periodic table. He also states in his biography that Aristide von Grosse had urged him to explore the matter further.

The second important social consequence of the early years of radioactivity relates to the effect that his work with Rutherford had upon the ultimate career of Frederick Soddy, for it led him into messianic pathways and a search for a peaceful world through scientific approaches to economics and sociology. In great contrast, of course, Ernest Rutherford regarded radioactivity as primarily a laboratory phenomenon, having few auxiliary applications. As late as the 1930s, he made public statements to the effect that any talk of producing significantly useful energy from the nucleus on the basis of what was known then as "moonshine."

In his book *Nuclear Fears,* Spencer Weart expresses the belief that well before his association with Rutherford, Soddy developed strongly the view that chemistry would not only make enormous advances in the future, but would have the effect of liberating humanity from drudgery and provide it with unlimited capabilities, including access to essentially free power to drive the machines of the world. With this background, it was easy for him, once atomic disintegration--with its relatively enormous release of energy on the atomic scale--had been revealed, to assume that in some way, through the advancement of science, mankind would learn to gain access to such energy. He in effect became the first scientist apostle of the nuclear age. He turned his attention to what he regarded as scientific approaches to economics and sociology and became a wide-ranging public lecturer, who attracted large and often very distinguished audiences. No doubt his earlier association with Rutherford, whose fame was growing continuously, played an important role in the reception he received. One of the individuals who took his predictions very seriously was H. G. Wells who, as early as 1913, before World War I, foresaw the potential dangers associated with the development of what we now term nuclear weapons; and wrote a book with the title *The World Set Free*, in which among other more desirable things such weapons are used and cause great destruction.

It is noteworthy that Wells' book appeared before the start of World War I and at a time when the products of advances in science and technology were generally regarded to be overwhelmingly beneficial to mankind. Soddy seems to have retained this optimism until the horrors of that war descended upon Europe. He was particularly shaken by the death at Gallopoli of the brilliant young physicist Moseley, who had demonstrated the relation between the frequencies associated with x-ray spectra and atomic number. After that he became a pessimist concerning the effects of the release of nuclear energy on a practical scale, which he continued to believe was imminent.

Interestingly enough, his biographer, Muriel Howorth, makes no mention whatever of his reaction to the news of the discovery of fission or to the successful development of the nuclear bombs during World War II and in the postwar period. Soddy was in his sixties at that time and still very active. One can only presume that he had become mentally adjusted to these developments long before they occurred.

It is clear that the well-established scientists in Soddy's generation felt that he had wandered far afield and lost touch with his own community. One item in a biography extols his early contribution to the unraveling of the mysteries of radioactivity, but states that he lost his genius in later years.

The next major scientific figure to give warning that the nuclear age was imminent, for better or worse, was Leo Szilard; who became interested in the scientific, technological, and social aspects of the subject in the mid-1930s and never left it until his death in 1964. He undoubtedly was influenced in part by Chadwick's discovery of the neutron in 1932 and by his friend Eugene Wigner's early involvement in the theory of nuclear structures, but his mind soon soared far above all of this. We apparently know of no direct connection between him and Soddy; but having moved to Oxford in 1934 when Soddy was still in his prime, he could not help but know of Soddy's predictions and anxieties. We also know that he was strongly affected by H. G. Wells' book, *The World Set Free,* which he first read in German translation.

In any event, Szilard became concerned with the possibility of a neutron-induced chain reaction using one neutron-rich nucleus or another some five years before fission was actually discovered. I first met him through Eugene Wigner in 1935 or 1936, during one of his visits to the United States. His conversation focused almost entirely on two themes: the dangers Hitler posed to the free world, and the imminent feasibility of releasing nuclear energy if the right steps were taken. A year or so later, when I was working at the General Electric Research Laboratory in Schenectady, he was doing his best to convince the head of the laboratory, Dr. William Coolidge, to undertake experiments under Szilard's direction. As far as I can recall, he focused at that time on using the neutron rich nuclei of the alkali metals in a highly compressed state as the source of energy to be derived from a neutron-induced chain reaction. In retrospect this was a very dubious proposal for an earthbound experiment, although his enthusiasm was unquestioned. As might be expected, he focused on the positive aspects of achieving access to nuclear energy. He eventually became discouraged with this approach but was immediately on hand as a leader when fission was discovered a short time later.

There is, of course, no doubt that Szilard played a major catalytic role both in England and the

United States in pressing for work on a chain reaction. This became an all-consuming activity, as we all know.

It was my privilege to spend two years at the University of Chicago with Szilard during World War II. My wife was serving as a substitute college faculty member in Pittsburgh at the time, so I was virtually a bachelor in Chicago and spent many evenings with Szilard at the Quadrangle Club. At that time, his mind reverberated continuously between the potential benefits and hazards of the nuclear age, tempered always by a day-to-day interest in the course of the war and speculations on the progress being made by a hypothetical German version of the Manhattan Project.

Once it became clear in the summer of 1945 that functioning bombs existed and that the United States alone possessed them, Szilard essentially lost interest in peaceful uses of atomic energy except in a very peripheral way, and focused all his attention on the matter of control. Apparently, he had first accepted the thesis that our country was controlled by a few wealthy families and business people, for the most part located in New York City; a theme which President Roosevelt used in the 1930s during some of his campaign speeches. On reading items in the press regarding Beardsley Ruml, a prominent financier on Wall Street, he went to New York, introduced himself to Ruml and arranged a meeting with a number of New York businessmen and investors; urging them to take steps to make certain that future control of nuclear bombs and their development be kept in safe, civilian hands. His listeners heard him out with rapt attention, since many of the things which he said were new and of great interest to them. When he concluded his presentation, however, they stressed the fact that the country was run from Washington and not from New York, and offered to introduce him to influential senators and congressmen. He essentially took the next train to Washington and quickly gathered together a very effective lobbying group consisting of scientists and others sympathetic to his mission.

His great success was, of course, the defeat of the May-Johnson Bill concerning the control of nuclear energy, which he presumed would in one way or another extend the authority of the Manhattan Project in military hands. He and his colleagues played a major role in the legislation which led to the creation of the Atomic Energy

Commission, but it is noteworthy that many of the most prominent positions were soon given to individuals who had endorsed the May-Johnson Bill.

There is no evidence to indicate that Szilard was ever consulted by Bernard Baruch when the latter dealt with the United Nations in connection with the Lillienthal Acheson report. Instead, Baruch selected as his advisors individuals who came out of a more conventional stream. Nevertheless, we can assume that Szilard kept close track of the developments through the various organizations of scientists, and that he placed great hopes upon the success of some form of agreement toward international control that would override narrow national interests as he saw them.

When it became clear that the Soviet Union would not accept the Baruch plan, Szilard took the initiative in his own way in an attempt to gain some form of international agreement. As one of the instruments to achieve this purpose, he formed the so-called Einstein Committee, on which I served for a number of years after returning in 1947 from a year at Oak Ridge as a colleague of Eugene Wigner, and where I was Director of the first training program on nuclear reactors. This committee was based on the ashes of an earlier committee, the Emergency Committee of Atomic Scientists, which had burned itself out. While Albert Einstein was its honorary chairman, most of the planning and action was carried out by Szilard and by Harrison Brown, a geochemist who had worked with the Manhattan Project and was then at the University of Chicago. One of Szilard's greatest hopes in the period between 1946 and 1949 was to attempt to arrange a meeting of scientists from the United States, the United Kingdom, and the Soviet Union at a neutral place--preferably a Caribbean island such as Jamaica or Trinidad, in order to discuss means of establishing international control over nuclear energy in general--and nuclear weapons in particular. Several meetings were arranged between Harrison Brown and Foreign Secretary Andrei Gromyko to discuss this proposal, but at the end, Gromyko was compelled to say that his country opposed such a meeting. What we did not know then and which came to light in 1949 was that the Soviet Union had obtained sufficient information through Klaus Fuchs to get on with its own program for a fission bomb much more rapidly than most of the experts in our government had believed possible. The genie was out of the bottle. Szilard's dream of such gatherings of scientists took a full decade to be realized, and is now reflected in the somewhat humdrum meetings associated with the name, "Pugwash."

I am inclined to believe that this failure at that time in the cycle of events accompanied by the Soviet rejection of the Baruch proposal had a very profound effect on Szilard, and that henceforth he believed that a very destructive world war was inevitable. National ideology had gained the upper hand.

By the time the Pugwash movement had come into being, Szilard was no longer at center stage. In fact, he was devoting much of his time to biological research, at which he was highly innovative, and to writing intriguing science fiction. I noted that in one of the histories of the Pugwash movement, published in England, an English group is mistakenly given almost complete credit for its conception and realization. Szilard became much more the interested observer, offering a combination of imaginative and unconventional proposals, whereas Harrison Brown became the organizer.

If one looks back on the various organizations of scientists which were generated between the period of 1945 and 1948 in response to the work of the Manhattan Project as, for example, is related in the comprehensive work by Alice Smith, *A Peril and A Hope,* one realizes that at that time Szilard was in one way or another deeply involved in the creation and guidance of most of them. He covered essentially all activities, such as the organization and development of the Federation of Atomic Scientists, the Federation of American Scientists, the various movements at the national laboratories and universities, the action in Washington, and the creation of the *Bulletin of Atomic Scientists*. In addition, he aided in the collection of philanthropic funds and their distribution to the various operating groups.

There is little doubt that his basic motives were humanitarian, and that his ultimate goal was to help in the formation of a universal government which would assure world peace. Some of us who worked with him, however, were sometimes put off by the fact that he was initially at least inclined to see no really fundamental difference between our own form of government and that of the Soviet Union. In fact, I believe that it is safe to say that in spite of the freedoms which he enjoyed in a democratic society, he distrusted the idea that the general public should have a strong voice in determining the course of events. On this score I might quote Eugene Wigner, one of Szilard's oldest friends, who stated in his memorial to Szilard: "It was a favorite saying of Szilard that one stupid person may be right as often as a bright one, but two stupid people will be wrong much more often than two bright ones. They should not have as much to say about national politics as the latter. However, his good will toward all including the stupid ones was always wholehearted and no one could accuse him of malice."

Unfortunately, many of the organizations which Szilard helped to create have fallen into the hands of individuals who have more than humanitarian goals.

There are, of course, two great weaknesses to Szilard's approach to societal matters. First, it is very hard to get a representative group of even the most intelligent individuals to agree on relatively simple issues, let alone upon matters as complex as world government. Something in the nature of coercion by a selected few would be needed, and we know where that can lead if appropriate good will and safeguards are inadequate. Moreover, the various meanderings of the United Nations Organization over the past decades do not give us confidence in the wisdom or steadfastness of any such international organization. Second, various groupings of people on our planet are highly diversified in essential ways, and for one reason or another, end up with different forms of government. I do not believe that it is reasonable at present to equate the open democratic countries with dictatorial ones in the way in which Szilard might have preferred. The former are worthy of being defended in their own right until such time as we are all prepared to form an appropriately effective world government.

In the meantime, we must continue to carry on in what some may regard as a schizophrenic way by developing nuclear energy as a boon to mankind, in order to provide essentially unlimited amounts of clean energy for the enrichment of our lives. At the same time we must do our best to keep the destructive genie tightly sealed in the bottle, while maintaining our own defenses at a realistic level. I know of no other rational approach to the complex situation which we face in the world in which we live today.

INTRODUCTION OF
DR. K. ALEX MÜLLER, NOBEL LAUREATE
IBM ZURICH RESEARCH LABORATORY

Dr. Donald Gubser

A little over two years ago, I participated in organizing a meeting which also was a commemorative meeting honoring the 75th anniversary of the discovery of superconductivity. The Chairman of that conference, Ed Edelsack, is here in the meeting today; as well as Ted Berlincourt, who organized a special symposium featuring historical reviews in the field of superconductivity. The meeting took place at the end of September 1986. At that time, I think many of the people felt that the field of superconductivity was relatively mature, and there didn't seem to be much exciting new physics in superconductivity. Many of the applications which people had proposed for superconductivity were slow in coming to realization, although there was a commercial market for superconductivity: in magnets for magnetic resonance imaging and certain military applications.

We did not know as we were celebrating this 75-year anniversary that the speaker whom I am about to introduce had just published one of the most exciting discoveries ever to occur in the field of superconductivity: one which turned the field around almost overnight. It was one of the most exciting discoveries in the last few decades, and perhaps there will be a 50th anniversary celebrated for this discovery.

As you know, the President of the United States realized the importance of this discovery and he held a special conference on superconductivity. It is really exciting for me, having been in this field all my career, to participate in the excitement. It is very difficult, however, to stay up with the field. Many reprints appear daily which must be at least perused to find out what is happening. The national vigor, the national visibility, is certainly difficult to handle; but new science is there, as is new technology. It is really wonderful to know that the age of discovery is not over.

Professor Müller received his Ph.D. from the Swiss Federal Institute in 1958. After five years working at the Battelle Institute, he went to IBM in Zurich in 1963. Professor Müller also is a professor at the University of Zurich. Throughout his career, he has been working in the field of ferroelectricity and superconductivity.

Professor Müller has done truly pioneering research throughout his career, looking for new concepts and new opportunities, rather than just exploring and filling-in details of other work. He is certainly not using conventional wisdom in his approaches to materials, as the discovery of high-temperature superconductivity would indicate. Very few people were looking at ceramics, low-carrier concentration materials, for superconductivity. Conventional wisdom says that metals and inter-metallic compounds with large number of electrons are the place to find superconductors. It was this non-conventional area of research that led Professor Müller to the discovery of high-temperature superconductivity.

Professor Müller has over 200 technical publications in the field of superconductivity, and he is still very prolific. I met Professor Müller at another conference about a year and a half ago. After speaking with him for some time, I retired for the evening around 9 o'clock. Today, I found out that he stayed up to midnight, talking with others and doing homework for another publication in superconductivity. So, he is still very active in the field.

In 1973, Professor Müller was manager of the Physics Department at IBM, and in 1982 he became an IBM fellow. Since 1985, he and his group have been doing research in superconductivity and ferroelectrics.

Professor Müller has many awards. I read his resume and really could go through them all. As we all know, Professor Müller won the Nobel Prize for his discovery of high-temperature superconductivity in 1987. After searching around a little bit, I finally found where he mentioned his Nobel Prize award, just one of several awards! The modest mention of his Nobel Prize in his resume is indicative of his character. Some of the more prestigious awards received by Professor Müller are the Fritz London Award and the American Physical Society International Award for New Materials.

He also has received many honorary degrees from the University of Geneva, the University of Munich, Boston University, and Tel Aviv.

I would like at this stage to turn the platform over to Professor Müller. The title of his presentation is "High Temperature Ferroelectricity and Superconductivity." I believe he will tell us most about superconductivity, and perhaps we will hear a new discovery--and will have to have a 50th anniversary, 50 years from today.

LECTURE

HIGH TEMPERATURE FERROELECTRICITY AND SUPERCONDUCTIVITY

Dr. K. Alex Müller

First, I would like to thank Dr. Gubser for his very kind introduction and, also, for the invitation to come here and meet a number of you I have known before, especially Professor Liebowitz, and to meet people I know only by name but not personally. As Dr. Gubser said, in consideration of the audience, I restrict myself to the superconductivity area because including the ferroelectric part may actually narrow the scope of this talk rather than broaden it--for reasons I don't want to discuss here.

Now, for the high temperature superconductivity I plan to give you a summary of how we arrived at these oxides and, if I have the time which I don't know yet, also tell a bit more for those of you who are specialists in the field.

Superconductivity, as probably most of you know, was discovered in Leyden by Kamerlingh Onnes. You see here a picture of him. Maybe I should mention that he got the Nobel prize for liquefying helium, and that superconductivity was part of his research. He could liquify helium at the beginning of the century, and hence kind of monopolized research in low temperature physics because his competitors were not able to do that. He had liquid helium in his laboratory for about 12 years, and among other things he wanted to know what the metallic conduction would be at low temperature. At Leyden they quickly realized that the resistivity of the metals would become independent of the temperature, that on cooling, it would settle on the so-called "residual resistance" due to impurities, and therefore they decided to take a metal which was very pure. Mercury is such a metal, because you can distill mercury and therefore make it purer and purer.

So they took a mercury rod and measured the resistance. What they found is shown in this figure. (Fig. 1) Here is the resistance and here the temperature scale. This is about the boiling point of liquid helium, 4.2 degrees Kelvin, from which you have to subtract 273 if you want to have centigrades. The resistance came down linearly on cooling and suddenly disappeared. Actually it was a student who measured that, and came to Onnes and said, "Well, the resistance disappears." Onnes suspected that some current lead had been detached and said, "Go back and measure again," and the student went back and measured. He came back again and said, "The resistance disappeared." So, Kamerlingh Onnes said, "Now, I am going to measure myself," and he found, indeed, the resistance was disappearing. Then he did a number of very nice experiments to prove that the resistance really dropped beyond any detectable value. At that time, this was done with galvanometers and so on.

The transition temperature of mercury was, as I said before, at 4.2 Kelvin or about the boiling point of helium. Slowly over the decades, the transition temperatures increased, until at the beginning of the seventies 23 Kelvin had been reached for Nb_3Ge, which was the highest one. It was at least above the boiling point of liquid hydrogen, whereas before, in all the other compounds liquid helium with its low boiling point had to (Fig. 2) be used for superconductivity to occur. One thing you see is that most of these compounds are cubic and contain niobium.

One important property of such superconductors is of course that superconductivity withstands a magnetic field. There is the so-called "Meissner" effect, and this is the crucial test whether you have a superconductor or not. If you set the temperature above the critical temperature and then apply a magnetic field, it penetrates the superconductor. If you cool a Type 1 superconductor (Fig. 3) below the critical temperature, the magnetic field is completely expelled. This magnetic test is also very important with high T_c superconductors because in the past two years you often got news that a superconductor at nearly room temperature had been found, but all these compounds have not passed the Meissner test.

Actually in Type 2 superconductors the magnetic field can partially penetrate but you still get a higher diamagnetic contribution which has to be looked for. The observed superconductivity was found in metals, and I have shown you the progression of the transition temperature (F where the resistivity disappears in the metals. I am now going to talk about the oxides, but since 1980, you also have superconductivity in the organic conductors, which I think are quite promising because if you plot the transition temperature versus time, you find that the slope of T_c versus time in these organic superconductors is even steeper than in the oxides. Earlier these organic conductors were more like one-dimensional strands, but now they are also becoming two dimensional. I may come back to that if I have time.

Now, to the history of superconductivity in the (F oxides. The first two oxides to become superconducting were reduced strontium titanate. Strontium titanate normally is an insulator, but if you reduce it, it becomes a conducting metallic compound. The National Bureau of Standards group, it was Frederickse's group, found a transition temperature of 0.3 Kelvin here. This was not a high temperature. In the same year, the group of Bernd Matthias, in San Diego, found in the tungsten bronze a T_c of 0.6 Kelvin. Having found that, it was okay. So, one knew that also in oxides you can have superconductivity, but it was regarded partially as exotic because the transition temperatures were very low. The lattice of (: both of these compounds was of perovskite structure. Here is the structure of strontium titanate. What you have is a symmetry which is the highest you can get, namely O^1_h. At the center of the octahedron, there is a transition metal ion, at the corners there is an oxygen, and in this center--for charge compensating reason--a large ion.

At that point and having heard the address by Professor Seitz, let me digress a bit. I had not planned this, but now will do it. Let me try to shed some light on the more transcendent aspects of this research. Professor Seitz has alluded at the beginning of his talk to the work of Segre and especially to certain images which are called mandalas. He mentioned that spheres or circles are images of these, and if you read, for instance, an essay by Wolfgang Pauli, he shows, I think it translates as--it is in German--"On the archetypical pre-existence of scientific discoveries." Well he shows there that Kepler strongly believed in the spherical aspect of God and therefore was looking for projections of that on a plane which would be circles. This is why he was so strongly convinced that he would find orbits in the shape of circles; finally he found ellipses. But there is another aspect of these mandalas, namely, that often you have, in addition to circles, squares. For me, and I can only talk for me, this perovskite structure here has had this aspect because I have been doing research on the perovskites for over 30 years, on many properties such as structural phase transformations. For instance, these octahedra have a common corner. Thus they can rotate, say around such a tetrahedral axis and also around another axis, thus breaking the symmetry. This has led to the discovery of critical phenomena in structural phase transitions. It has also led to the discovery of the so-called "Potts transition." We were, I think, the first to prove the existence in nature of a Potts transition by applying stress along the diagonal axis. Further, we found photochromic effects and so on. It is based on these successes that I felt that maybe looking at the possibility to further enhance superconductivity in these crystals could be worthwhile--and it worked out.

Of course, in order to achieve something, you have to know a bit more. Thus, let us go back for a moment to the history. Johnson, a student of Matthias, found in 1973 that in the lithium-titanium spinel you get a T_c of 13 Kelvin. This was already something which was worth thinking about. Of course, then one could not make single crystals of this material. So, one did not do much (Fig. 7) work on this system. Then Slight at Dupont found, again in the perovskite lattice, a T_c of 30 Kelvin in the barium-lead-bismuth oxide, and recently by replacing barium by potassium, 30 Kelvin were reached in a real perovskite material at AT&T. What was remarkable at the time was that the concentration of carriers as measured by the Hall effect was very low, namely, only some 4×10^{21}. If you look at the well-known BCS formula for weak coupling, the T_c has exponential dependence on the carrier density at the Fermi energy, not the same as this density n given here. N(E) is measured per unit cell times the electron-electron attraction mediated by electron phonons. You can see that if this quantity N(E), which is related to this n here, is low, you have to have a large electron-phonon interaction, and so, from this knowledge, one wanted to go further. From this thinking, we derived the concept for our search for the high-temperature superconductivity, namely, to search in metallic oxides and turn away from the intermetallic compounds; also, because reading reports on intermetallic compounds, I became aware that practically no more progress had been made in those areas for over a decade.

Now, perhaps just to give you an image of where we were working, Georg Bednorz and myself, this is the IBM Research Laboratory near Zurich. It is atypically small for IBM. It now employs perhaps 200 people, but at the (F time there were only 150. This is the materials and device area here. This is the communications area. Here is the adminstrative building. I should rather start up here. This is the Lake of Zurich, and here is the Autobahn, where if you drive for an hour and a quarter you are in the skiing resorts. The cafeteria is an important meeting place, and until recently because of the size of the laboratory everybody knew everybody else, which I think is an advantage in certain aspects in research.

Of course, you often need to have very large facilities like the IBM Thomas J. Watson Research Center. There you have silicon lines and other research areas which will require quite a bit of personnel and also, a laboratory of size. The work which we started was with Georg Bednorz. He is on the left. The picture is from (F the Nebelspalter, a Swiss satirical publication, and maybe it illustrates that you should take things seriously but perhaps not too seriously. This is why I like to show this transparency.

Of course, we needed to have a new, an additional concept. You cannot just work in oxides. You will work for decades without finding much, but as I said before, what you want to have is a strong interaction between the electrons and the lattice. This is what we had in mind and having worked earlier on the Jahn-Teller effect, we looked for such possibilities.

We therefore decided to restrict ourselves to working only in metallic oxides containing nickel and copper. Why? Because they have partially filled d orbitals which point toward the oxygen ligands, and therefore we expected them to have a large interaction. It is shown in a simplified form here. You can put, say, a Ni^{3+} ion at the center of this octahedron, and the Ni^{3+} (I has one of the e_g orbitals occupied. There are two independent e_g orbitals. You can have this configuration or that one. They are degenerate, and from my electron spin resonance work I knew that the Jahn-Teller stabilization energy, the way the energy is lowered by distorting this octahedron, is largest in copper or nickel. The Ni^{3+} and also the Cu^{2+} show this effect.

Now, how did we visualize what is happening? Well, I became aware of a theory by the authors shown here, namely Höck, Nickisch and Thomas, in which they tried to explain some resistivity anomaly in metallic Laves phases (these are not oxides) by way of so-called "Jahn-Teller" polarons. In my graph you have the Cu^{2+}, which has three of these e_g orbitals, and therefore elongates. You have essentially two electrons in this blue orbit and one in the green elongated orbit. If you have a Cu^{3+}, you have one electron in one orbit and one in the other, and the octahedron is not distorted. (Fig. 11)

Since these objects can travel along one direction, this would be a Jahn-Teller polaron. I should perhaps mention that nowadays one partially comes back to these views. There are so-called "string" theories, on objects which travel along a string. A further aspect of this concept was that it should have a mixed valence. This is important. So, you have Cu^{3+} and Cu^{2+}.

Georg Bednorz and myself first worked for about two years on the nickel compound, and we did not make any progress toward superconductivity, rather toward localization. We knew that the perovskite $La^{3+}Cu^{3+}O_3$ is a metal. It has a Pauli susceptibility. This was known, but for this valence, you have no orbital degeneracy. At that moment, Georg Bednorz while searching the literature found a paper by Michel and Raveau from France, who had produced this mixed perovskite. Namely, they had replaced part of the lanthanum by barium and therefore had a mixed valence on the copper. Why did these people look into these compounds? The reason was catalysis. The first works on mixed perovskites were done by Paul Hagemüller's group in Bordeaux-Tolance about 1973, then they gave up. From about 1978 until 1979, a Russian group continued. Finally, Michel and Raveau, a chemical group, worked on it. Why are these compounds good catalysts? Because they easily lose oxygen, and therefore are oxidation catalysts. This however is derogative for the high T_c materials because you want that the oxygen is stable; otherwise you (Fig. 12) change the stoichiometry, and the T_c changes. This is a difficulty.

Now, in Rüschlikon we prepared the compound in a different way than Michel and Raveau did. They just took the oxides and fired them together, whereas having done quite a bit of work in ferroelectricity and especially in quantum ferroelectricity, we wanted to have a microscopic mixture of the compounds. Georg Bednorz had developed a method of co-precipitation from aqueous solution of oxalates which were then reacted by heating. Whereas Michel and Raveau had heated their substance to 1100 degrees centigrade, he heated our compound to 950. This was a stroke of luck because by heating the compound in this way, he found that the resistance was disappearing. However, the compound was now a *derived* perovskite. It was the one you see here on the left, which is by now well-known. You again have octahedra, with copper ions at the center surrounded by oxygens. Furthermore, there is a sodium chloride layer; here in red is the lanthanum, and here an oxygen; then again a sodium chloride layer, followed by another layer of perovskites and so on. This compound has a transition temperature of 35 Kelvin.

Now, going a bit further in the work, I come to the compound here on the left, which was found by Paul Chu and Dr. Wu. We shared the American Physical Society Materials prize. At first, the compound looks a bit complicated, but what you recognize is that you do not have any octahedral layers but that now you have pyramids here. You have five oxygens and the copper in between. They are again linked together at their corners and mirror symmetry exists about this plane. Here you have again the pyramids facing upwards. In the middle you have an yttrium layer. So, you see, there is a progression. At the beginning, we first looked at compounds with a perovskite lattice, but we found superconductivity in the layered perovskite. A higher T_c is now in a compound which has these pyramids where more or less the octahedra are split. You can go further with these structures found in 1987. Some of the results of 1988 are shown here. These are now compounds which contain either thallium or bismuth instead of rare-earth ions. I do not show you the formulae, I hope you see the progression: Namely, on the left, you have a compound which transforms at 60 Kelvin with two layers of oxygen of either bismuth or thallium, and then you have again here the copper octahedra linked together.

The next compound, called n = 2, has pyramids which we have already seen in the Houston-Alabama compound. Then you can go to n = 3 with an even higher T_c, where you have pyramids and also squares. These compounds show a still higher T_c, both for the bismuth and the thallium compound. The thallium compound now holds the confirmed world record of 125 Kelvin. So, you may think, aha, you go still higher with n = 4, and you will get an even higher transition temperature. This is not the case, because it is not always true that the sky is the limit. If you make the compound n = 4 for thallium, then the T_c is lower. Professor Raveau in Caen has done this experiment.

Now, what do we learn from this? The compounds are all layered copper oxides, and they form a new class of superconductors. I may now, in the remaining time, say a few words about their properties. First of all, you want to know whether the mechanism is more or less the one we know from normal superconductivity or not. One characteristic of the classical superconductor is that they contain Cooper pairs, namely that you have two electrons with one spin down and one up. They also have opposite momentum, indicated here by the arrows k^- and k^+. The electrons are "on speaking terms" over a certain coherence length. In normal superconducting metals, the intrinsic coherence length is very large. It is about 1000 lattice distances. We will see what implication the coherence length has for the new oxides. (Fig. 15) (Fig. 16)

First of all, you want to know whether we have such Cooper pairs, and I think one of the nicest experiments in this area was by Gough's group in England. There they measured the magnetic flux through such a core. From the work of London, one knows that this flux is quantized. The flux is n times a certain fluxon Φ_0 which is given by h, the Planck constant, times the velocity of light c divided by the charge q. In normal superconductors it was shown that this charge is 2e because you have two electrons. The same was shown by Gough to be the case for the oxide superconductors. What they did is they measured the flux as a function of time by a magnetic method. This flux was found to be quantized with a charge (Fig. 17) q of 2e. Therefore the existence of Cooper pairs was confirmed.

The Cooper pair and especially its coherence length are very crucial for the properties of the new compounds. Why? If you put a superconductor into a magnetic field, you can increase the magnetic field until superconductivity breaks down. This is called the critical field, and it is very large in these oxides, of the order of a megagauss. It can even be 2 megagauss. It has not been measured, only extrapolated. One megagauss means 100 Tesla, and this of course immediately starts you thinking that one may apply them to generate extremely high magnetic fields.

The critical magnetic field is given by a very simple formula, namely, it is equal to the flux quantum (F divided by the area of a circle with the radius of the coherence length. Because H_{c_2} is so big, if you put in the numbers, you will find that the coherence length of the superconductors is very small. The most recent experiments yielded that in one direction ξ is only 2 to 4 Angstroms. This is lower than the unit cell distance. In the planes, ξ is of the order of 20 Angstroms. Because of that, you observe phenomena which you did not see before. Before I go to that, let me say why the coherence length is so small--in words so that I need not bore you with formulae. It is basically due to the Heisenberg uncertainty principle. In a normal metal, these electrons have huge velocities and therefore they have to keep apart. This is like the electron which does not fall into the nucleus in an atom, because there is the uncertainty principle which keeps the electron out due to its large velocity.

In these new compounds, the velocities are considerably smaller because of two effects. First of all, the effective mass, that is the mass of the carrier, is of the order of 4 electron masses, and relatively large. Furthermore, the density of states is quite small, it is of the order of 10^{21} per cubic centimeter, two orders of magnitude smaller. Therefore Fermi velocities, if they exist, are quite small.

There is now a big debate among the theoreticians and maybe I should say something about it, but only in words. Do new experiments prove the existence of a Fermi liquid surface in these oxides or not? If not, then the theories which are based on magnetic interaction, the resonance valence bond state or fractional quantum states have an edge. If there is a Fermi liquid surface, you can, to a certain extent, deal with the BCS approach, whereas of course the electron-electron coupling may not be phononic but can be excitonic. This is one aspect which I think has some future. There are now more and more experiments which indicate that a Fermi surface exists. Those I believe the most are nuclear magnetic resonance experiments. In nuclear magnetic resonance, one can measure the relaxation time and if you do that, you find that for the oxygens where the carrier--I come to that in a moment--follows essentially the temperature dependence which Hebbel and Slichter had found in Urbana for the aluminum metal superconductor. This is a quite strong indication that the new superconductors behave like a Fermi liquid. There are other experiments. Now, I said to you just a moment ago, what are the carriers? The carriers are not electrons in these compounds. These materials are hole superconductors. By doping the (Fig. 19) material, for instance in our compound by replacing the lanthanum with strontium, one creates holes. These are located on oxygen orbitals. This is generally accepted now, and they are essential for the formation of Cooper pairs. At the beginning, we were thinking--and I showed you a picture--that you would have the hole on the copper site. Instead of having Cu^{2+}, one would have Cu^{3+} adjacent to an O^{2-}.

More recent experiments, and I show you just one experiment, indicate that a large fraction of the holes are on the oxygen p orbital. Here is the relation. The notation is more or less this one. The configuration here (Fig. 1) would be $3d^9$ and a hole in the p shell. Here on the left, you have the hole in the 3d configuration. You have one electron fewer, and you have no holes in the p shell. This evidence, which is important, is related to ferroelectricity. Why? The oxygen in vacuum is stable at O^-, not O^{2-}. What stabilizes the oxygen as 2^- in the oxides is the Madelung energy. This is true in a three-dimensional lattice. If you have two dimensions, the potential is such that the holes go onto the oxygen. In a similar way in ferroelectrics, the O^{2-} in the perovskite barium titanate tries to put its last electron as far away from itself as possible. So it puts it into the titanium orbitals. Thus, there is a relation to the ferroelectricity of these superconductivity compounds.

Now you will ask me, "How do you know that we have the oxygens O^- partially?" There is a specific experiment, an electron loss experiment. What you do is you take a layer of the material, and you shoot an electron through. So, you get rid of some surface effects. You really know what is happening inside the superconductor, and if there is an O^-, there will be a characteristic transition, namely from the 1s to the 2p state. If the 2p state has six electrons, i.e., it is full, there is no absorption. If there is one electron missing as in O^-, you get an absorption which occurs at 528 electron volts.

Such an experiment was done by the Karlsruhe group of Nücker and coworkers using the core level excitation. First look at the experiment with the original compound, the $LaCuO_4$. You replace the three-valent lanthanum by the two-valent strontium, and thereby create holes. If you have no strontium, $x = 0$, you look at the absorption, and you find that at 528 eV--nothing. Now, you dope it with 15 percent strontium, and here is the peak. You have holes on the oxygens. Now, let us look at the compound from Houston: the same effect. Here the hole concentration is fixed by the stoichiometry of the oxygen. For O_7 you have maximum hole concentration. If the oxygen stoichiometry is 6, you have an insulator. So, if y is 0.8, you have a stochiometry of 6.2, essentially an insulator, and there is no absorption. If you have 0.5, you get absorption. If you used $y = 0.2$, which means 6.8 here, it is a good superconductor and here is the peak. So, this proves to you that the holes are on the oxygens. (F

Of course now you want to know where exactly are these holes, since there are different p orbits. This problem has been tackled this year by measuring the anisotropy of the electron losses, and one found that the holes are in the planes of the oxygens. So, you have a p_x and a p_y orbital in the plane, and the hole should be there. Whether it is in the X or the Y orbital, we do not know. Therefore we do not yet know whether you have sigma or pi bonding.

. Let us look at the behavior of the superconducting transition temperature as a function of the hole concentration. This is work from several groups. You begin doping. No superconductivity. Therefore there clearly is a threshold for the T_c. From here, the T_c increases, and then decreases again. From there on you have a normal metal, and any theory of superconductivity has to show quantitatively why you get a normal metal on the right side at high hole concentrations. What happens on the left side? The material is an antiferromagnet. I show you here a diagram from a Japanese group, but other groups have also obtained this diagram. So, here you have the doping of the holes, and here T_c or the Néel temperature T_N, and what you find is that doping strongly, dramatically reduces the antiferromagnetism. (Fig. 21) (Fig. 22)

Here is a so-called "spin glass phase" which actually extends into the superconductor, and here you have superconductivity. Another notation is used in this graph: here 2-1/2 means 5 in my previous graph. With that, the belief that magnetic interaction may be important was quite prevalent a year ago, and quantitatively the change of the antiferromagnetic phase was elucidated by the fact that if you have holes, the holes couple ferromagnetically into the antiferromagnetic lattice, and therefore, you get a frustrated situation. The antiferromagnetism is destroyed, and you obtain a spin glass phase.

If this were the case and if magnetism were important, then one should seriously consider magnetic theories like resonance valence bond theory and others. However, the resonance valence bond theory tells you that the gap disappears in one wave vector direction, and you should get a specific heat effect, which is linear with temperature, which was not found by the Berkeley group. This here is for the bismuth compound. In this graph T^2 is plotted against the heat capacity divided by temperature, and you see that all these lines beautifully go to zero at these green points; this means that you have no linear term. Given these measurements it is very difficult to think that the resonance valence bond is prevailing. (Fig. 23)

Now, you would like to know what is the superconducting gap in the material, and whether you can do magnetic relaxation experiments. I have just mentioned

.the classical work in aluminum by Hebbel and Slichter. You can perform the same measurements in these new compounds, and you will find a gap that is considerably larger than what BCS predicts. This is a graph of the relaxation rate as a function of inverse temperature, and you find here a straight line from which you obtain the forbidden gap of the superconductor. You find the ratio is 7.1 which indicates that you have strong coupling. There are a number of other experiments in the other compounds which also point in this direction. So, you have not a weak-coupling, but a strong-coupling superconductor.

I think I should come slowly to an end, but I would like to point out one important point with respect to applications, namely, the very short coherence lengths. I mentioned this property of the Cooper pairs at the beginning. Because this length is very short, the behavior at the surface of the superconductor is strongly modified. If you have a superconductor adjacent to an insulator, then the superconducting gap, which is the order parameter of the superconductor, drops quite a bit near the surface even at very low temperature. If you apply the theory of de Gennes, the gap is about one-half of what it is inside at the material. This is quite different from normal superconductors with their long coherence lengths, and where the gap really stays practically the same at the surface and within the compound.

If you now enhance the temperature to near T_c, the gap drops dramatically, which has a variety of consequences, both scientifically and for applications. Why scientifically? If you have domain boundaries inside the crystal, or twins, each twin gives a discontinuity in the superconducting wave function. This is one of the reasons that the superconducting state is glass-like, especially in ceramics. There are other aspects, but this is one of them. So, crudely speaking, if you have two domains in a crystal, you have the twin. Then on that side there is a superconducting phase, and on the other side another phase. This forms a superconducting glass state which has been shown in our laboratory: a memory effect like in a spin glass. You can cool the substance down in zero magnetic field, then apply a magnetic field, and measure the magnetization. Then you change the magnetization after an hour, and again measure the magnetization. After another hour, the slope of magnetization changes. You can wait six hours before changing the magnetization. After six hours the system shows its memory. You wait 24 hours. After 24 hours, the system shows its memory, similar as in a spin glass state or, also, in the models of neurons in the biological systems for your brain. Another aspect of the short coherence length across a boundary is shown in my last transparency--Don't get too impatient--It is a measurement from our research center at Yorktown Heights. This is the (Fig. 27) critical current across such a boundary, and it follows that curve. They interpreted it in terms of so-called "Ambegokar-Baratoff formula," but a better fit has been obtained now by Deutscher using the scheme I have just shown you. So, the current as a function of temperature decreases quite a bit across such a barrier. Now, if you want to use high T_c cables or so, you have to watch out. Say these are normalized units. In these oxides, the critical current j_c has been measured at Stanford. It is near what one calls the depairing current. The depairing current is reached if its own magnetic field is so strong that the Cooper pairs get broken, this is of the order of 10^8 gauss, i.e., 100 megagauss. This is a huge amount; however, you see that the j_c drops quickly. What you see is that if you take that particular compound, and you want to work at 77 K, there is not much current left. So far for possible applications--and I think I should now come to the end of my talk and just summarize a little bit what (Fig. 28) I have been trying to tell you here. After its discovery the field has essentially split into three branches, namely, the search for new compounds, which is still continuing, and I should draw your attention to the fact that internationally this research has been extremely successful. Especially if you consider that ferroelectricity was discovered in 1922 in the seignette salt; the next compound was potassium dihydrogen phosphate in Zurich by Bush and Scherrer 14 years later, and 8 years later, at the end of the war, barium titanate was discovered in Russia, in the United States and in Switzerland. So, after 22 years there were three ferroelectric compounds. We already have well over a dozen of the superconductors in only three years. You may know that now there are about 300 ferroelectric compounds which you can use depending on what you want, and therefore I would expect that also in the superconducting field we will see considerable progress. Regarding experimental analysis I have given you a little bit of a taste. I have not been, I hope, too specific. Then of course you have the theoretical models. An essential thing is that in these compounds there are Cooper pairs. As to the models there are the polaronic ones, the magnetics I have discussed a bit, and also the excitonic models. In the latter, it was Bardeen and also Ginsburg, who in the seventies suggested layered compounds. Here also the work of Little may be of interest. It seems that right now, with some modifications these models may be important in understanding what the mechanism of superconductivity is in the oxides.

Of course, many electronic properties have been measured. I could not describe all of them to you. I do not want to show you the entire list. However, there is a gap, and the short coherence lengths are quite crucial-- and with that I thank you for your attention.

: We have time for a question or two. Would anyone like to raise one?

: Has one detected the jump at T_c in the specific heat of the metallic component of the compound?

PROF. MÜLLER: I am not so sure whether they picked that up. You have seen the data. There is no linear term, and what you see is a jump in the specific heat more or less what BCS would predict, but you have to show some goodwill because it is not as nice a discontinuity as you are used to, but rather two slopes of the specific heat which cross over.

: These twins you were mentioning, are they prevalent in all the high T_c materials, thallium and bismuth or --

: No, they are not. You have twins in the yttrium compound, which has been much investigated. Now, you have proof from microwave absorption experiments in our laboratory from Keith Blazey, and at Berkeley from C.D. Jeffries that flux lines penetrate first along these twins, and afterwards they move into the bulk.

: One more?

: Would you please say a few more words about the status and potential you see for organic superconductors?

PROF. MÜLLER: Okay, yes, thank you for this question. Yes, in giving my talks I have always tried to encourage the people who are interested in organic superconductors. I think they really have a future.

: What is the highest temperature?

PROF. MÜLLER: The highest temperature already is 13 degrees Kelvin, and the first organic superconductor was found in 1980. So, in eight years one went from 0 to 13 K. Very interesting are actually the structures, because at first they were laminar in character. They had strands, which were weakly coupled. Then Phil Anderson felt that one could not go much further because if you have strands, then you can get a charge density wave type phase transition to an insulating state. Then you are sunk. However, the high temperature compound which I just mentioned (from Japan) also has layers. So, the compound is two dimensional as in the oxides. But it is more difficult to make it. In contrast, an oxide compound you can make in three days. So, you have a practical advantage. You go and you mix it and fire it, if you have an idea; whereas if you talk to the organic chemist, they tell you that it takes three to four months to synthesize a new compound according to your idea.

Now, maybe I should mention another thing about these organics, namely, that in the oxides we have the copper and the oxygen, and it seems that in the organics it is more the manganese and the sulfur which are important.

: Manganese and sulfur?

PROF. MÜLLER: Yes, and nobody has checked that yet, but it may be that the reason is similar, namely that the charge transfer energies of the manganese to sulfur may be similar to those of the copper to the oxygen--

: The carbon?

PROF. MÜLLER: The carbon apparently doesn't play a role. I don't know.

: (Inaudible) without carbon?

PROF. MÜLLER: You are right, but, I don't know.

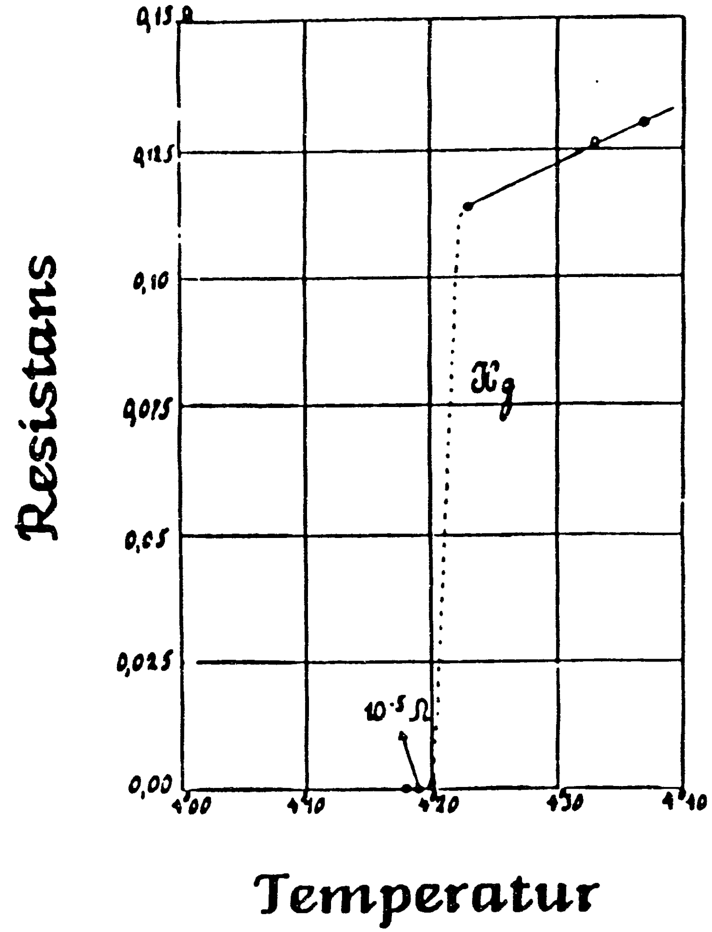
: The carbon is there structurally.

PROF. MÜLLER: What you have in both cases are charge transfer mechanisms. You have very small charge transfer energies, say here between the oxygen and the copper which is now estimated to be of the order of 0.1 eV. For instance, in Los Alamos, their cluster calculation depended on what they started from, once they got the holes in the copper and once in the oxygen because the energies are very closely the same, and it may be the same thing with the organics. I don't know.

High-Temperature

Superconductivity

Heike Kamerlingh Onnes



· H. Kamerlingh Onnes (Nobel Prize 1913) discovered that the electrical resistance of mercury was vanishingly small at temperatures below 4.2 K.

FIGURE 1

Superconducting Transition Temperatures

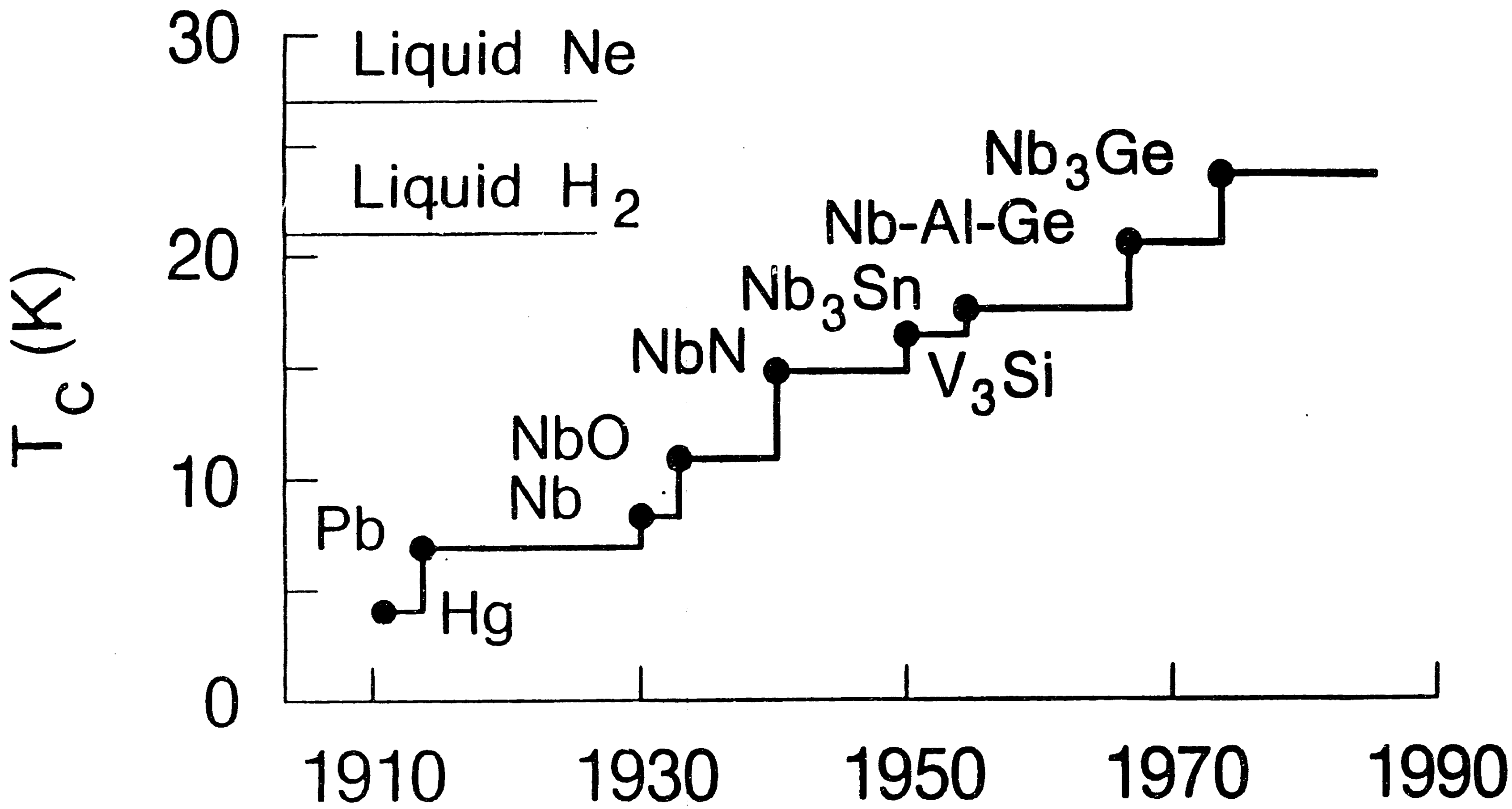
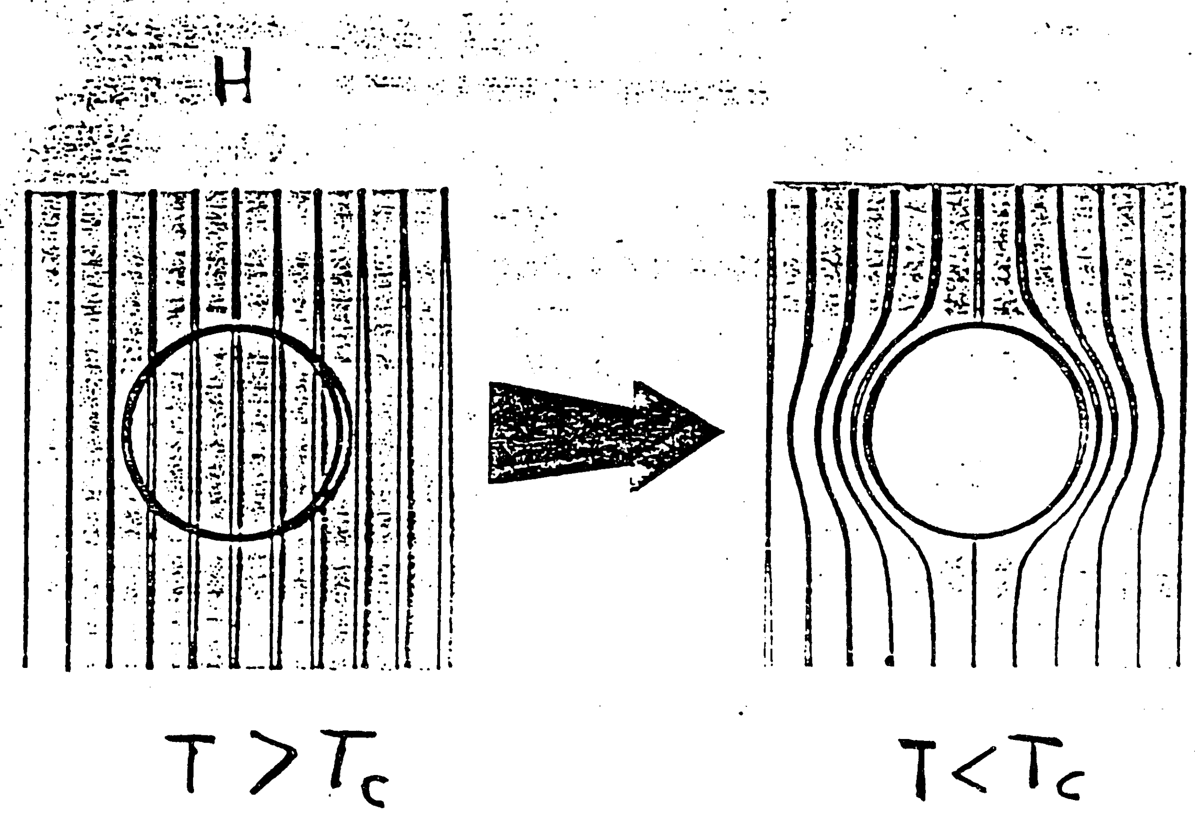


FIGURE 2

The Meissner Effect



- Compensation Current
- Perfect Diamagnetism

FIGURE 3

Observed Superconductivity

in: Metals

Oxides

Organic conductors

FIGURE 4

First Oxides showing Superconductiv

with low carrierconcentratic

1964 $SrTiO_3$: reduced, metallic

NBS-group $T_c \simeq$

1964 Na_xWO_3 bronze

San Diego $T_c \simeq$

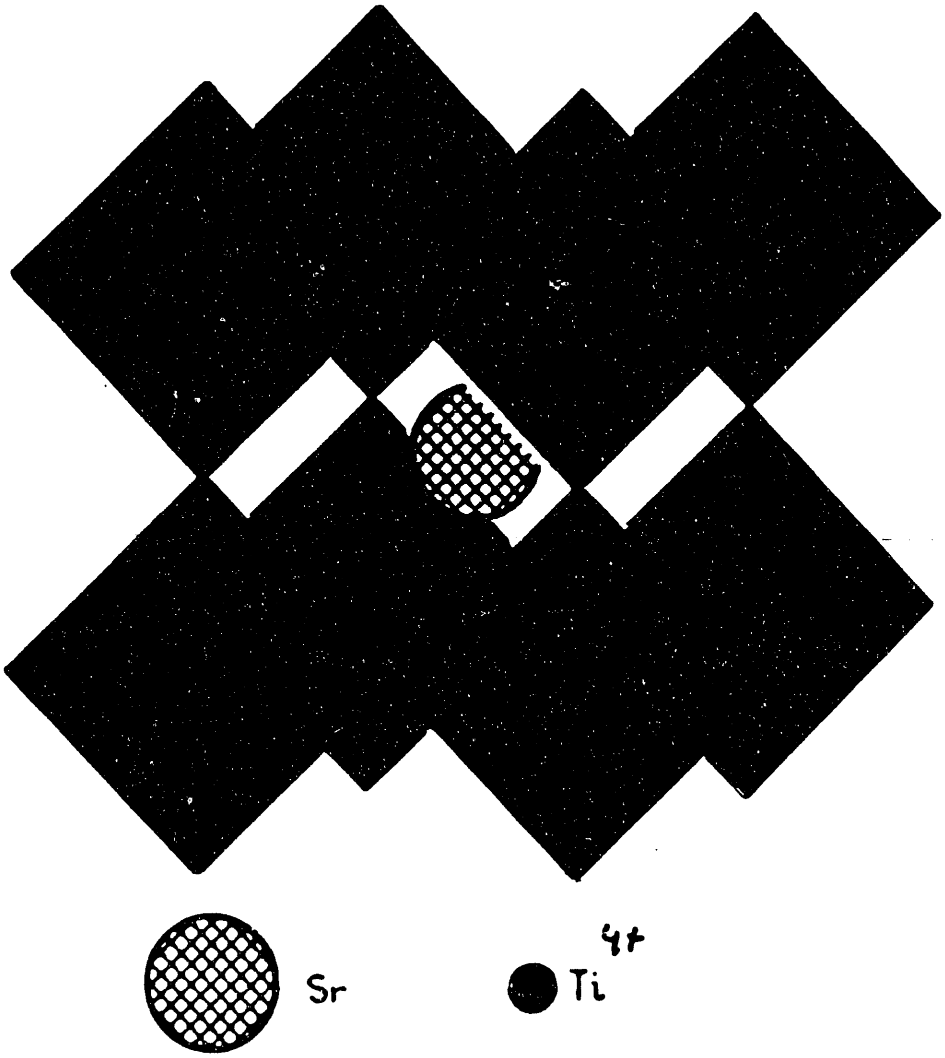
B. Matthias et.al.

FIGURE 5

PEROVSKITE - STRUCTURE

$SrTiO_3$

$LaCuO_3$



Nb^{5+} : $T_c \simeq 0.9K.$

FIGURE 6

Superconducting Oxydes

1973 Johnson et. al. Li-Ti-O System

Spinel : $Li_{1+x}Ti_{2-x}O_{4-y}$ $T_c = 13.7$ K

1975 Sleight et. al.

Perovskite $BaPb_{1-x}Bi_xO_{3-y}$ $T_c = 13$ K

$n = 2\text{-}4 \times 10^{21}/cm^3$ S. Tanaka

BCS: $$T_c = 1.13\,\Theta_D \cdot e^{-1/N(E_F)V^*}$$

V^* : attractive el-el ineraction, normally via el-phonon.

Concept for High-T_c Superconductivity:

- x search in : Metallic Oxydes
- x turn away from : Intermetallic Compounds

FIGURE 7

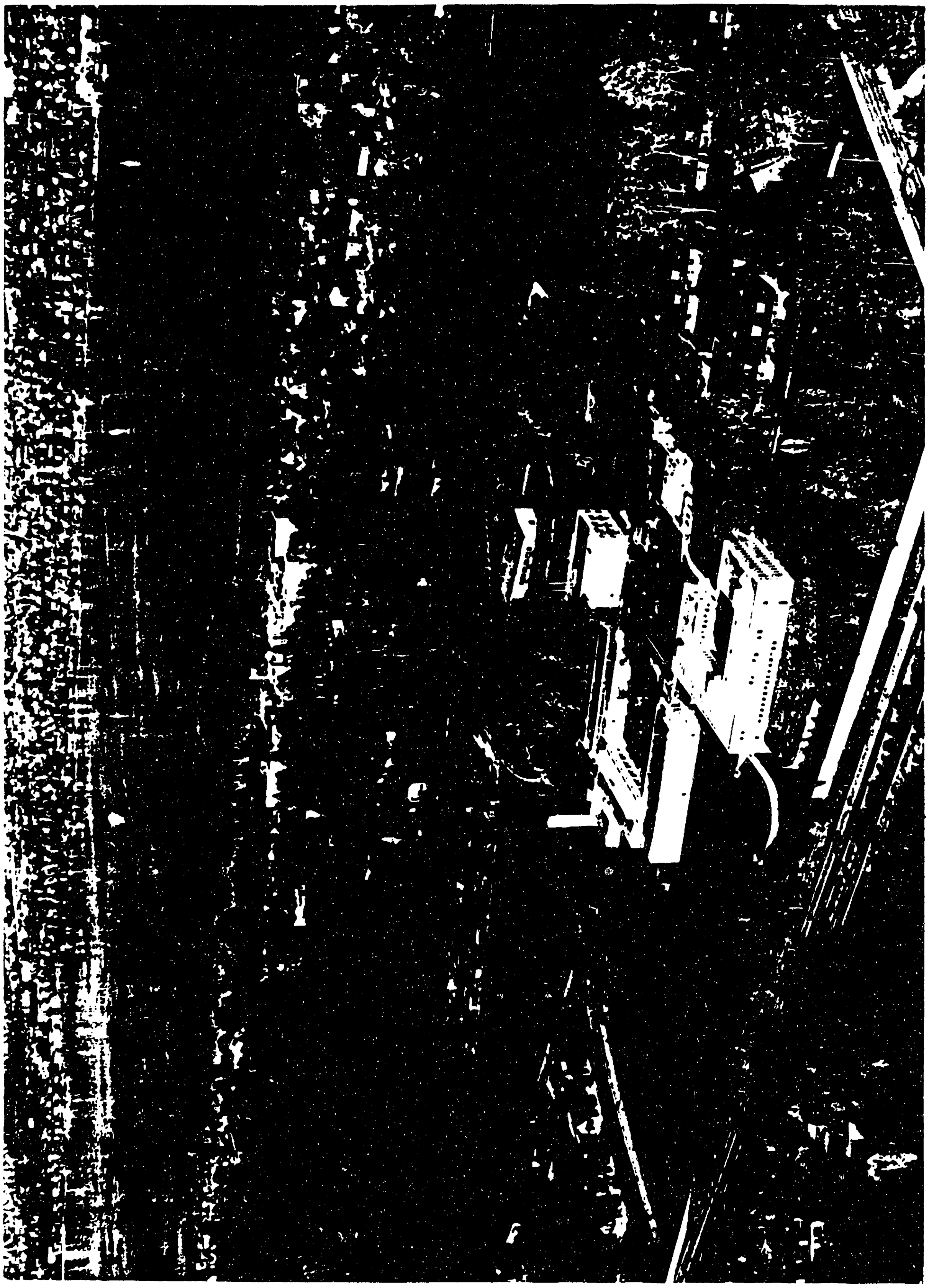


FIGURE 8

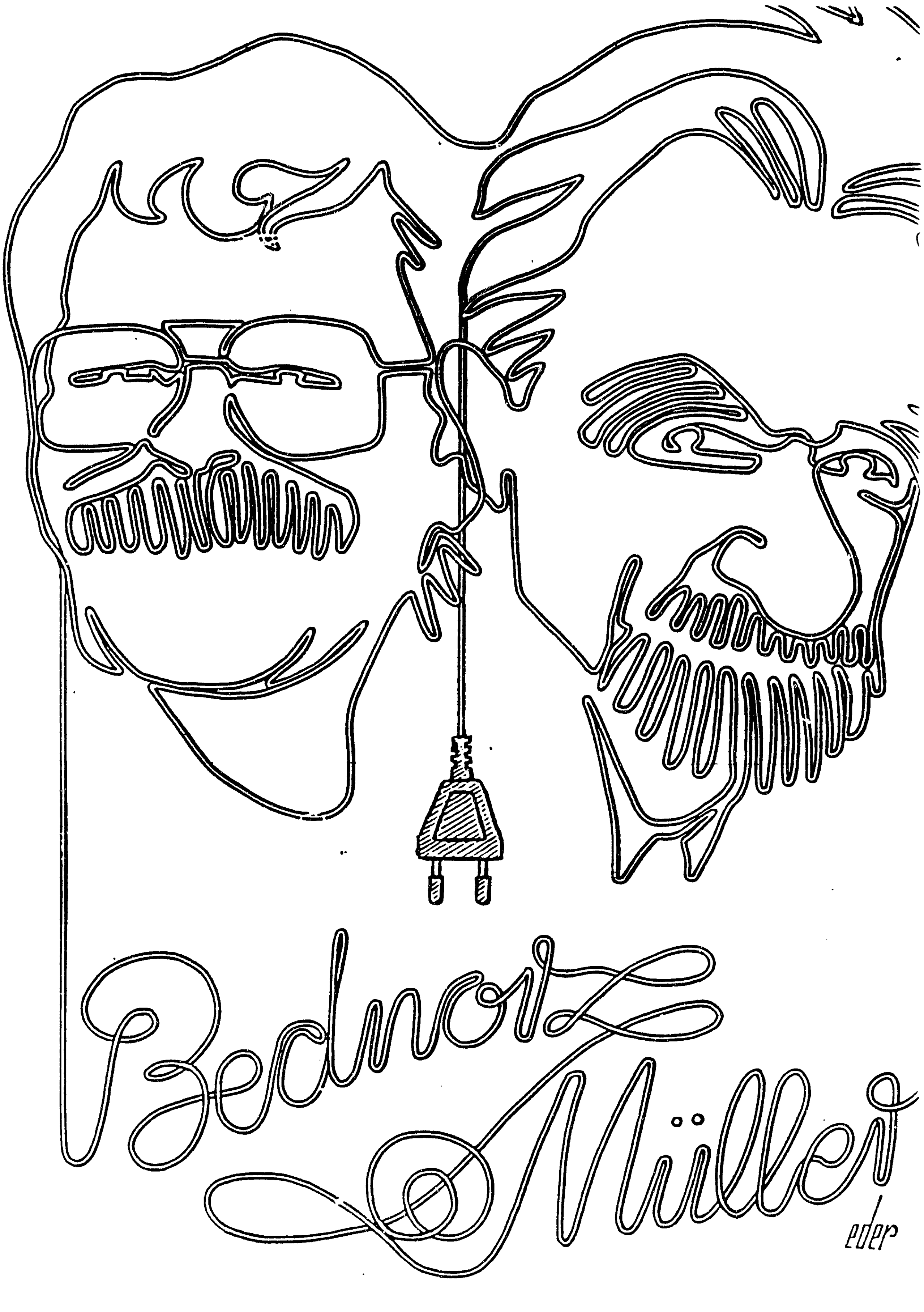


FIGURE 9

Restrict Search to

Metallic Oxydes containing

Ni or Cu

their partially occupied d-orbitals point towards the oxygen ligands

→ large interaction

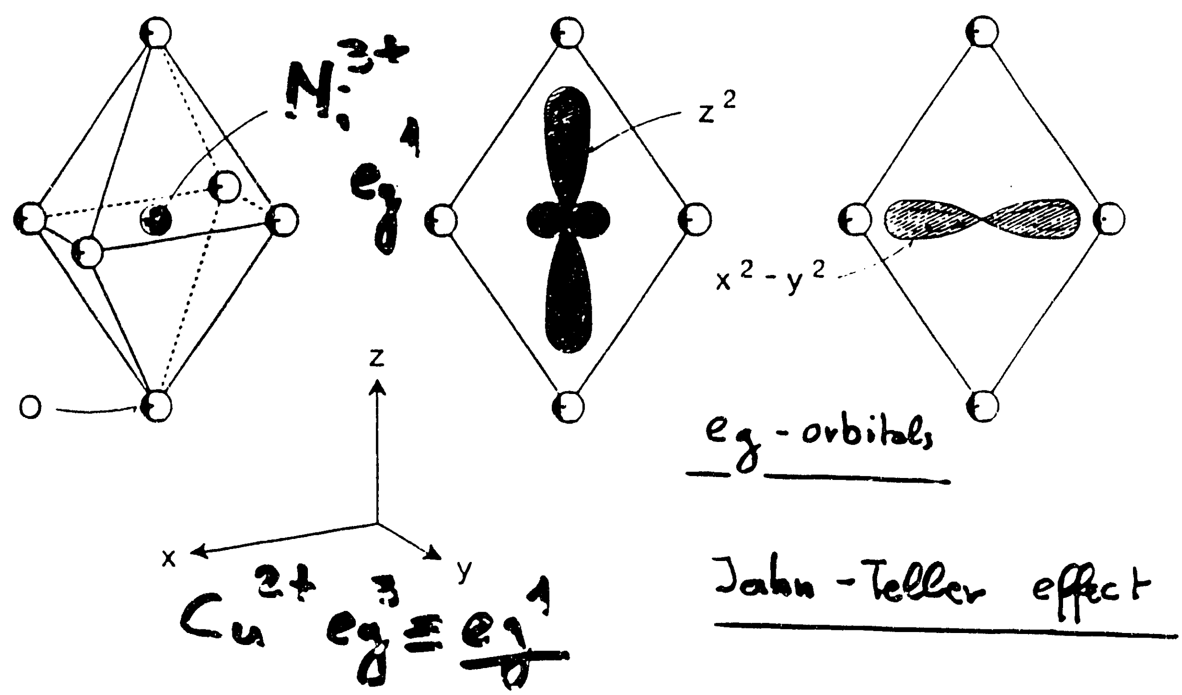
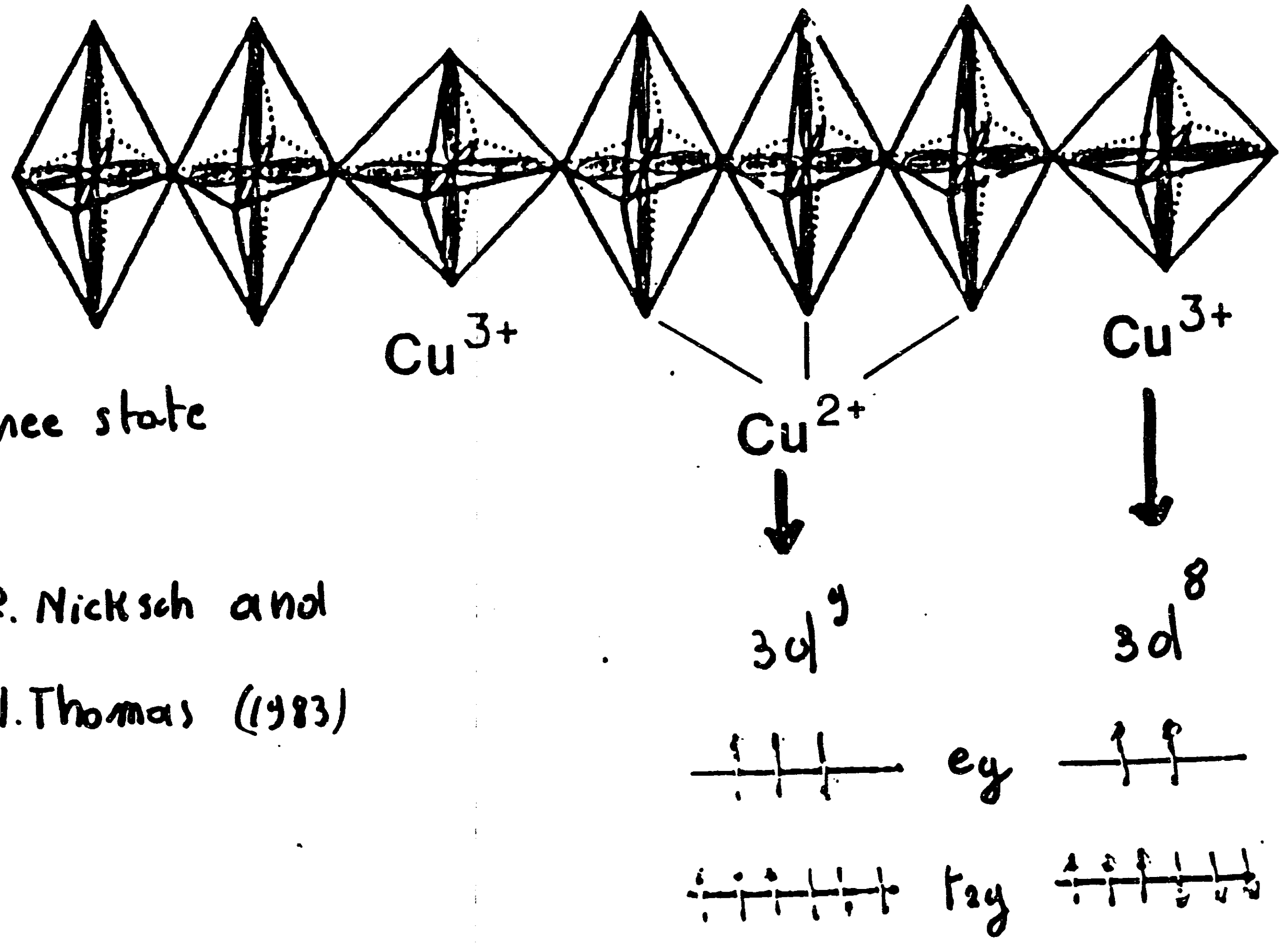


FIGURE 10

Jahn-Teller polarons :

Possible realisation:

Cu^{3+}/Cu^{2+} mixed valence state



C.H. Höck, R. Nicksch and H. Thomas (1983)

<u>Perovskites</u>, first Ni^{3+} (2 years)

$La^{3+}Cu^{3+}O_3$: Metal, $\chi^{(T)}_{pauli}$

$Ba^{2+}_x La^{3+}_{5-x} Cu_5 O_{5(3-y)}$: Metal Michel & Raveau 1100 °C 1985

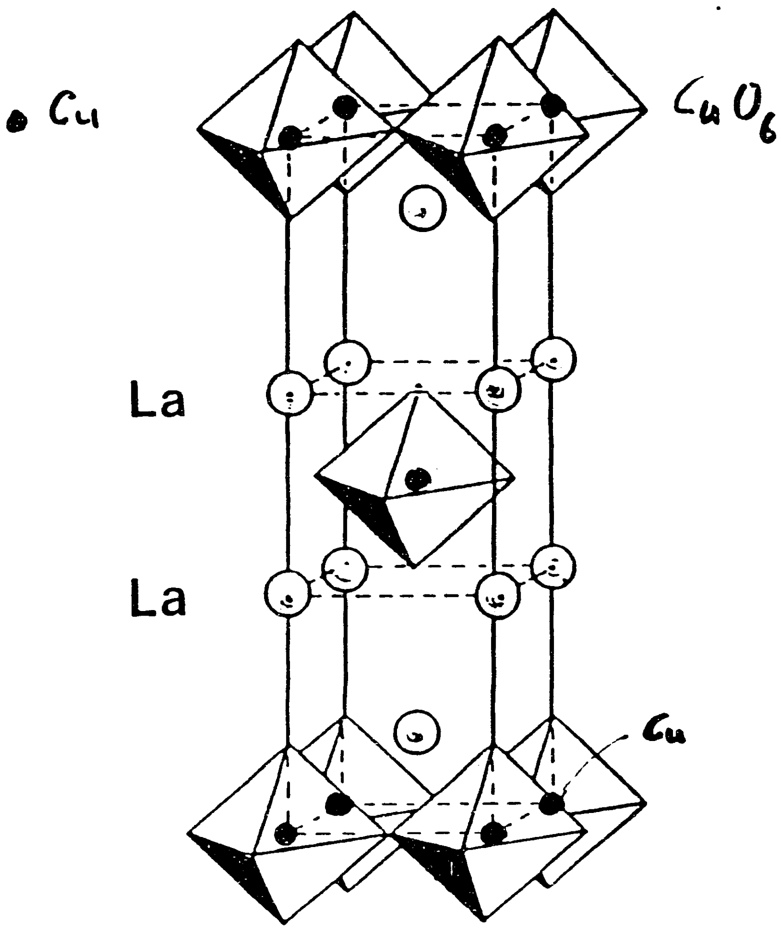
mixed valent <u>in</u> perovskite

Preparation in Rueschlikon:

- Coprecipitation from aequeous solutions of oxalates, then
- solid state reaction by heating 900 °C
- measured sinterd pills $\rho(T)$

<u>J.G. Bednorz</u>

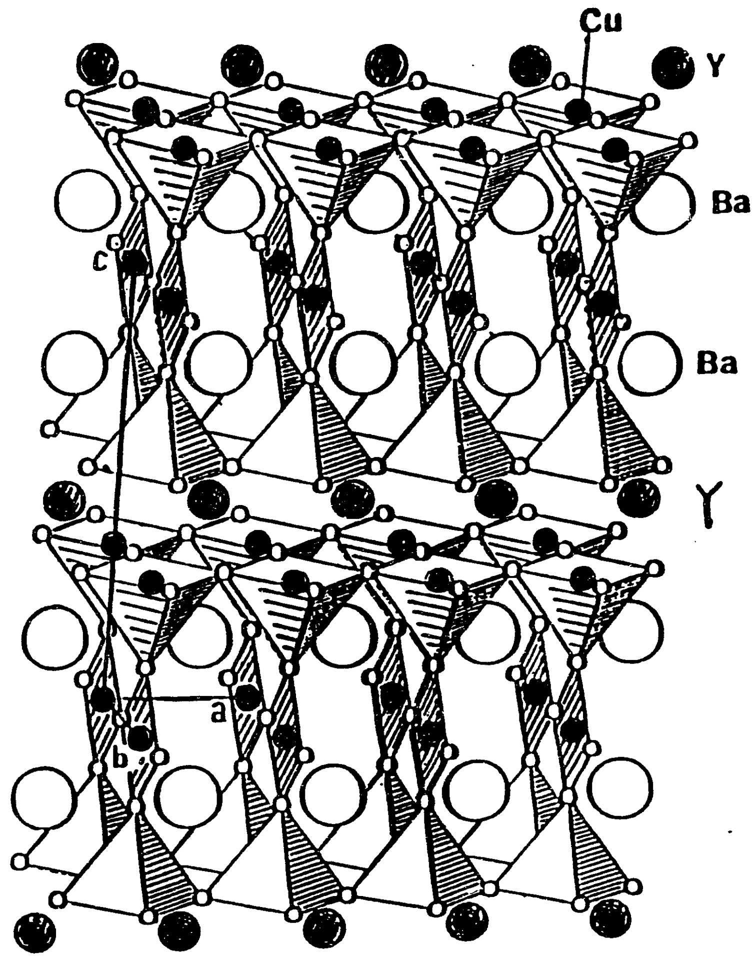
FIGURE 12



$Ba^{2+} \rightarrow$ $\overset{3+}{La_2}\overset{2+}{Cu}O_4$ $\leftarrow Sr^{2+}$

35 K

Müller /Bednorz, Jan. 86



$YBa_2Cu_3O_7$

92 K

Houston, Febr. 87

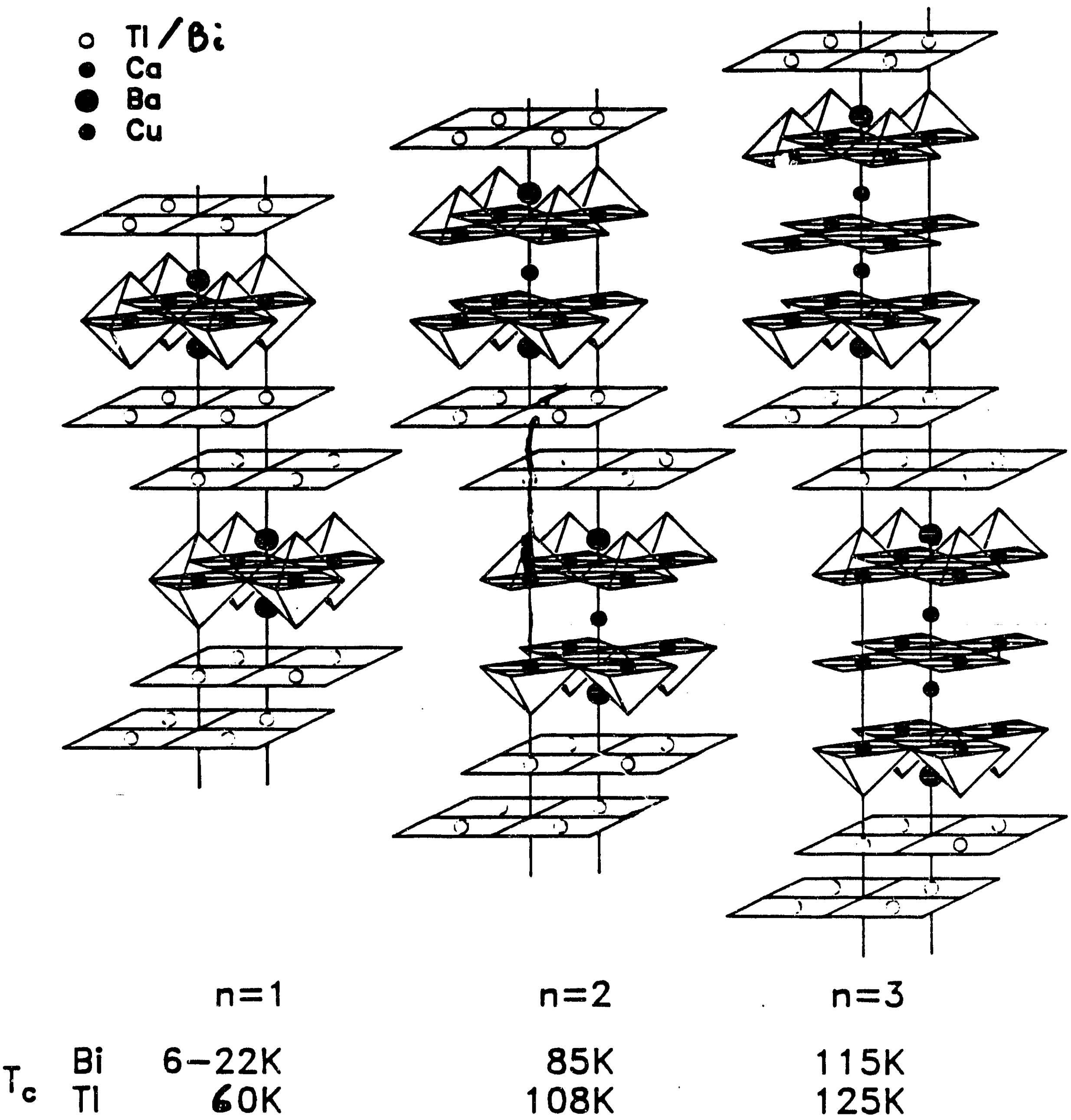


FIGURE 14

They are

Layered Cu-Oxydes:

a new class of Superconductors

FIGURE 15

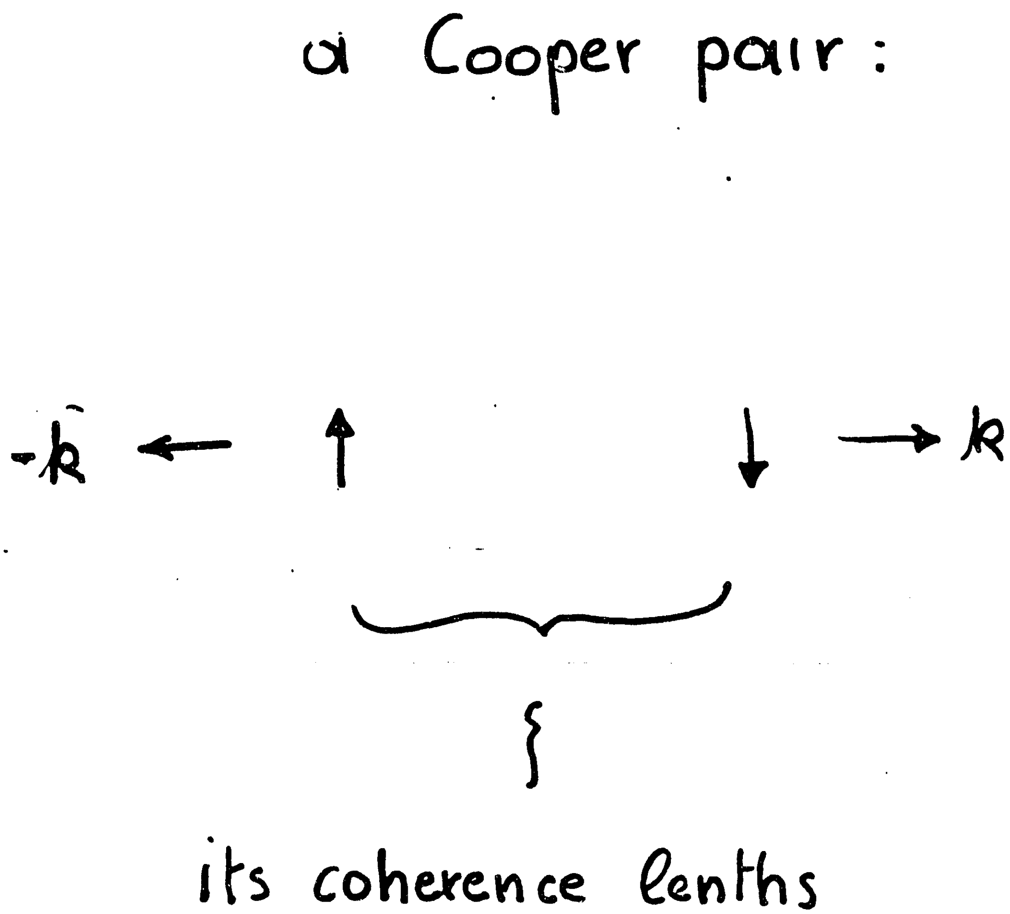
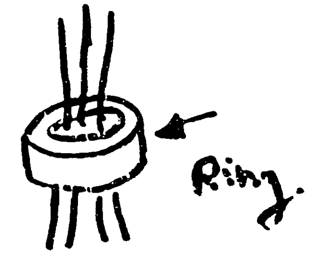


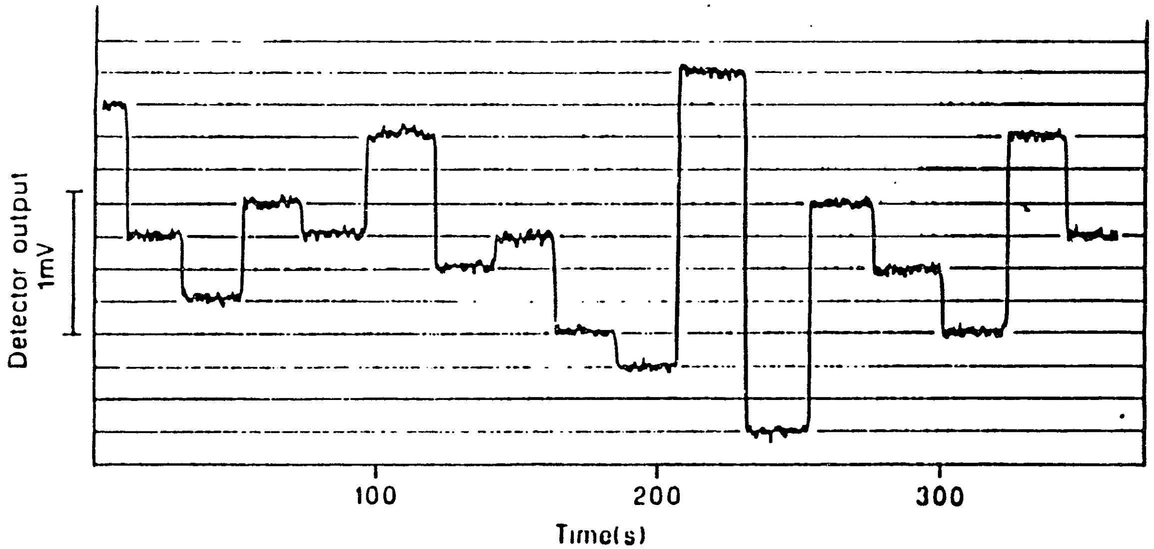
FIGURE 16

C. E. Gough et al. Department of Physics, University of Birmingham

Magn. Flux

$\phi = n\phi_0$





Output of the r.f.-SQUID magnetometer showing small integral numbers of flux quanta jumping in and out of the ring.

Flux quantisation : $\phi_0 = hc/2e$

F. London (1952) : q

Critical Fields, H_{c2}

H_{c2} : about 100 Tesla = 1 Megagauss

$H_{c2} = \Phi_0 / 2\pi\xi^2 \qquad \phi_0 = 2 \times 10^{-7}$ gauss cm^2

$\rightarrow \quad \xi \approx$ lattice constant

measured $\begin{cases} \xi_c \simeq 2\text{-}4 \text{ Å} \\ \xi_a \simeq 20 \text{ Å} \end{cases}$

IBM - group

Stanford - group

FIGURE 18

these materials are

hole superconductors

the oxygen 2p-orbitals are essential for the formation of Cooperpairs.

$$Cu^{3+}-O^{2-} \rightarrow Cu^{2+}-O^{-}$$

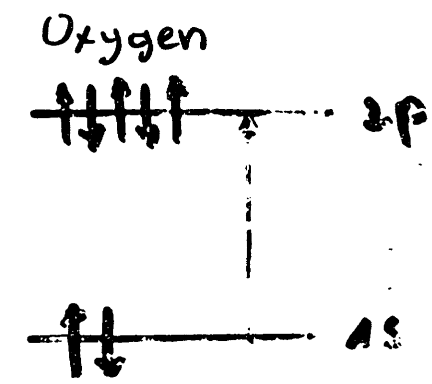
$[Cu-O]^{+}$: notation

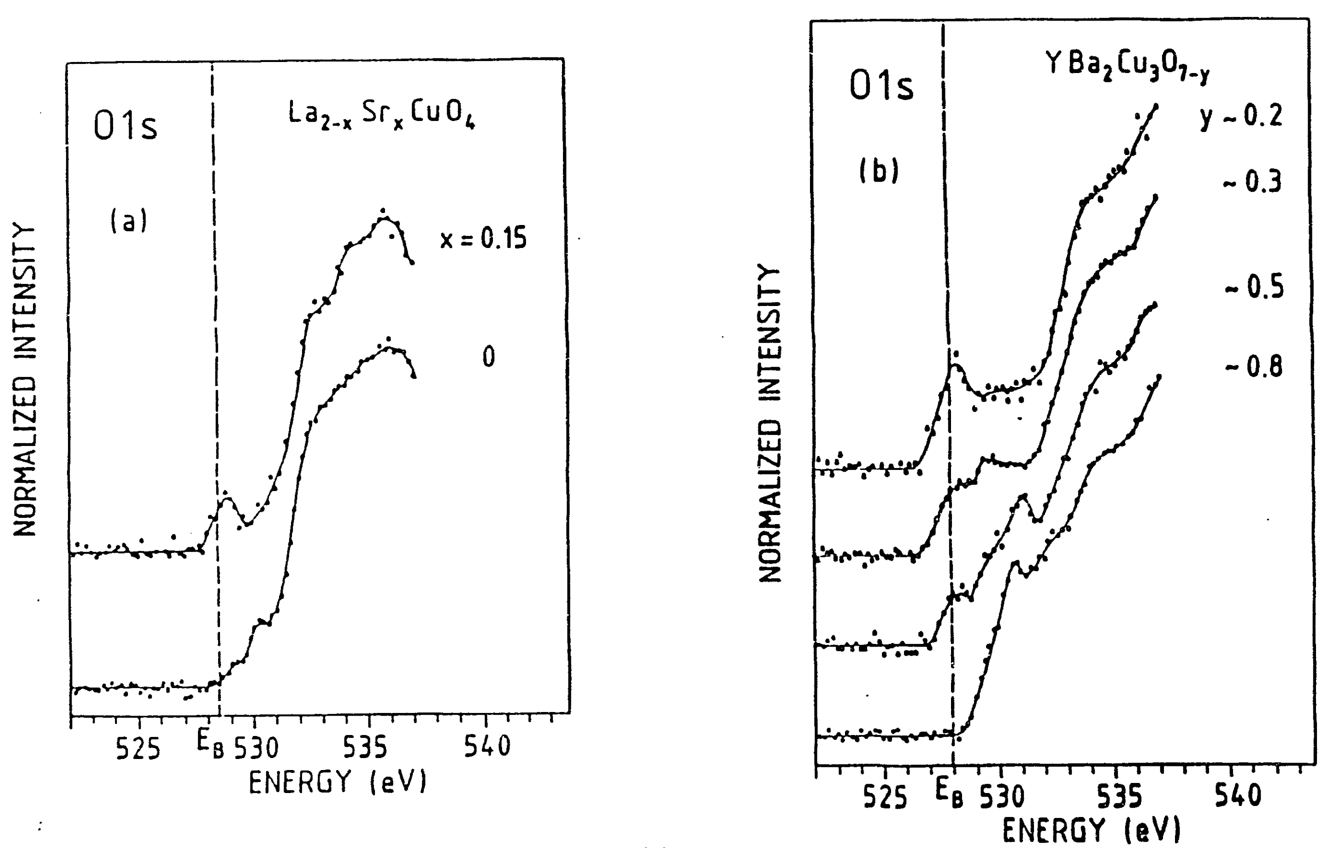
$3d^{8}\ \underline{2p}^{0}$ $\qquad$ $3d^{9}\ \underline{2p}$

FIGURE 19

M. Nücker et. al. K.F.A. Karlsruhe

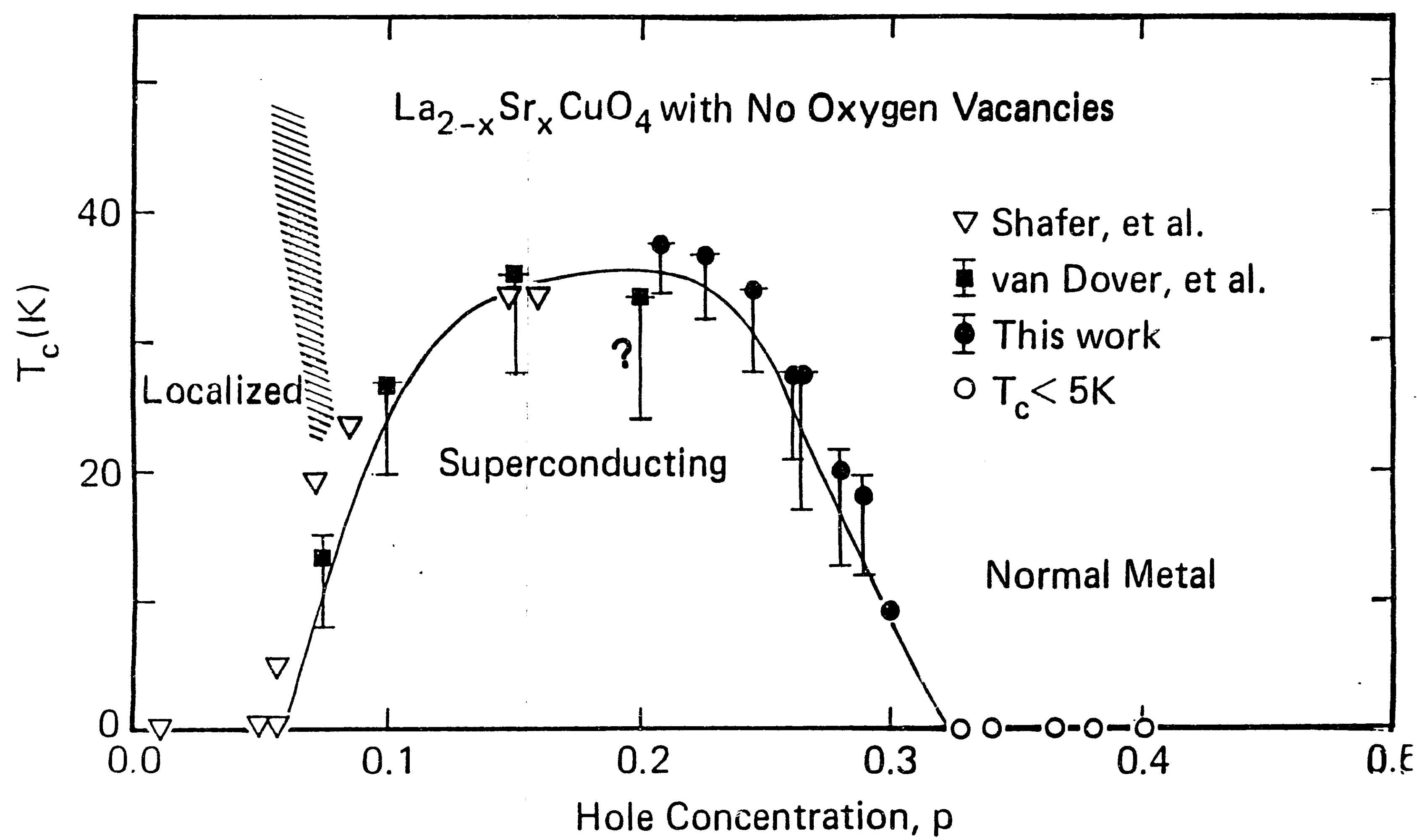
Core-level excitations of oxygen 1s electrons by electron energy-loss spectroscopy.





E_B : binding energy.

FIGURE 20



J.B. Torrance et al

965 · IBM · 18

965 · IBM · 18

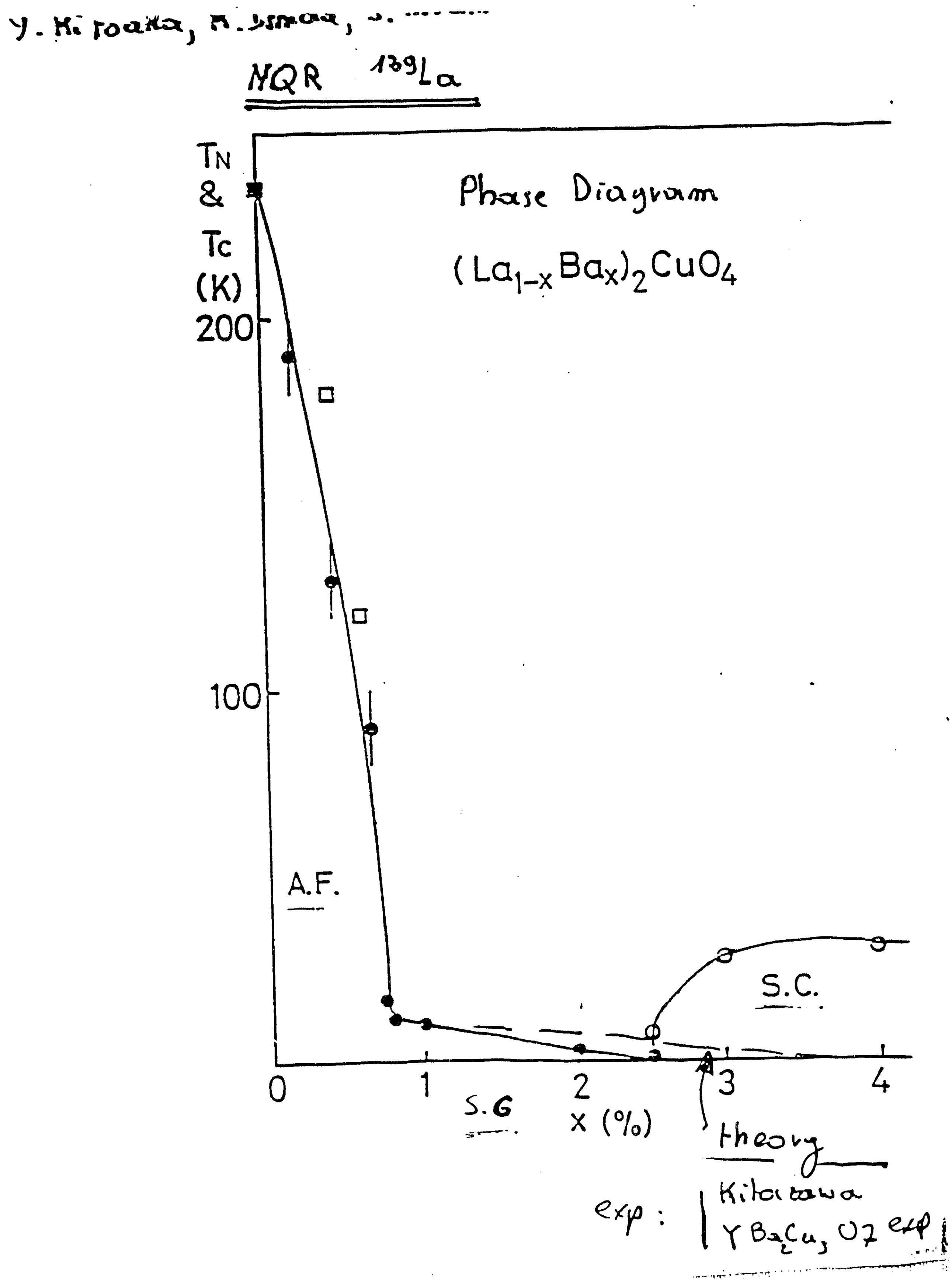
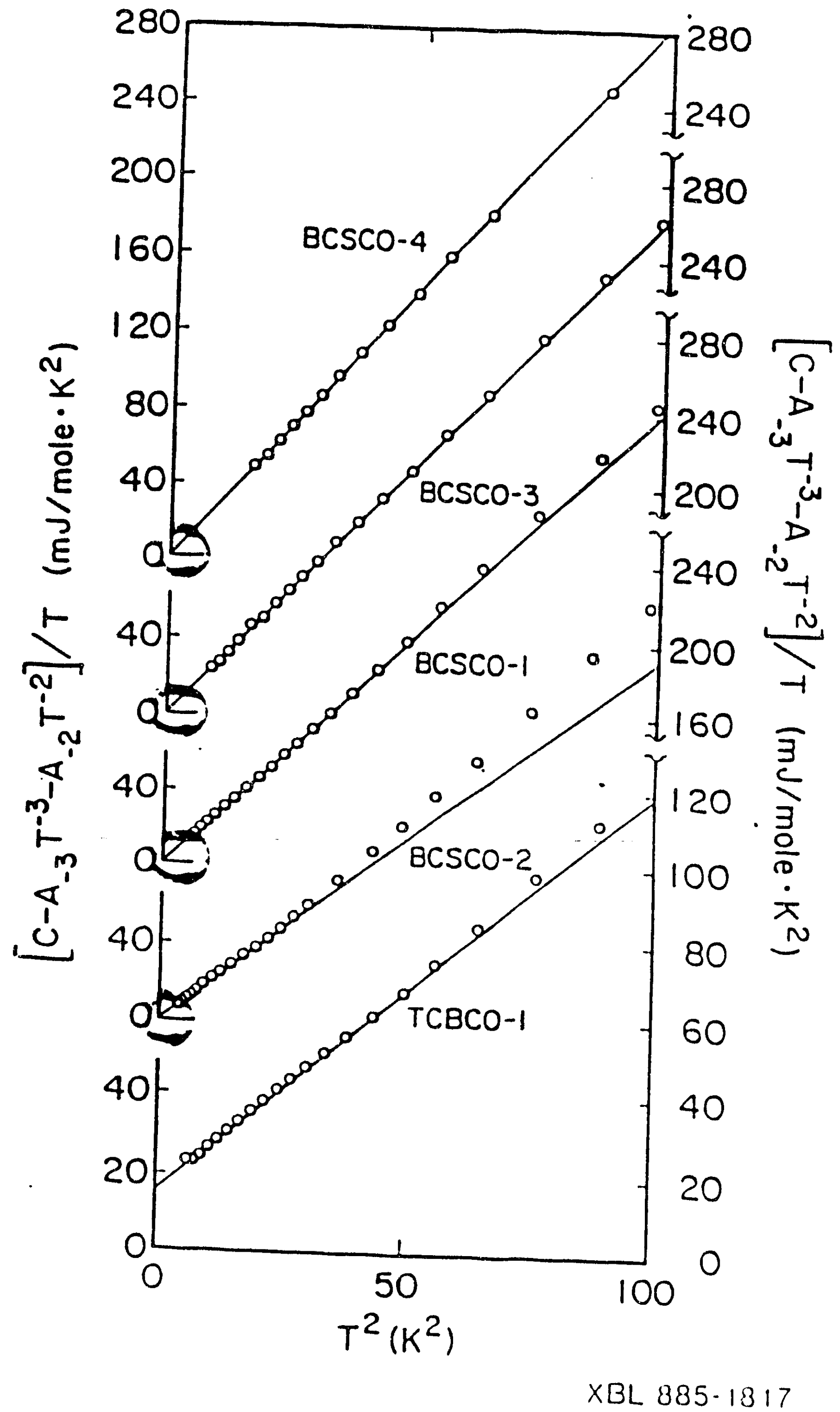

Y. Kitaoka, K. Ishida, ...
NQR ^{139}La
Phase Diagram
$(La_{1-x}Ba_x)_2CuO_4$
TN & Tc (K)
200
100
A.F.
S.C.
0
1
2
3
4
S.G
x (%)
theory
exp : Kitaoka
$YBa_2Cu_3O_7$ exp

FIGURE 22

Specific Heat in Bi-Ca

no linear te



XBL 885-1817

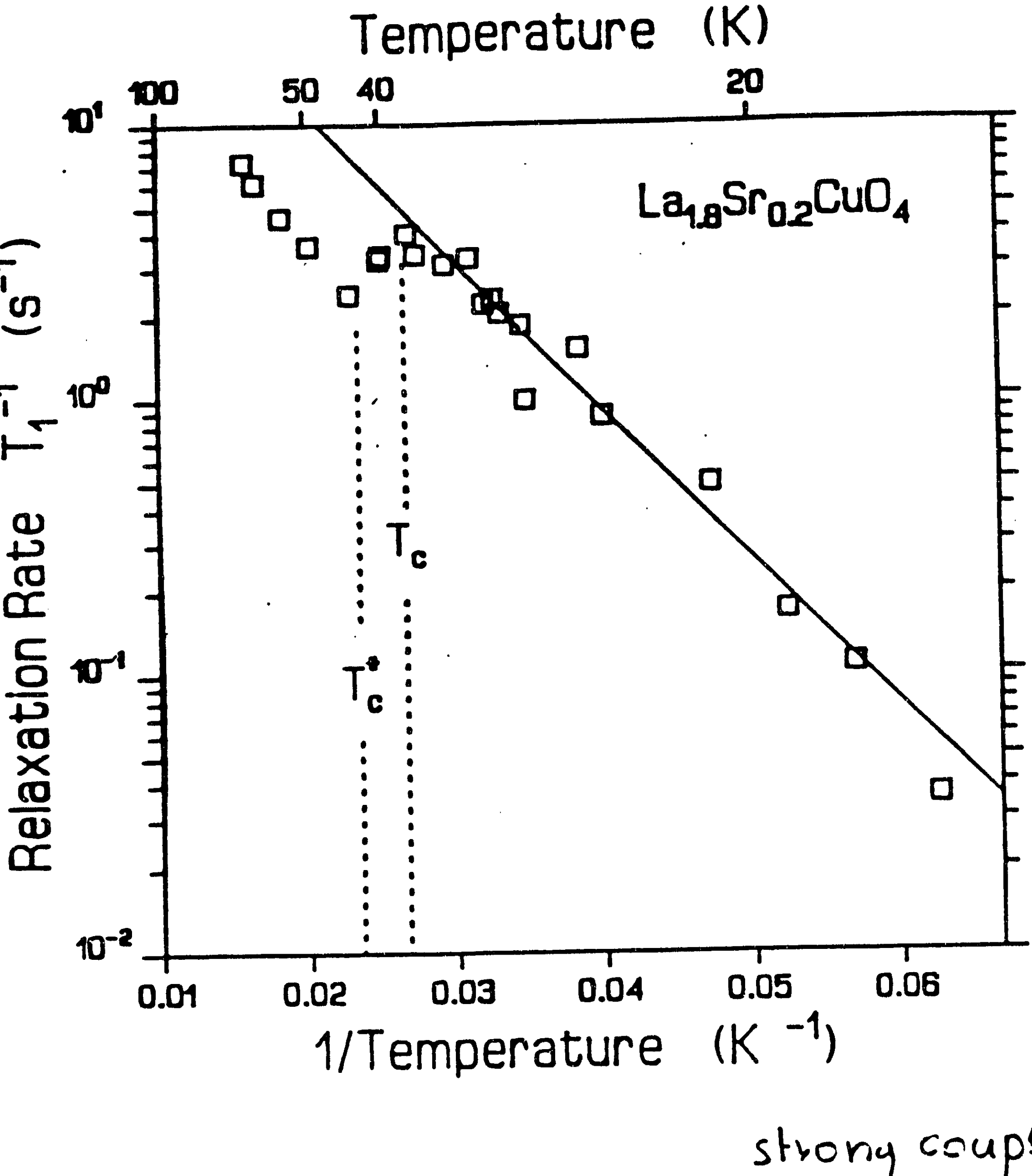
Fig. 4

R.A. Fisher et.al Berkeley

FIGURE 23

La^{139} relaxation rate $1/T_1$

M. Mehring group Stuttgart



strong coupling:

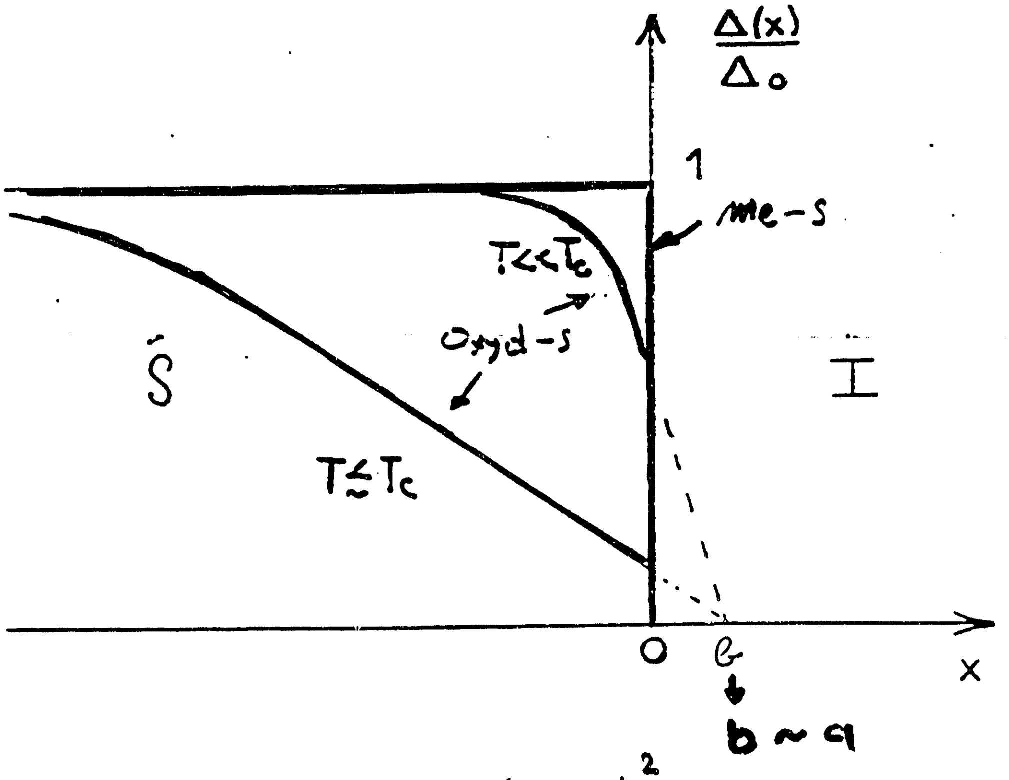
$$1/T_1 \propto \exp \Delta/RT \qquad \frac{2\Delta}{RT_c} = 7.1$$

FIGURE 24 Fig. 2.8

Pair potential Δ(r) in short coherence lenth superconductor

G. Deutscher and K.A. Müller

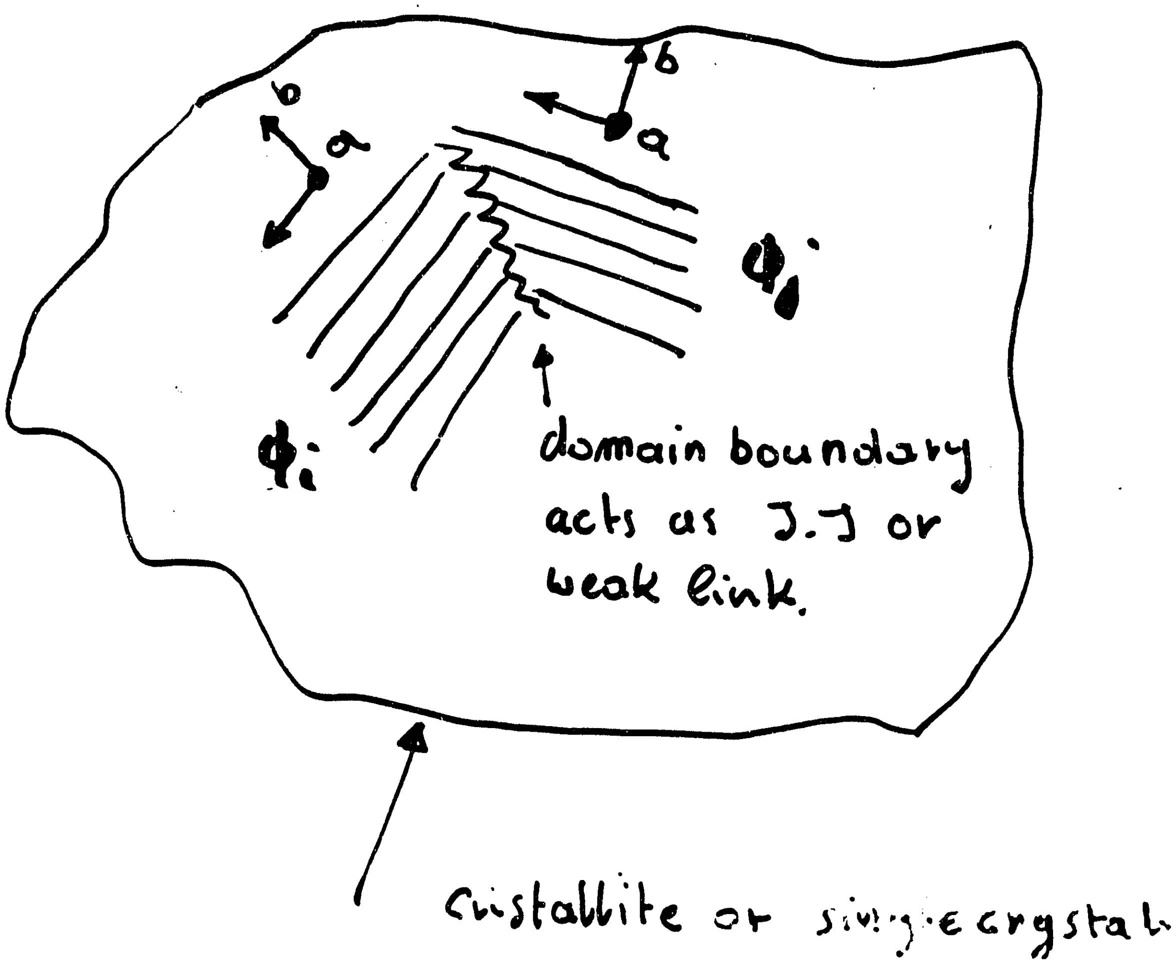
relevant for SIN & SIS junctions



near T_c : $J_c \propto (T_c - T)^2$ now observed

not $(T_c - T)$

FIGURE 25



→ Superconductive Glass-State

$$\hat{\mathcal{H}} = \sum_{ij} J_{ij} \cos(\phi_i - \phi_j - A_{ij})$$

$$A_{ij} = H\, x_{ij}$$

FIGURE 26

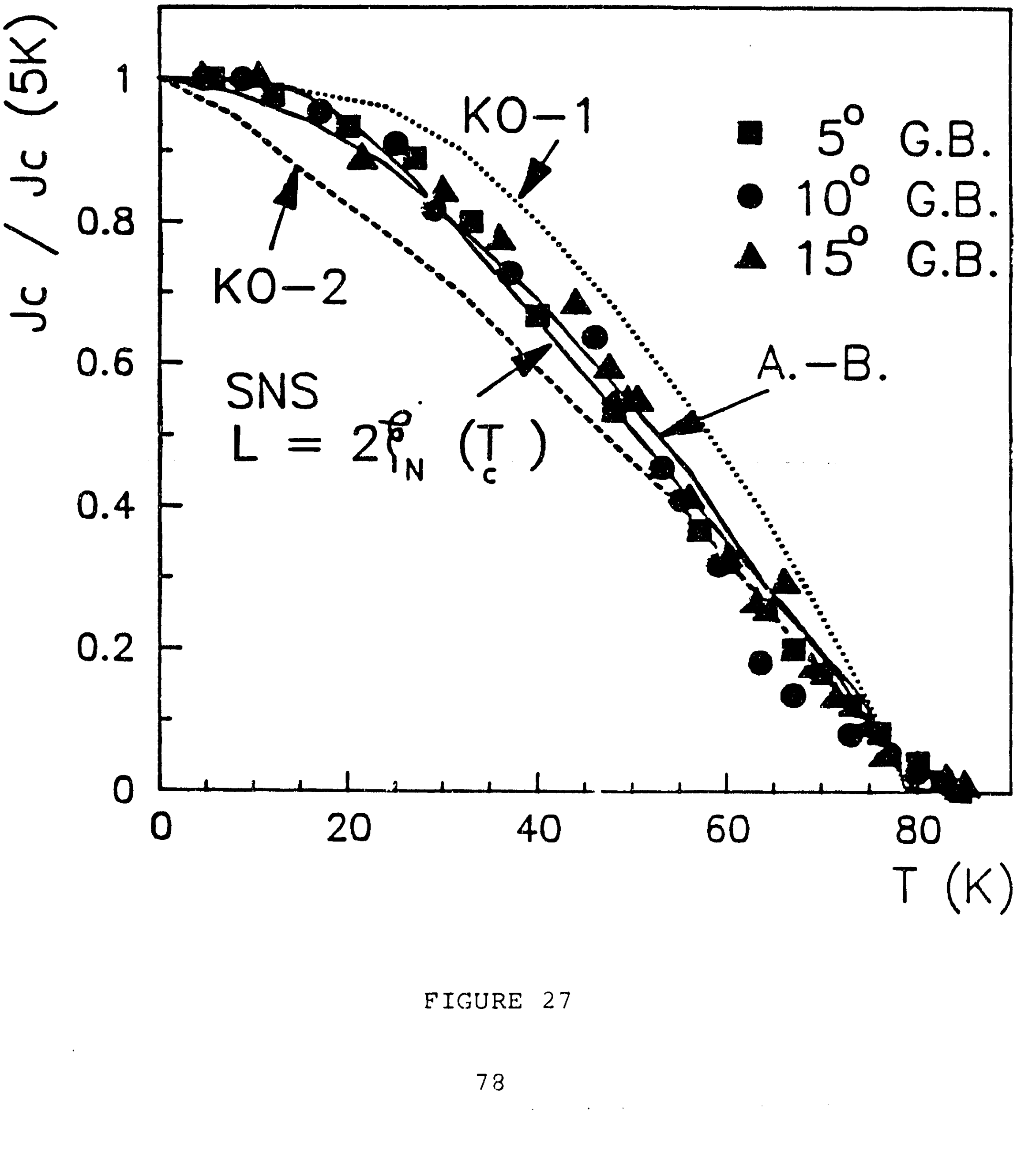
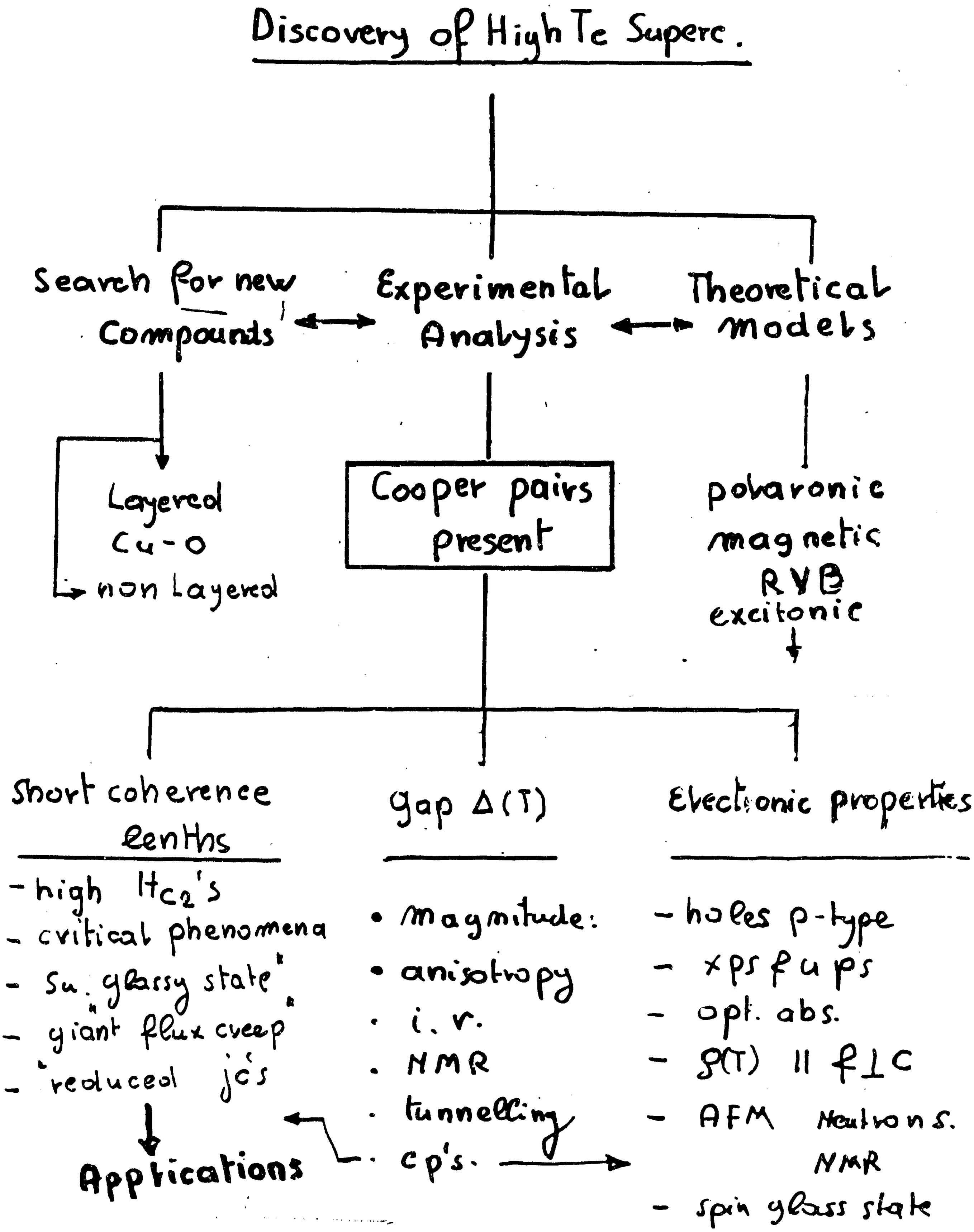


FIGURE 27



Discovery of High Tc Superc.
Search for new Compounds
Experimental Analysis
Theoretical models
Layered Cu-O
non Layered
Cooper pairs present
polaronic
magnetic
RVB
excitonic
Short coherence lenths
Gap Δ(T)
Electronic properties
- high Hc2's
- critical phenomena
- Su. glassy state
- giant flux creep
- reduced jc's
Applications
• magnitude:
• anisotropy
. i.r.
. NMR
. tunnelling
. cp's.
- holes p-type
- xps & ups
- opt. abs.
- ρ(T) ‖ & ⊥ c
- AFM Neutrons.
NMR
- spin glass state

FIGURE 28

INTRODUCTION OF
DR. EDWARD TELLER, HONORARY DIRECTOR
INSTITUTE OF FOR TECHNOLOGY AND STRATEGIC RESEARCH

VADM John T. Parker

My pleasure this afternoon is to introduce the lunchtime lecturer, Dr. Edward Teller. You may wonder why I was asked to introduce Dr. Teller on this occasion. Certainly not because of my part in his work, or my part in the announcement that we are celebrating today, because on the day of the announcement I was in the second grade. Nor was I asked on the basis of my personal relationship with Dr. Teller: I met him for the first time only a few months ago.

Nevertheless, the honor has come to me to introduce a man whom most of you know better than I do. You know of his brilliant history. Some of you have worked with him in the offices and classrooms where he forges a vigorous future. He was here at the University as a faculty member at the time of Niels Bohr's announcement.

Many illustrious names are associated with the development of nuclear weapons, but among those names, Dr. Teller's has always had special significance because he has been, to quote *U.S. News and World Report*, "a powerful influence on the nation's defense elite since 1939, when he played a role persuading Roosevelt to develop the atomic bomb."

In that eventful year, Dr. Teller accompanied Eugene Wigner and Leo Szilard on a visit to Albert Einstein, which precipitated the famous letter resulting in the formation of the Manhattan Project.

I should point out here that after World War II, the Department of Defense portion of the Manhattan Project became the Armed Forces Special Weapons Project and then the Defense Atomic Support Agency. Now, the great grandson of the Manhattan Project is the Defense Nuclear Agency. Hence my presence here today.

The agency is privileged to carry on work which lessens the risk of nuclear combat by assuring that we have a strong nuclear deterrent. We work to sustain the vitality of the deterrent given us by Dr. Teller and others.

You might say that if it were not for Dr. Teller and others, I would probably not have a job today.

Dr. Teller is probably best known for his part in the development of the hydrogen bomb. Again, his contribution was much more than his science. To quote the *Encyclopedia Britannica,* "his stubborn perseverance in the face of skepticism and even hostility from many of his peers played the major role in bringing that project to a successful completion."

Yet through the years he has worn many hats: as a professor, science adviser to presidents, and distinguished guest lecturer at several universities. He was instrumental in the creation and founding of Lawrence Livermore National Laboratory and has served as the Laboratory's director.

He has contributed countless ideas to young research physicists, and has been a source of inspiration to all of his students. He has been an increasingly influential spokesman on nuclear concerns: not only nuclear weaponry but, also, the peaceful uses of nuclear energy and the development of alternative energy sources. He was an ardent champion, for example, of Project Plowshare, the peaceful use of nuclear explosions.

In addition to his doctorate from the University of Leipzig, Dr. Teller holds numerous honorary degrees from universities worldwide. He is Senior Research Fellow at the Hoover Institution of War, Revolution and Peace at Stanford University, and Professor Emeritus of Physics at the University of California at Berkeley.

He has been both official and *ex officio* consultant to Presidents and the Congress for fifty years. The world has, indeed, been fortunate to have enjoyed forty years without a nuclear conflict. The potential for such a holocaust has been upon us ever since the Soviets learned to make nuclear weapons. Many visionary people set about to try to ensure that nuclear war would not happen. Dr. Teller has been at the forefront of this group, constantly arguing for national strength and preparedness as the deterrent to nuclear disaster.

More recently, Dr. Teller has given his support to the nuclear-driven x-ray laser as a promising concept for achieving the goals of the strategic defense initiative, and thereby this dynamic man of many parts earned again from *U.S. News and World Report* the envious statement, "Dr. Teller is still controversial after all these years."

Of one thing we can be certain: Dr. Teller's career during the nuclear age has greatly impacted on all of our lives, and will continue to influence those of future generations.

LUNCHEON ADDRESS

TOWARD A MORE SECURE WORLD

Dr. Edward Teller

I intended to talk exclusively about the future; but in the last few hours, I have heard so much about the past that I cannot entirely resist adding a few comments about it, too. In particular, I want to mention the man who played a very great role in getting these conferences started, my very good friend, Joe Gamow, who got me to Washington in the first place.

Together, we looked into all kinds of peculiar things. The topic of our conference in 1937 was, I think, the energy source of the sun. We didn't make any headway at the conference, but I made one important contribution: I persuaded Hans Bethe to come. He was not interested in that strange subject, but hardly more than a couple of months later, he had solved the puzzle. A few years later, he got the Nobel Prize for that work.

Gamow usually called me early every morning--about 9 o'clock--with a brand new idea about some aspect of physics. In ninety percent of the cases, the idea was wrong; but in ten percent of the cases, it was excellent. I found it very pleasant to serve as Gamow's filter, because he had a wonderful property: he did not mind being wrong. The important thing for him was that the idea was new. It did not have to be right. That was really very nice.

The evening before the conference in 1939, Gamow called me and said, "Bohr has arrived, and he has gone crazy. He says that uranium splits." For a minute, I thought that was a little peculiar, but it very quickly became clear that the idea resolved the problem of all the unexplained elements. Fermi was supposed to have made a couple of transuranic elements by bombarding uranium, but others showed up and the list grew longer than all the arms of an Indian goddess put together.

When the conference opened, we were supposed to talk about low-temperature physics. But Bohr had something to say first, and his remarks were not exactly on low-temperature. We listened to him. It was the news of the century. But after about half an hour, we got started on the proper topic of the conference.

That evening, Merle Tuve asked us to come to the Carnegie Institute of Terrestrial Magnetism. In the intervening few hours, he and some of his colleagues had reproduced the experiment that showed the fission fragments. Fission had been there, waiting to be found for a considerable time, though the facts had stared in our faces.

During the conference, Mici and I were very busy: we were in charge of the social affairs. We had had people in our home continuously throughout the conference and were quite exhausted by the time it ended. But one-half hour later, there went the phone. "Szilard is here. I am at Union Station. Will you come and pick me up?" Leo Szilard had not been at the conference; but when we picked him up, I heard in detail all about the consequences of fission, which Szilard had been thinking about for much longer and in much greater detail than anyone else.

The following summer I was teaching at Columbia, where Szilard was working. Szilard was very ingenious about everything else, but he did not know how to drive a car. Furthermore, he had the strange idea that I was a good driver. He asked me to give him a lift to Long Island to see Einstein, and I agreed. When we got there, Szilard pulled a letter out of his pocket, and Einstein signed it. The letter was addressed to President Roosevelt. That was the beginning. What it led to, all of us know.

Now let me turn to the future. In our end-of-the millennium atmosphere, many of us are worried about the end of the world. I want to talk about another worry. It is connected with science and technology, and I can imagine that it will have a happy ending.

Just before I was half a year old, on the 30th of June 1908, a meteorite arrived at a spot about 300 miles northwest by north of Lake Baykal near the Tunguska

River, which is a tributary of the Yenissey River. Nobody was in the immediate vicinity.

The meteorite approached the earth's surface from the southeast on a very oblique path. It exploded a few degrees south of the Arctic Circle. No portion of the meteorite has ever been found. In a region of tens of square miles the forest was laid flat. The event was registered on micro-bargraphs throughout the world.

Very good Soviet research work has shown that the meteorite was a stony meteorite, a chondrite, that had a mass of a few million tons. Very small pieces of it were distributed around the world. They were deposited, among other places, near the South Pole; where fragments, found in the glaciers and dated to 1908, have been analyzed. The fragments show an iridium concentration a thousand times greater than is found in terrestrial materials.

Iridium is the noblest of the noble metals. In the early stage of the earth, oxygen combined with the easily oxidized metals. Roughly half of the iron was oxidized; and the other half, which stayed metallic, now forms the liquid core of the earth. All the metals that were not easily oxidized, including iridium, were dissolved. Hardly any iridium was left in the mantle. Iridium is not similarly removed, however, in the formation of a meteorite; so its presence is a prime indicator of a meteorite or a meteorite fragment.

The Tunguska meteorite, weighing a few million tons, came in on a very glancing orbit at an unknown velocity. Usually, that type of meteorite travels between 15 and 20 miles per second. An object 3,0 feet across moving with a velocity much higher than the velocity of sound produces a high pressure ahead of it. When the meteorite reached an altitude of about five miles above the earth's surface, the pressure of air ahead of it, which had heated the surface of the meteorite to luminosity, became too great. The meteorite broke into fragments, which were then individually heated and vaporized. In that process, they produced an explosion of a force equivalent to 12 megatons of TNT.

Had the meteorite's force been spent a few thousand miles farther to the west, many people would have been killed. Had that been the case, we would still be reading about the Tunguska meteorite today. But fortunately, that did not happen.

How frequently do big meteorites arrive? Some reach the surface of the earth. The moon, not protected by an atmosphere, is full of meteorite craters. On earth we have but a few. Also on earth ancient craters erode. On the moon, they are preserved.

There is a meteor crater in Canada that is seventy kilometers across. The details of the crater are no longer readily visible. The center of such craters usually is somewhat elevated. Today, the center of the Canadian crater is surrounded by a belt of water, nearly seventy kilometers across. That meteorite, which struck approximately 200 million years ago, was ten times the size and one thousand times the weight of the Tunguska meteorite.

One meteorite that arrived sixty-five million years ago has become famous. The son of my friend Luis Alvarez discovered its occurrence from the pattern of iridium deposits throughout the world. That meteorite is estimated to have been ten miles across and about a million times as heavy as Tunguska. It may have led to the extinction of the dinosaurs.

There is no exact information about the distribution and frequency of occurrence of such meteorites. However, generally speaking, the bigger ones come less frequently. Meteorites ten times heavier than Tunguska occur perhaps one-fifth as often. My friends in Livermore looked into the question of how much damage meteorites throughout the world cause each year and found it to be somewhere between 10 and 100 million dollars. I want to talk about what we can do about preventing that damage. Even ten years ago, no real good options existed; but now there is one.

Astronomic equipment can include arrays of photoelectric cells where each pixel in the cell is just a few microns across. What is new and is steadily improving are the associated computers. If such a combination were put up in space, meteorites of considerable size could be identified more easily by at least a factor of ten, perhaps by a factor closer to a hundred.

In order to see a Tunguska-sized meteorite two or three weeks ahead of time, we would have to watch objects of the twenty-third magnitude. That means that the object is perhaps ten million times less luminous than the faintest star visible to the naked eye. Furthermore, such an object could be anywhere in the entire sky. What is amazing is that today the needed surveillance can probably be done at a cost well under 100 million dollars.

Of course, all those faint objects could not be catalogued. But the objects of interest are those that are very close to earth, that have an unusually big parallax, and that move in relation to the background of the other stars. The task is most formidable because objects approaching us are those that appear to move the least. But the problem can be addressed by putting the telescope in orbit, because the orbiting telescope moves a few miles a second, which therefore makes it possible to distinguish our object that moves with respect to it--even if the object is coming straight at the earth.

Knowing what to look for, the necessary observations can be made. With one good observation post we would find out quite soon how many meteorites approach the earth that are as big as the one that blew up over Tunguska.

In the case of meteorites, we must find out what is going on: not by using bird watchers, but by using electronic equipment and computers. They are already fabulous and are becoming year by year more fabulous still. If that is done, we shall learn not only the frequency with which meteorites approach the earth, but practically free of charge we shall learn a hundred times as much as we now know about nova stars, particularly about super novae. Most of the time, the sky is viewed through telescopes of a very narrow field of view; there is no general survey. We have learned that the sun changes its radiation on an eleven-year cycle. We have the rudiments of information about perhaps a hundred other stars, but for most gently changing stars we don't know their variability.

Improving our observational equipment would increase our knowledge about the whole science of the stellar atmospheres. The computer could be programmed to attend to almost all variable objects, or to any specified class of variable objects. Information about cepheid variables, quasars, or seyfert galaxies would become greatly increased once we had an appropriate observation station.

It is much more difficult to decide what we might be able to do about the meteorites we shall discover. I am not certain that my proposal is the right answer, but doing something is obviously of interest to everyone on the planet. I hope that any program to limit damage from meteorites will be undertaken not by the United States alone, or together with just the other super power, but that such a program will be the work of all nations, for everyone's benefit. Here is a topic that is clearly of universal interest and universal importance.

The program, of course, would involve sending up something to meet and destroy or deflect the meteorites: For example, a nuclear bomb or a non-nuclear device such as those developed under the Strategic Defense Initiative. A nuclear weapon must not collide with the meteorite, because its structure would be destroyed and the nuclear reactions would never get going. But if the explosion were set off just a few feet short of collision, a few percent of the bomb's energy would be coupled into the meteorite. In that case, a meteorite of the approximate mass of Tunguska would break-up into small pieces, which would become completely harmless. The few pieces that reach the earth at all will burn up in the high atmosphere.

A meteorite one million times bigger than Tunguska--one the size of the Alvarez meteorite--cannot be blown up. But it can be noticed when it is much farther away, about a year ahead of the collision. All that would be needed would be to give it a little sidewise shove. That could be done by exploding a hydrogen bomb a short distance from the surface at the side of it. Blowing a crater half a mile in diameter in the meteorite would reduce its mass only by about one-tenth of one percent. This could produce a sidewise movement big enough to deflect it and avoid a collision.

The new art of breaking-up or deflecting meteorites could be developed year to year, if we wished, by practicing on objects that come as close as the moon.

We could safely experiment, and when a real danger should occur, we would than be in a position to avert the damage.

It is unlikely that meteorites several hundred times bigger than the Alvarez meteorite can ever be sufficiently deflected. Fortunately, the Alvarez meteorite is apparently the biggest object that has ever hit the earth during its four and a half billion years of existence.

In the geological record, there is evidence of periods of mass extinction of living species. These may be connected with the arrival of giant meteorites, which can cause a thousand times more destruction than a nuclear war. Fifty years ago we have made the first step toward nuclear power. Fifty years from now we could be well underway to use this power for universal protection.

APPENDIX

50th ANNIVERSARY COMMEMORATING THE FIRST PUBLIC ANNOUNCEMENT OF THE SUCCESSFUL TEST OF FISSION

ATTENDEES

Dr. George Abraham
Naval Research Laboratory

Mr. Steven Adrian
Graduate Student
Physics Department
The George Washington University

Mr. John F. Ahearne
Resources for the Future, Inc.

Mr. Abdallah Al-Dabbas
Graduate Student
Department of Engineering Administration, The George Washington University

Dr. David Aldrich
SAIC

Mr. William O. Allen
SAIC

Mr. Alastair Allcock
Counselor for Science, Technology and Energy
Embassy of the United Kingdom

Mr. Edward Ardery
Potomac Electric Power Company

Mr. William. M. Arendale
U.S. Department of Energy

Mr. Barry Ashby
President
Ashby & Associates

Mr. Nikolay I. Avdoshkin
Third Secretary (Science & Technology), Embassy of the Union of Soviet Socialist Republics

Dr. Marcel Bardon, Director
Division of Physics
National Science Foundation

Dr. Ralph E. Beatty, Jr.
Alumnus - The George Washington University

Dr. Eric Beckjorg, Director
Office of Nuclear Regulatory Research, U.S. Nuclear Regulatory Commission

Dr. James W. Behrens
Center for Basic Standards
National Institute of Standards and Technology

Mr. Everett H. Bellows

Professor Simon Y. Berkovich
Department of Electrical Engineering and Computer Science, The George Washington University

Dr. Ted G. Berlincourt
ODUSD (RANT)
The Pentagon

Prof. Berry N. Berman
Physics Department
The George Washington University

Dr. Frederick Bernthal
Assistant Secretary for Science & Technology
U.S. Department of State

Prof. Giorgio V. Borgiotti
Department of Electrical Engineering and Computer Science, The George Washington University

Mr. Albert Bottoms
Institute for Technology & Strategic Research
The George Washington University

Mr. Ray Bowers
Editor and Publications Officer, Carnegie Institution of Washington

Mr. Scott Bradley
Kaman Sciences Corporation

Prof. George Brier
School of Engineering and Applied Science
The George Washington University

Mr. Luis Brooks
Student
T.C. Williams High School

Dr. Louis Brown
Department of Terrestrial Magnetism, Carnegie Institution of Washington

Dr. John L. Burnett
U. S. Department of Energy
Division of Chemical Scien

Mr. Allan D. Carlson
National Institute of Standards and Technology

Ms. Rebecca Caress
Graduate Student
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