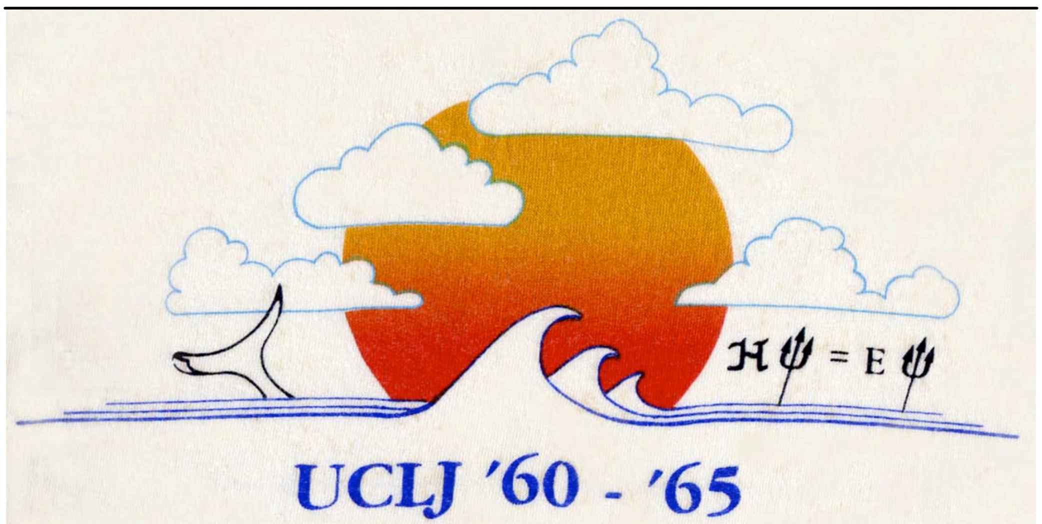
Proceedings of the

LA JOLLA PHYSICS SYMPOSIUM



University of California, San Diego
September 6 – 8, 1985

Physics 25th Celebration/Reunion

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LA JOLLA PHYSICS REUNION/SYMPOSIUM

University of California, San Diego

September 6 - 8, 1985

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PREFACE

by Thomas N. Delmer

This is a collection of papers presented and photos taken at the **La Jolla Physics Symposium** held on September 6 - 8, 1985 in conjunction with a **Reunion** of the people associated with the Physics Department of the University of California at San Diego during the academic years of 1960 to 1965. Historical pictures of the campus and the first five years of graduate students also appear. Those were the founding and formative years of the Department. Several students from that time were asked to give talks on their work since leaving the UCSD. When possible, their thesis advisors introduced their talks, otherwise, someone who was familiar enough with them to make a few incisive remarks was asked to give an introduction. The Symposium opened with a talk that described the beginning of the Department and closed with a panel discussion of the future of the University of California, the San Diego campus, as well as the Physics Department.

The response was positive: about thirty percent of all those identified as being associated with the Department in those early days came to the Symposium. The talks, which were informal because of the broad range of subject matter and interests of the audience, showed an emotion that indicated that formation of the Department was a personal and moving experience for all involved. This was not a complete surprise; the choice of the first five years as the central theme, the limitation to those years, was made because after the mid '60's, the University had grown and the calcification mentioned by Greg Benford had begun to set in. But, the degree to which the feeling was universal and deep could not have been known beforehand. This remark is not just sentimentality. As Keith Brueckner describes, the University was started from the top down, from research faculty to graduate students and in 1965, to undergraduate students. The beginning of the Department consisted of a small group located at Scripps Institution of Oceanography in La Jolla (hence the UCLJ of the logo). The small size and the accompanying closeness made the experience unique. Changes took place rapidly. As Karl Eckart once said, "It was not that things were so exciting then, but that things have been pretty dull ever since." The skit, *Physics on the Beach -- Scape* written by Mike Green, Arnie Sherwood, and others, included here after the papers, qualifies the excitement.

The importance of the early days of the Department as a personal experience and a method of starting a University became evident at the Symposium. Remember that physicists have a strong tendency to be arrogant. Read these pages and see how suppressed the arrogance was in the people recalling the events; you will see that even the most self important were in awe of what they experienced. It was not originally intended to produce a Proceedings since the talks were to be informative rather than of archival value. But as a whole, they painted a picture of the nascence of an important entity and documented the technical strength of the Department. It is the importance of the composite, the whole of the talks and photographic record, that make these Proceedings of archival value.

Nevertheless, this document is of most interest to those readers who were part of the first five years. Other readers will credit that the beginning was unique, but may question the importance and quality of the Department and its product, i.e., the graduates during the years 1960 - 1965. The best way to judge the quality of the graduates is to read this document and form your own opinion; the introductions and talks have many clues to help make a judgment. The reader who is not interested in the technical content can read the opening talk by Keith Brueckner which provides a candid view of the beginning of the Physics Department. For those who want a succinct answer, they should read Norman Kroll's thoughts included in the Panel Discussion.

Those who are interested in how the products of those early days of the UCSD Physics Department view various social and physics problems can find the answers in the papers included here. If you want to know how the hard nosed handled international relations and the Soviet threat, read Skid Masterson. If you want to get his advice in a nutshell, just read the summary of his paper; you will find that his desideratum is exactly what is happening in international relations four years later (1989). If you want to see sheer power in the application of physics and the scientific method, read Bud Jacobson; see the attention to detail, to precision. Read between the lines to see the tremendous discipline required to carry out the project from beginning to end. And what about the sea? We started at SIO on the ocean and men are drawn to the sea. Read Prothero. For the reader who thinks that is all saccharin nostalgia, read his remarks about how tough it was, read what Dick Morse says to back him up. It was tough. The linear prediction of the percentage passing the Departmental Exam went through zero in 1964. The behavior of the students preparing for that exam was bizarre, at best.

If you think science and scientists are all specialized, arcane; read Herb (Herby Baby) Bernstein's paper on efforts to simplify, to unify. That became part of a book. Or Tom O'Neil's paper on a pure electron plasma. The plasma group had turned away from the muddle of controlled thermonuclear fusion in plasmas to the intellectually beautiful realm of the pure and conceptually simple, a brilliant move. Searching for simplicity in nature is a theme of particle physics, finding the basic, the smallest parts, from which matter is made. Mari Abolins shows how the search for simplicity in the smallest parts of nature requires the largest, most complex machines. If you are interested in the complexity of science, and want to see how the ultimate in the complex, solid state physics is handled, read the papers by Brian Maple or Dick More. To see how a major force in academia performs, read about Brian Maple in his introduction by Walter Kohn. Walter's measurement of accomplishment by inches of descriptive text in Brian's c.v. is unusual, but effective, and it is clear from the measurements that Walter is amazed (mark this, Walter may never have been amazed before). Tom O'Neil describes how Norman Rostoker measured the quality of Tom's thesis; it's somewhat similar to Walter's, showing that the two had known each other awhile. Bernd Matthias, Brian's thesis advisor, died much too early and Walter took Bernd's place for the introduction of Brian. But, though Bernd was not present, read Walter's comments, you can deduce very accurately what Bernd would have said about the theorists by reading Walter's remarks. More difficult than conventional solid state physics is the physics of matter as described by Dick More. Such a wide variety of physical processes are important in his work that one wonders how progress is possible. The explanation is that a larger error, is more tolerable in high densities and temperatures than in conventional solid state physics, where the smallest interactions produce the most interesting results.

Greg Benford, who arrived at UCSD with his twin brother in 1963, gave a different sort of talk; he could have given a technical talk, but was asked for the old SF talk. Both his subject and his attitude were different from the other speakers'. He shows more of the arrogance characteristic of physicists. This attitude could be due to this coming later in the history, but is probably due to his twin brother. Imagine looking at yourself, not in a mirror, but in another person. Since it is another person, you can praise his abilities highly without being immodest. With these two it is appropriate. But you end up praising yourself. The brothers are both plasma physicists and also have useful talents. Read Jim's introduction to Greg; read Greg's talk; judge who is the more articulate---if you can.

Greg described the University as atrophied; Bill Frazer doesn't agree. Read the two and judge for yourself.

If you wonder about the personalities in science, you could read descriptions of scientists in Lewis, or Watson, or even Benford. But if you have read this far, don't bother with them. Just keep right on reading. You won't find much about the common denominator of all men, but you will find the part that is unique to scientists. You will hear from scientists middle aged to old talking about scientists young to middle aged. They were acting in a environment that was new to all of them and which, consequently, elicited responses that accentuate their characteristics. If you are not interested in the technical, ignore it. It is a skeleton which supports the non-technical flesh. Read the flesh then and, using your intuition, interpolate. You will be able to formulate an accurate view of scientists.

Program

La Jolla Physics Symposium

University of California, San Diego
September 6 – 8, 1985

FRIDAY, SEPTEMBER 6

1:00-6:00 p.m. Registration and Check- in at Fireside Lounge, 3rd College

6:00-7:30 p.m. Reception, Top of Tioga Hall, Rms. 1101/1102

7:30 p.m.-on Luau Buffet Dinner, Muir Cafeteria, Muir Commons. (Please show tickets)

11:00 p.m.-on Mountainview Lounge open.

SATURDAY, SEPTEMBER 7

7:00-8:30 a.m. Breakfast. Participants on campus housing breakfast plan should eat at the Muir Cafeteria. Family members and others may pay cash at the door.

9:00-12:30 p.m. **SESSION I, LA JOLLA PHYSICS SYMPOSIUM**
2622 Undergraduate Sciences Building.
Session Chariman: **George W. Webb**

9:00-9:30 a.m. "Early Development of the UCLJ Physics Department"
Keith A. Brueckner
Physics Department, UCSD
Introduced by George W. Webb, Energy Science Laboratories, Inc., San Diego.

9:30-10:00 a.m. "National Security in the Year 2005"
RAdm. Kleber S. Masterson, Jr.
Booz, Allen, and Hamilton, Inc., Arlington
Introduced by Keith A. Brueckner, Physics Department, UCSD.

10:00-10:30 a.m. "Astrophysical Gamma Ray Spectroscopy"
Allan S. (Bud) Jacobson
Jet Propulsion Laboratories, Pasadena
Introduced by Laurence E. Peterson, Physics Department, UCSD.

10:30-11:00 a.m. Coffee

11:00 - 11:30 a.m. "Some Interesting Problems in Geophysics/A Physicists View"
Willam A. Prothero, Jr.
Department of Geological Sciences, UCSB
Introduced by John M. Goodkind, Physics Department, UCSD.

11:30-12:00 p.m. "From the Imperscrutable to the Infinite"
Maris A. Abolins
Department of Physics and Astronomy
Michigan State University, East
Introduced by Oreste Piccioni, Physics, Department, UCSD.

12:00-12:30 p.m. "Pursuit of Simplicity"
Herbert J. Bernstein
Hampshire College, Amherst
Introduced by Norman M. Kroll, Physics Department, UCSD.

1:00 p.m.-on Beach Barbeque, Lawn south of SIO Snack Bar and Scripps Beach. (Please show tickets)

Dinner (on own) Muir Cafeteria open from 5:00-6:00 p.m. Cash may be paid at the door. See list of local restaurants. Mountainview Lounge bulletin board.

9:00 p.m.-on Mountainview Lounge. open.

SUNDAY, SEPTEMBER 8

Breakfast. Participants on campus housing breakfast plan should eat at the Muir Cafeteria. Family members and others may pay cash at the door.

9:00- 12:30 p.m. **SESSION II, LA JOLLA PHYSICS SYMPOSIUM**
2622 Undergraduate Sciences Building
Session Chairman: **Richard L. Morse**

9:00- 9:30 a.m. "Pure Electron Plasmas"
Thomas M. O'Neil
Physics Department, UCSD
Introduced by Norman Rostoker, Physics Department, UC Irvine.

9:30- 10:00 a.m. "Statistical Mechanics of High Density Matter"
Richard More
Lawrence Livermore Laboratories, Livermore
Introduced by Richard L. Morse, Physics Department, University of Arizona, Tuscon.

10:00- 10:30 a.m. "25 Years of Superconductivity at UCSD
M. Brian Maple
Physics Department, UCSD
Introduced by Walter Kohn, Physics Department, UCSB

10:30 - 11:00 a.m. Coffee

11:00- 11:30 a.m. "Why Does a Scientist Write Science Fiction?"
Gregory Benford
Physics Department, UC Irvine
Introduced by James Benford, Physics International, San Leandro

11:30- 12:30 p.m. "Panel Discussion: Future of the University, the Campus, and the Department"
William R. Frazer
Senior Vice President, Academic Affairs
University of California
Harold Ticho
Vice Chancellor, Academic Affairs UCSD
Norman M. Kroll
Chairman, Physics Department, UCSD

1:00- 4:00 p.m. Check-out at Fireside Lounge for those who must depart.

12:30- 1:30 p.m. Picnic Lunch, South Patio of Mayer Hall.
(Please show tickets).

1:30- 6:30 p.m. Free afternoon (Mountainview Lounge will be open).

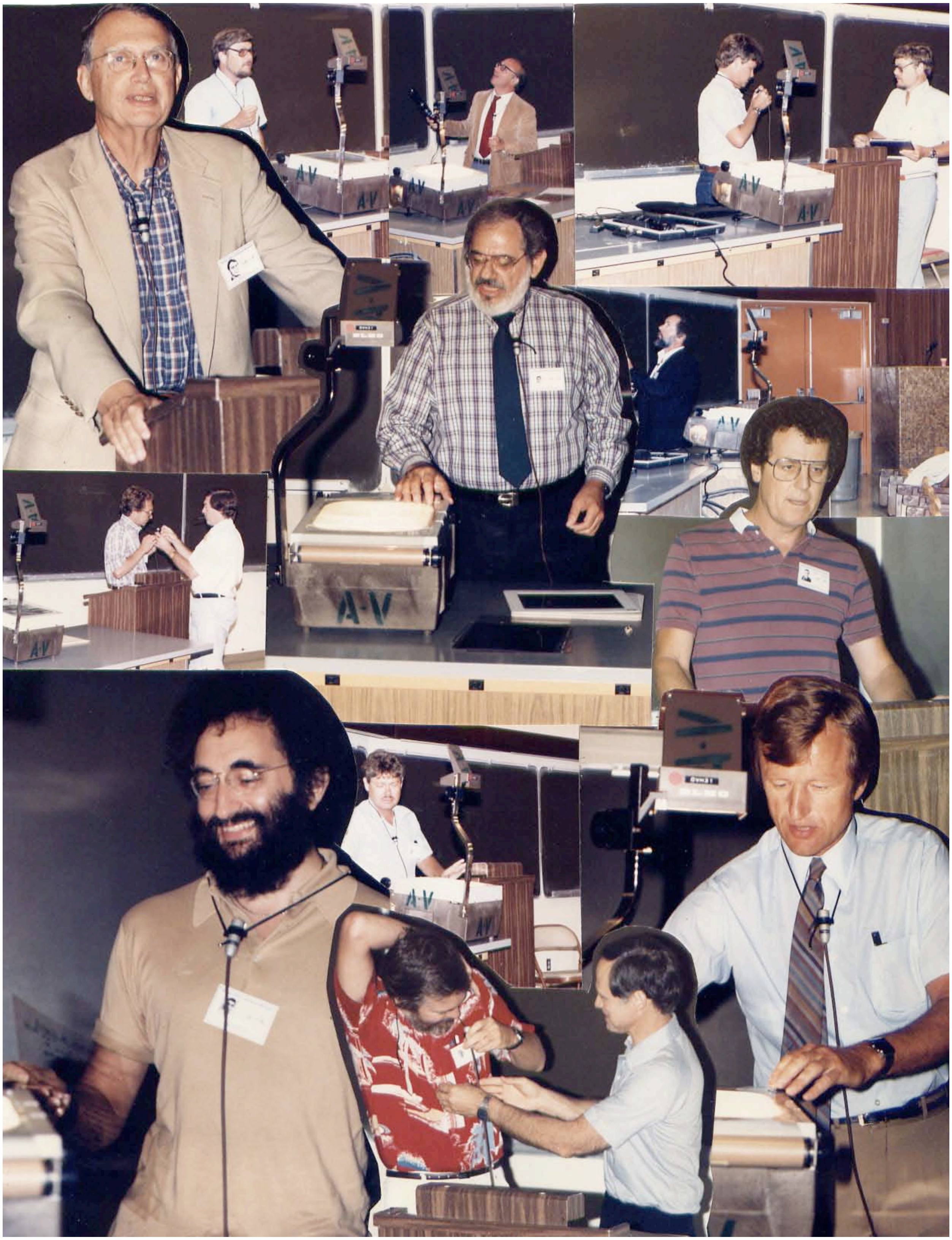
6:30 p.m - on Mexican Buffet Dinner,
Martin Johnson House (T-29, on Scripps Campus just above IGPP Building).
(Please show tickets).

11:00 p.m.- on Mountainview Lounge open.

MONDAY, SEPTEMBER 9

7:00- 8:30 a.m. Breakfast will be available at the Muir Cafeteria
$4.50/person at the door.

8:00- 11:00 a.m. Check-out at Fireside Lounge.

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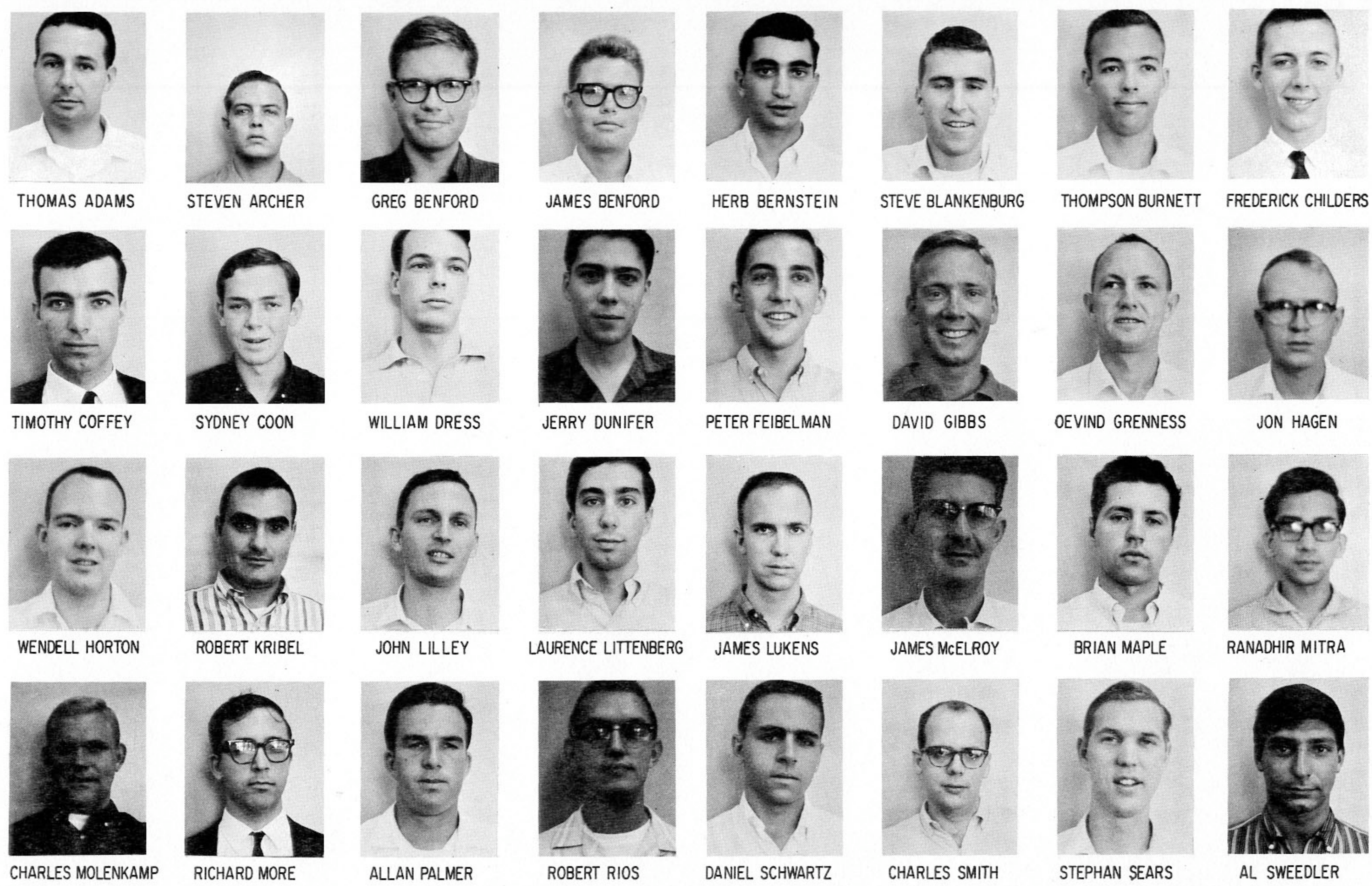
Class of 1960-61

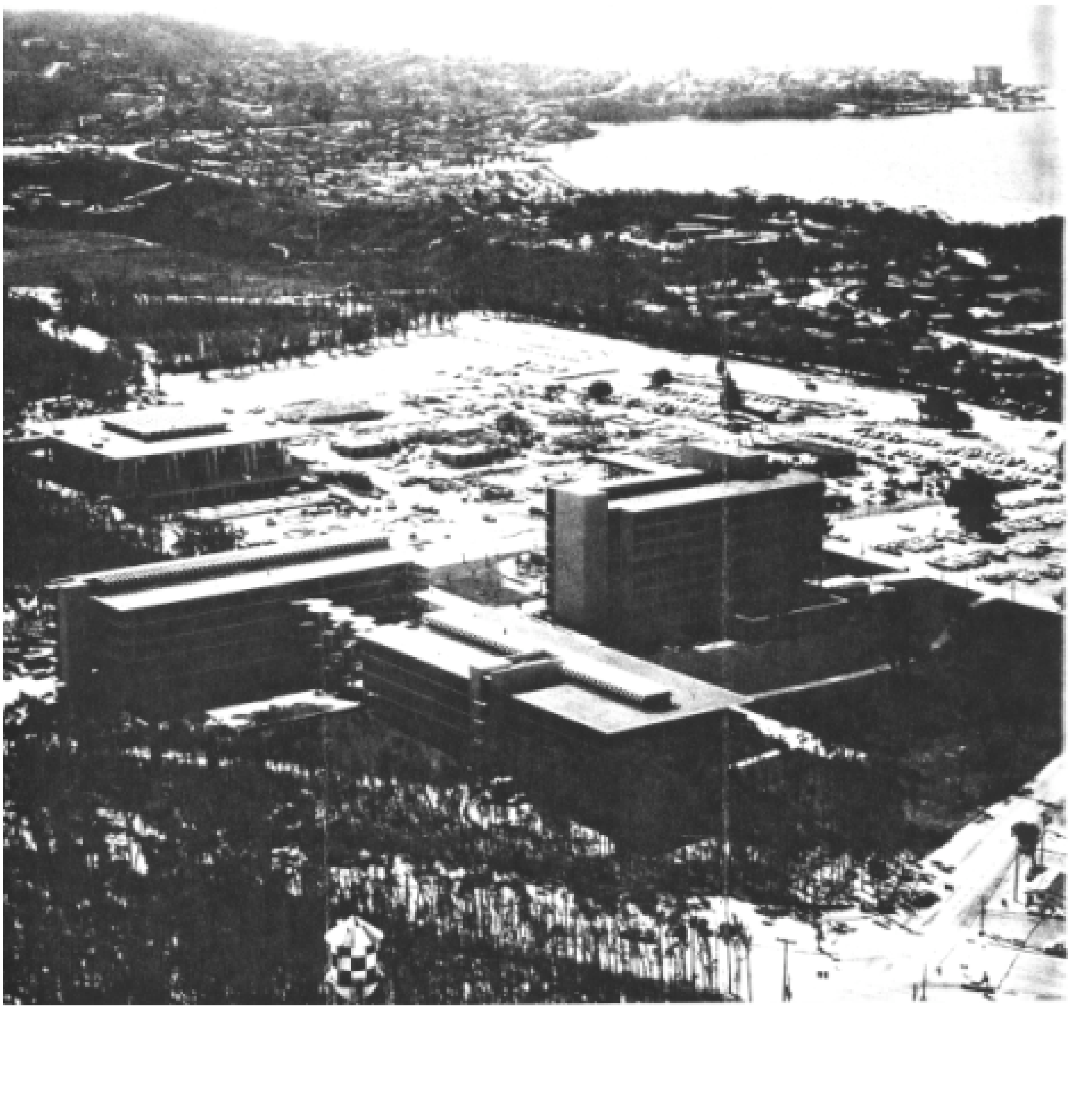
Class of 1961-62

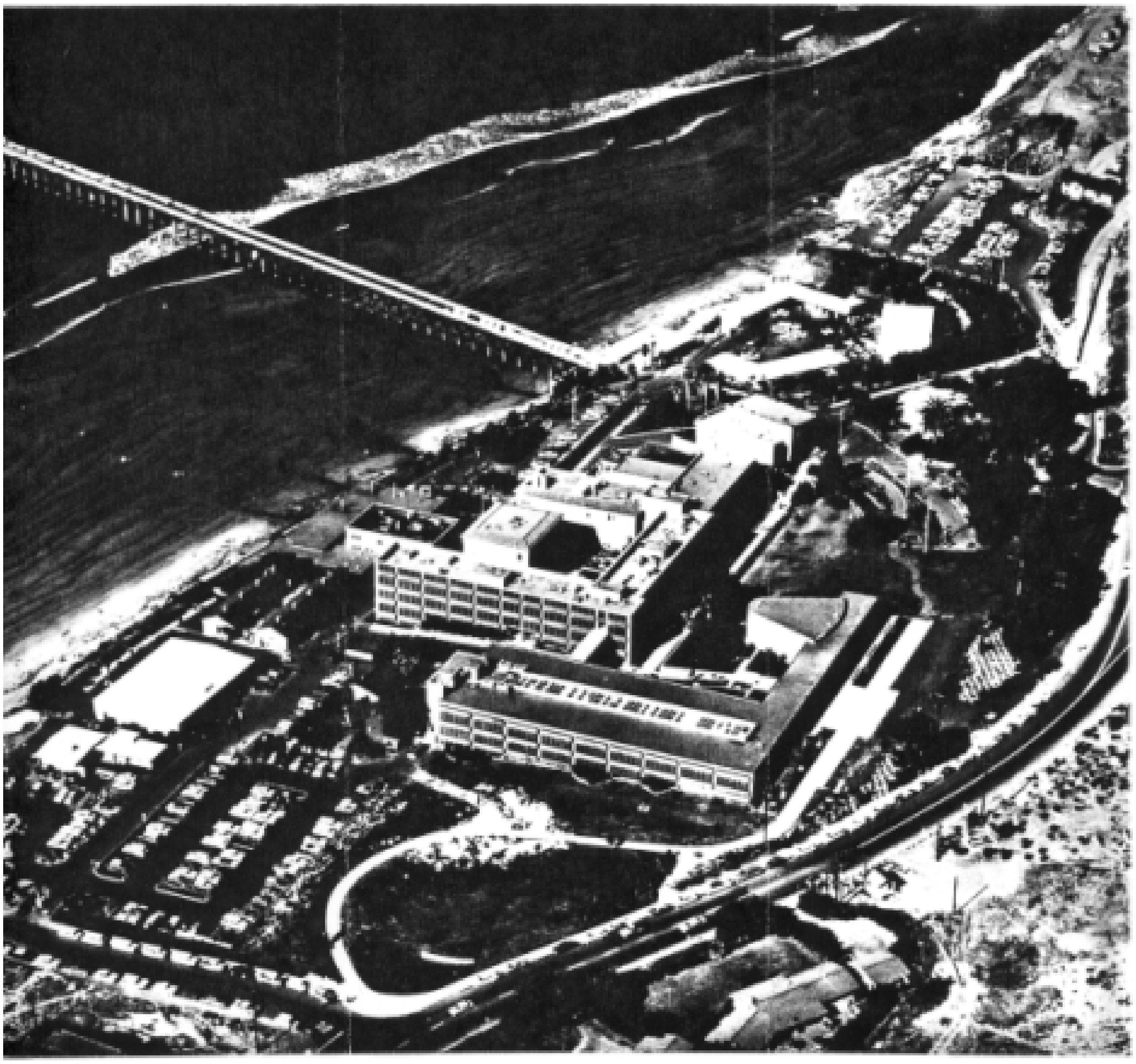
Class of 1962-63

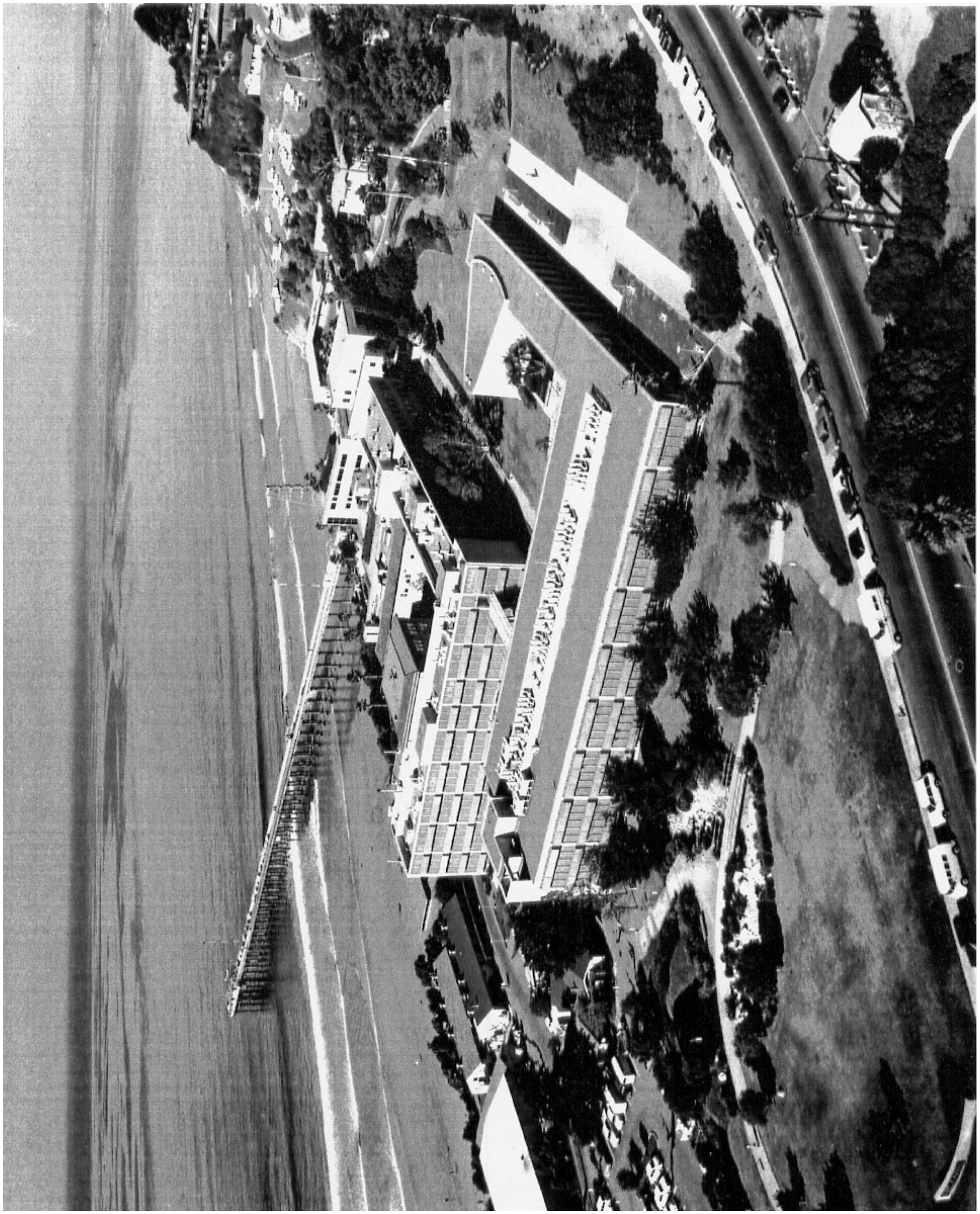


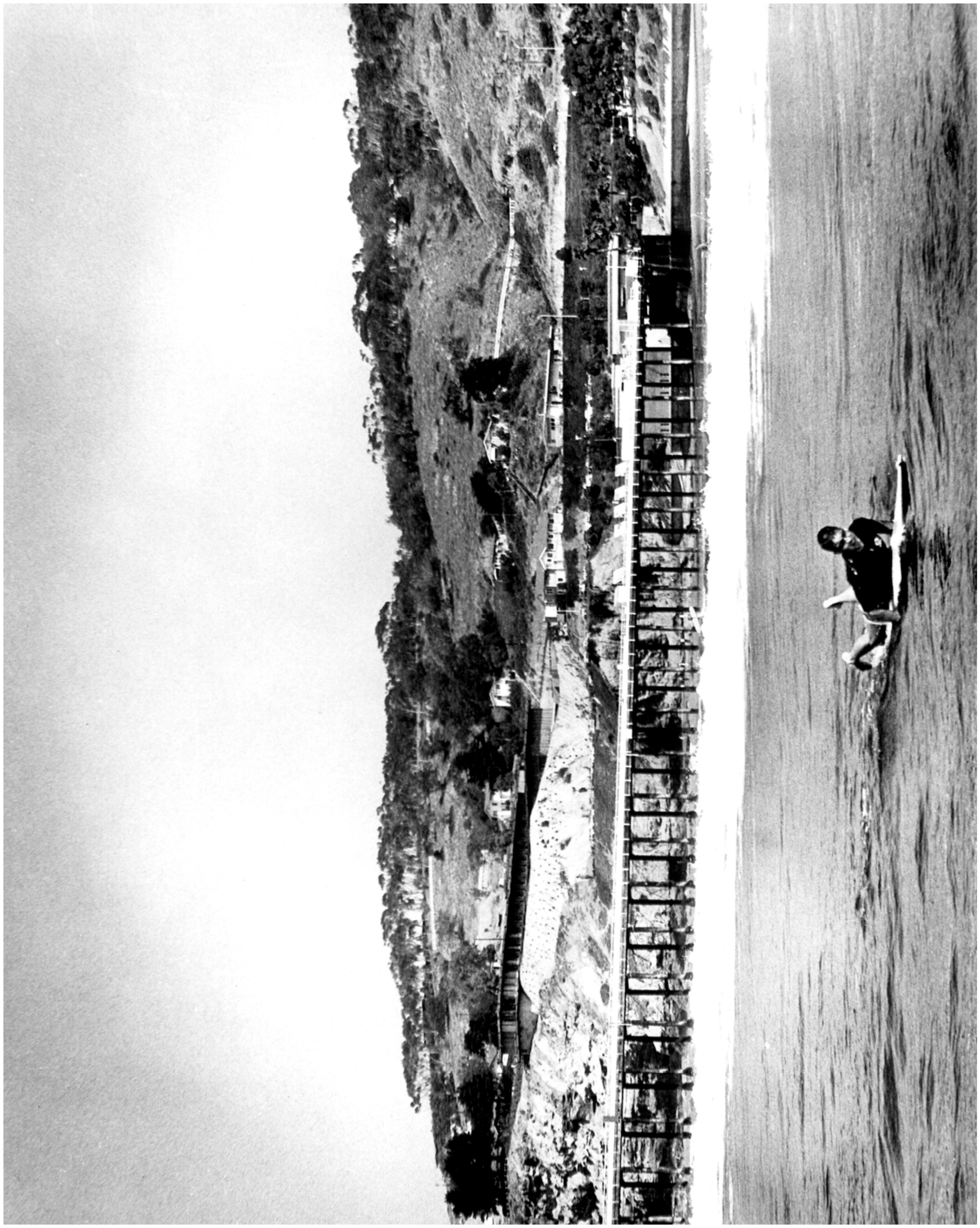
Class of 1963-64

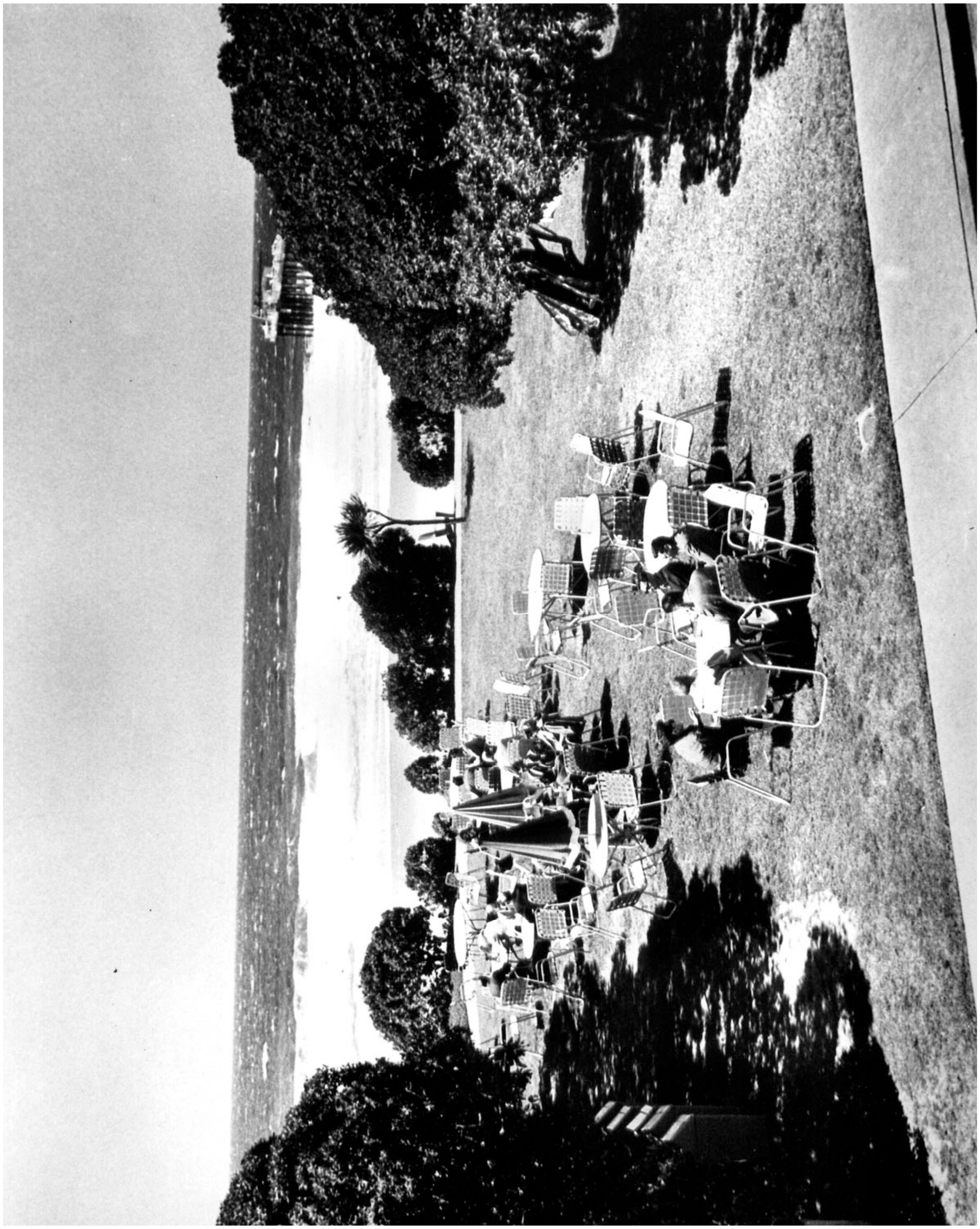
Class of 1964-65

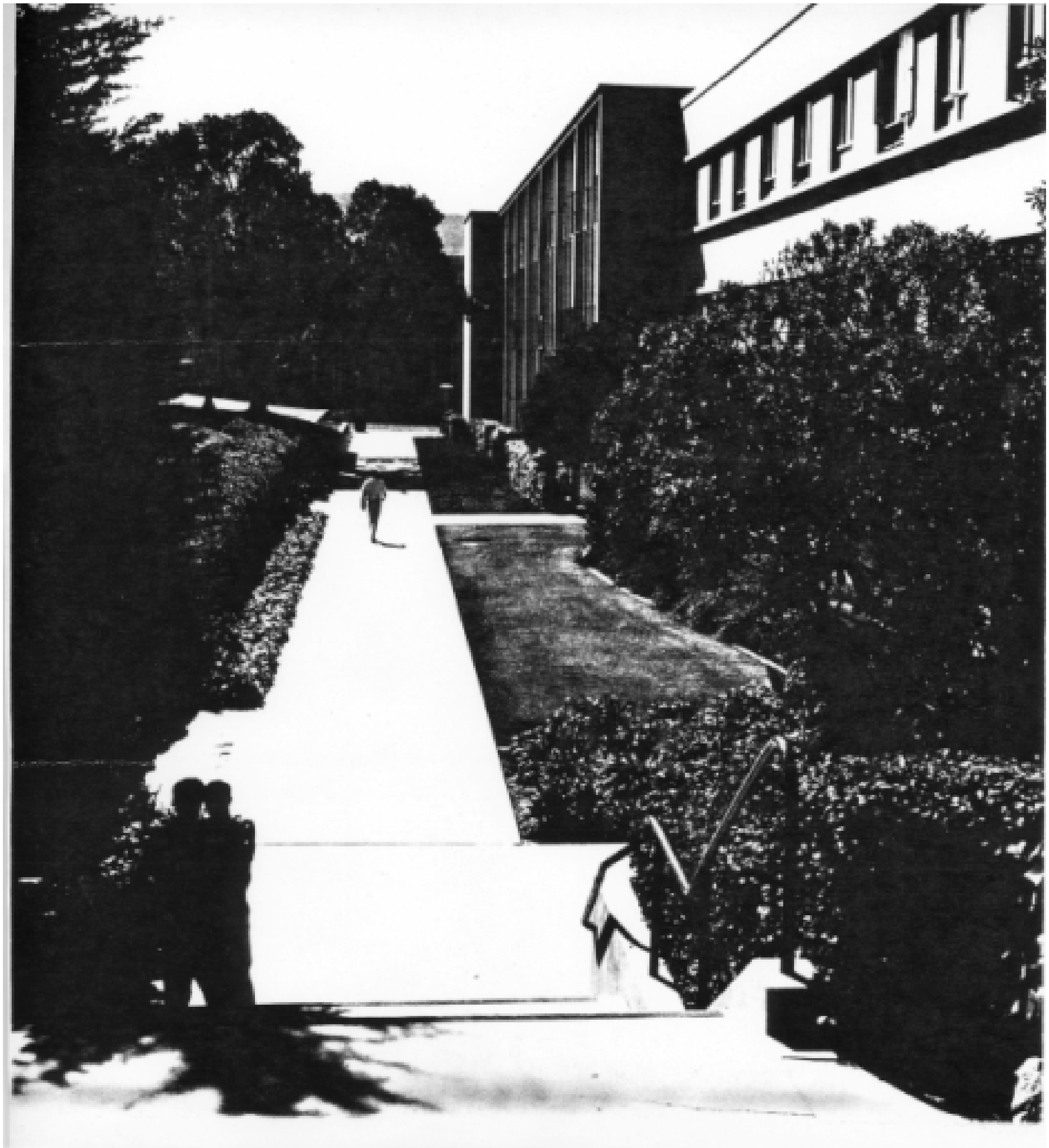












GENEROUS FINANCIAL SUPPORT FOR THE

LA JOLLA PHYSICS SYMPOSIUM

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UCSD PHYSICAL EDUCATION DEPARTMENT

ACKNOWLEDGMENTS

There are a number of individuals, groups, and companies that contributed to the events reported here. Most important are the financial contributions. The Physics Department of UCSD gave the original seed money to get the reunion effort going, and assist with the preparation of the Proceedings. Additional funds to complete the Proceedings were provided by discretionary funds associated with the Bernd T. Matthias endowed chair. Margaret Maple provided flower settings and other decorations for the two Reunion dinners and prepared the photo montages for the Proceedings. All of the manuscripts that appear in these Proceedings were edited by the Reunion Committee and typed by Nancy McLaughlin. Carolyn Rosado, Christina Scanlon, and Christina Kelly scanned the photographs and compiled the document. Maple Lab group members helped with the proofing of the final document and a website was designed and set up by Christina Kelly and Nicholas Butch. The fee that was charged to attend the Reunion/Symposium did not cover all of the expenses. The Physical Education Department of UCSD gave participants the use of their facilities without the normal charges. Contributions from local companies upgraded the Reunion/Symposium, taking it from what would have been an interesting event to a truly impressive one. The companies that added that bit of class were:

GA Technologies, Incorporated
Maxwell Laboratories
Science Applications International Corporation
Spin Physics, Incorporated/Eastman Kodak

In companies such as these different dollars have different colors, come from different bins and are of different values. The dollars that the above companies contributed were some of the most valuable, and were greatly appreciated.

The authors get special thanks. Preparing a paper is always more work than expected. In this case, in addition to presenting the paper, they provided manuscripts and edited the transcriptions.

Two of the original physics faculty members, Normal Kroll, then Chairman of the Department, and Shelly Schultz provided advice and encouragement. Three graduate students, Kevin Fine, Poul Hjorth and Ming Wu kindly agreed to take photographs at all of the activities; Hans Bauer and several physics graduates handled the audio-visual equipment. Bud Jacobson picked his banjo and sang a few folk songs as in days gone bye; Dilip Bhadra and Laura Matteson demonstrated a characteristic dance ritual--without injuring themselves-- that was popular in the early '60's.

A reunion committee was formed to do the work necessary to carry out the event and produce the proceedings. Although the committee may not have had any formal existence, it did have a form. The Co-chairmen were Brian Maple and Tom O'Neil. The other members were George Webb, Tom Delmer, Nancy McLaughlin (Smathers in the early days) and Margaret Maple.

The Reunion-Symposium consisted of the following activities: a Reception at the Top of Tioga Hall on Friday, from 6:00 - 7:30 p.m. followed by a Luau Buffet Dinner at Muir Commons Cafeteria from 7:30 p.m. on. On Saturday, we started with the first Session of the La Jolla Physics Symposium at 9:00 a.m. to 12 noon which was held in the 2622 Undergraduate Sciences Building auditorium. We all adjourned from Session I on a sunny, and warm day to a Beach Barbecue on the lawn south of the SIO Snack Bar and Scripps Beach. On Sunday, the second Session of the La Jolla Physics Symposium was held from 9:00 a.m. to 12 noon in the same auditorium. This second session was followed by a Picnic Lunch at 12:30 p.m. in the South Patio of Mayer Hall. On Sunday evening starting at 6:30 p.m. we held our farewell Mexican Buffet Dinner at (T-29) the Martin Johnson House overlooking the Pacific Ocean on the Scripps campus replete with a green flash and Mexican Mariachies.

Those who attended contributed greatly. Not only their time and money, but in enthusiasm, interest and excitement. Many who could not attend sent letters which were posted for all to read. In this last regard, our Indian colleagues should be remembered. Politics in their country were such that they could not come; many sent letters. The sound of their beautifully accented English was missing and missed.

La Jolla Physics Symposium

Saturday, September 7, 1985

Session I

GENERAL INTRODUCTION TO SESSION I

by George W. Webb

Energy Science Laboratories, Inc. , San Diego, California

Welcome to the first technical session of the La Jolla Physics Symposium. To those of you who weren't here last night, welcome back to La Jolla. We're here at this Symposium to be reunited and to celebrate the astonishing fact that the Physics Department is 25 years old already. This morning's program is part of that celebration. It is very varied and promises to be quite interesting. However, before we open the session, there are a few announcements that have to be made. First is that anyone who hasn't registered, can do so during the coffee break in 2306 Mayer Hall. If you have paid for your Registration and you haven't picked them up yet, there are some tickets in there to the social programs in the afternoons. You can also Register tomorrow morning at 10:30 am during the coffee break, as well. The second announcement is to the speakers and to those making the introduction, that each of the talks is allotted 30 minutes, the total of which is to cover an introduction, the talk and the discussion period afterwards, so that the people making the introductions are encouraged not to talk too long, only five minutes, otherwise you're going to cut into the speakers time and likewise the speakers are encouraged not to speak too long, say maybe twenty minutes or so in order not to cut into the discussion period, or worse yet, into our Beach Party time this afternoon.

Well, it is a pleasure to open the session this morning. As you were told the program is very broad in its scope and covers a period of 25 years. Keith Brueckner is going to tell us about the formation of the early Physics Department in that crucial period around 1960. And then, Skid Masterson is going to tell us about National Security in the year 2005. The talks vary in the complexity of the topic from the complexity of national security systems to Herb Bernstein's description of the search for simplicity. Bud Jacobson's going to tell us about investigating the properties of matter at great distances and then Mari Abolins will tell us about matter at very short distances in the laboratory. Bill Prothero is going to tell us about some interesting problems in geophysics from a geophysicists point of view. So, as you can see, there is quite a lot of variety in the program.

INTRODUCTION OF KEITH A. BRUECKNER

by George W. Webb

Energy Science Laboratories, Inc., San Diego, California

On behalf of the organizing committee, it is a really great pleasure to introduce our first speaker today, Keith Brueckner. Keith, more than anyone else is responsible for the formation of the early department in that crucial period around 1960. Keith received his Ph.D. in 1950 from Berkeley and then had gone on to hold a succession of posts at the Institute for Advanced Studies at Princeton, at Brookhaven, the University of Indiana and the University of Pennsylvania. All during this time, he was working on a variety of different topics, and was given much recognition and many awards for his work. Just as one example, he won the Denny Heinman Prize in 1963 for Mathematical Physics. That award summarized some of this work by citing his contributions to the theory of elementary particles, to nuclei, condensed matter and especially his courage and persistence in deriving the properties of nuclear matter from nucleon-nucleon interactions. Well, I guess at that time Keith's work had come to the attention of Roger Revelle who was laying the groundwork for the University at that time. Revelle persuaded Keith to come to La Jolla to help start UCSD and Keith then succeeded in attracting a lot of the key people here, who themselves later attracted people as well. But, I think Revelle summarized it for himself, in an interview in San Diego Magazine in 1964 where he said, "getting Keith Brueckner was one of the best things that I ever did in my life. He laid out the whole Physics Department." Well, that was just the beginning of Keith's service to the campus. He went on to be the first Department Chairman and then Dean on three different occasions. He was also the director of several Research Institutes which were responsible for training a lot of researchers.

For all these reasons, we are really very pleased that Keith has agreed to tell us about the early development of the UCLJ Physics Department.

EARLY DEVELOPMENT OF THE UCLJ PHYSICS DEPARTMENT

by Professor Keith A. Brueckner

Department of Physics, University of California, San Diego, La Jolla, California

It's a long time ago that I came to La Jolla and met Roger Revelle. How long ago it is, is apparent from the fact that my son, who is a Professor at Illinois in Economics now, is older than I was when I came here. And, when I see my young son and think that I was younger at that time than he is now, I wonder how such a young man could set out in an undertaking as difficult as starting UCSD, and UCSD Physics. I have tried to recreate the situation as it was back then, and the elements that made the formation of the Department relatively easy. Looking back at those years. I came to La Jolla in the late fifties once or twice, consulting at General Atomic, working on Project Orion--that was the nuclear bomb propelled space craft that a lot of physicists worked on at that time. In 1958 I was here, and I am not quite sure exactly how it happened, but I think I gave a talk at General Atomic, and I believe that Eckart and Liebermann came to hear that talk. They apparently liked what they saw because within a day or two, they came and picked me up at General Atomic for lunch and the great, tall, tanned Roger Revelle climbed out of the car and I was introduced to him. And that day, Revelle applied all of his persuasiveness and charm and vision in telling me about UCSD, although it was UCLJ at the time (University of California at La Jolla). Of course, it didn't exist with that name either. That was the name that Revelle had in mind for the new campus. Now, the description that Revelle gave grew out of a broader vision by Clark Kerr, who in the middle fifties, as President of the University of California, looking ahead into the last part of this century, had seen that the projected growth of the population in California and of the university aged students would vastly exceed the capacity of the existing campuses to handle and, therefore, a number of new campuses was necessary. Clark Kerr's projections were inaccurate. They were done by apparently drawing a straight line on log-log paper. And, I remember at the time, the projection for 1995 was that UC would have a total enrollment of about 250,000 students and that would take ten campuses the size of Berkeley to handle.

Now, why is Berkeley size an upper limit? I think it was felt that both Berkeley and UCLA at about 28,000 to 30,000 students were about the upper limit for a reasonable campus. Berkeley, also, I think, was somewhat limited in physical space. It couldn't grow to 50,000 students like Michigan or Minnesota had done. So Berkeley and UCLA were models of a good sized campus. They were big enough to be highly diversified, to have broad faculties in humanities, social sciences and engineering and that was the size that was sufficient to provide the variety of a great university.

So, Clark Kerr had projected the establishment of ten campuses and they were all to reach about Berkeley size in 1995. Those plans were too optimistic, but they governed the thinking of Revelle and eventually of the people around Revelle in those early years. San Diego was selected to be one of the new campuses, and it was chosen both because of its geographical location far in the southern part of the state where there was no campus of the university, and also because SIO provided a faculty nucleus around which to grow and an administrative structure from which the campus could be developed. So, that was the underlying thought. Now, in back of that thought, supporting it, of course, were the tremendous resources of the state of California passed through the Regents of the University into hands of the administrators. And it was the tremendous financial strength of California, made available to the planners of the new campuses, which was a vital element in proceeding. In other words, when Revelle talked to me he knew, he expected with Clark Kerr's assurance, there would be strong support for physical facilities, and also he had obtained, in some remarkable way, an agreement from Kerr that a large number of high level appointments could be made initially at this new campus in San Diego. That pattern, which Revelle was able to establish, was not followed at the other new campuses. So the extraordinary start which UCSD got was based, not only on the great financial strength of the Regents of the state of California supporting the planning, but also in Revelle's obtaining the initial high level appointments. So, what Revelle had, and what he was able to tell me about, was the existence of this very ambitious plan of the state of California, of the Regents of the University, and of the financial strength behind it which would support the vision that Revelle had. That was the essential element that lay behind the establishment of the new campus here.

The second element which was important was Revelle himself. Revelle had a tremendous vision and concept of what a great university should be, and he followed a path which had, to my knowledge, been followed only once before in the United States. I am not even sure of the campus, I think it was Clark University which was a nucleus from which another university grew. But, except for that example, no campus started following the pattern which Revelle established. His idea was that around SIO one should build first a science and engineering faculty with that as a nucleus to be built at the graduate level because SIO was only a graduate institution. And then with that as a nucleus, one would add the general faculty, and the general faculty at the graduate level. Then after a few years, 1965 was the plan, the first undergraduates would be admitted and then UCSD would start a continuous transition from a graduate institution into a general campus. Now for me coming and talking to Revelle, I think if UCSD had been imagined by Revelle and the University to be a graduate Cal Tech, I think it wouldn't have so interesting for me or for the people who finally came here. It was the idea that this would be a general campus with faculties not just in the sciences, but faculties in the humanities, fine arts, social sciences and also there would be a general campus with undergraduates. I think, at least for me, that plan was much more attractive than a more limited plan of a graduate technical school. This is the plan that Revelle had when he talked to me. So Revelle's thinking, the strength of the

University and its support, and finally, the extraordinary location in La Jolla -- those were of much greater significance than a more limited plan of a graduate technical school. This is the plan that Revelle had when he talked to me. So Revelle's thinking, the strength of the University and its support, and finally, the extraordinary location in La Jolla--those were the three elements which made the establishment of UCSD in retrospect rather easy to bring about. I should add that the city of San Diego itself also was making moves, although they weren't completed, I think, until after I came here. They were making moves to transfer the Camp Matthews land, the upper campus land, which was government property and partly city property, to transfer those to the University of California Regents. And that 1200 acres which is still the area which UCSD holds and doesn't utilize, the plan was there for the city and the federal government to transfer those properties to the Regents of the University. So, that location, Revelle's personality, Clark Kerr in the background with his planning of the University growth, these were the essential elements. And with those, I think that the other achievements were relatively minor.

Now at that time, 1958, I was younger than my son is now, and I must have been very confident, because I (from the audience), "You were." (general laughter) I was. (more laughter). It is hard to imagine what I must have been like at that age. At, any rate (more general laughter from the audience). At any rate, with no inhibitions, I went after the best people in the country. And, with very little consultation, although I think I must have talked to Revelle about this, and possibly with Eckart and Liebermann who were Professors of Geophysics in SIO, I decided that the Department should be built around some selected fields. Solid state and low temperature physics, plasma physics--they were to be the heart of the experimental program here. It was also apparent that one could with the limited facilities, which of course didn't exist at that time, one could build a strong elementary particle theory group, astrophysics and space physics were also a possibility. Again, those did not depend on strong facilities which didn't exist and couldn't be developed quickly. And, then, in addition to the elementary particle theory I felt there should be some elementary particle experimental work carried out in a users group which would do work at Stanford, Berkeley and possibly at the other national laboratories. So, those are the elements of the faculty as I saw them and the next year, 1959 and 1960, I attempted to make the critical first appointments in those fields.

Now, the people who watched this happen, know there was kind of a domino effect. I talked to many people in that first year, and as one person showed signs of interest I was responsible to tell other people that this great man had showed signs of interest. People became aware that they were being recruited as a member of an elite high powered faculty to come into UCSD. I think it was the knowledge of the simultaneous negotiation with many very good people that made the very good people one by one agree to come. Also, as some of the experimentalists remember I also had in my pocket the attractive currency of being able to offer not only new buildings, but also new laboratories. In the state of California, the Regents, as they build a new building, provide large funds for experimental equipment. So, people coming here not only had a new laboratory they could design, but money for equipping it. Also, as I talked to the senior people, I was able to make another promise which I was usually able to honor--I am sure I over extended my authority at that time--I was able to make promises that a person could invite his friends, his colleagues, providing they were good enough, and also some of the younger associates. The facilities, the money available, the possibility of bringing a group of people known to each other together, those were also very important in those first negotiations.

Well, let me just really end this by listing the people with whom I negotiated and that came here during those first years. (From the audience) "How about those who didn't come." (laughter) I'll mention those too. (laughter) I only remember a few of the failures. The low temperature and solid state group started off with Matthias whom I knew at the time through consulting work at Los Alamos, he was a friend of mine. And, I talked to him and he agreed to come, eventually. And, with his urging, I talked also to Feher and Suhl, those three were all at Bell Laboratories at the time. Also, I think it must have been at Matthias' suggestion that I talked to Walter Kohn, who was at that time I think recognized as the finest solid state theorist in the country. And, Walter agreed to come, and he put one condition--I remember it very well at the time--he would come providing I would become the Chairman of the Department. I think up to that time there was no such decision made, but that was the condition that Walter put on his acceptance of coming here. I talked to Marshall Rosenbluth, who was on the staff at GA at the time. I remember in the appointment letters, Marshall--I think he has heard this before--but, one of the appointment letters was from Chandrasekhar, and he identified Marshall as the finest classical physicist in the world. I don't know if Marshall knew that this was his description. Later, John Wheatley also came. And, with Matthias and Wheatley, the experimentalists, we certainly had, when they were here, the finest low temperature experimentalists in the country or in the world. Joe Mayer was coming. He had been recruited by chemistry. And, I talked to Maria Mayer, and she agreed to come. I had known her before, so it was easy to talk to her and she did not have a faculty position at Chicago and she accepted the appointment here. Then in astrophysics and space science, the Burbidges came, Geoffrey and Margaret. And, I think I went to Iowa and talked to Van Allen, and asked who were the very good experimentalists in space science based on whose recommendation I think--I have forgotten the sequence, but McIlwain and Peterson came. There also was a very famous and very fine elementary particle theorist, Norman Kroll, that I talked to in that first year. And, there was a hectic run of recruiting parties. And, I remember at those Wong and Frazer came, were invited and accepted. Also, in that very early period, Piccioni, who was a friend of mine, I had known him at Brookhaven, and I talked to him and he came as the nucleus of the high energy experimental group which we had decided to form. Now, as Marshall, or someone said, not everyone came. Well, I have given a list of people who did come. A few people turned me down. I talked to a lot of my theoretical physics friends, Gellman came early, was talked to by Revelle and was immovable. I talked to Murph Goldberger, Ken Watson, Francis Low and, one famous recruiting failure, Valentine Telegdi. I think Telegdi came in a few times in those early years and we tried repeatedly, but we failed. And, then Bill Thompson also came very early. I think I had met Bill in England actually when I was visiting Harwell, probably. So, I had known Bill and with Marshall having agreed to come, Bill's addition was also a wonderful strengthening of the plasma physics group. Well, that is the way it started and it took the elements I mentioned, the strength of the University, the money, the state of California, San Diego, La Jolla, UCSD, SIO, Revelle, Eckart, Liebermann and then the cumulative effect of dealing with so many good people, very quickly, at one time and having the resources to offer them something which did become a reality. Thanks.

INTRODUCTION OF RADM. KLEBER S. MASTERSON, JR.

by Keith A. Brueckner

Department of Physics, University of California, San Diego, La Jolla, California

I mentioned that thinking of my young son, who is older than I was when I came here, makes me realize how long ago that was. But, another history marks how long ago it was as well. The first student at UCSD was a very smart, young gentleman, Skid Masterson, Jr. He finished his Ph.D. work in 1962. I believe I had known him in Pennsylvania, and he followed me out here. He is now a retired Admiral. He shot up through the ranks in the Navy, reached the position of Rear Admiral and now he has been retired for three years. So, that is something else that that period of 25 years spans. So, Skid, -- Admiral Masterson --

NATIONAL SECURITY IN THE YEAR 2005

by RAdm. Kleber S. Masterson, Jr.

Booz, Allen & Hamilton Inc., Arlington, Virginia

Thank you, Keith. Keith regained five minutes of our schedule, and I will try to follow his good example since we are running behind.

Following up on Keith's description of how everyone was attracted to La Jolla, I have to say that my coming to La Jolla was my wife's doing. I had been invited to give a talk to a mathematics graduate student group at Stanford about a compiler I had written, and my wife and I had combined the trip with an interview with Sidney Drell, then head of admissions for the physics program. The interview went well, and Stanford did accept me. However, near the end of the interview, Professor Drell said: "Physics is a very competitive field and you should apply to several schools." I was about to say something noncommittal when, to my horror, a voice beside me asked, "What is a good school in Southern California?" I thought, "There goes my physics career!" Professor Drell replied, "A very good friend of mine, Keith Brueckner, is forming an absolutely outstanding group in La Jolla, and you really ought to check on it." That was good advice.

I wanted to thank two groups today. One group are the people who made this reunion possible. It has brought back a flood of very warm memories and it has been delightful seeing so many old friends again. Second, I want to thank the faculty that Keith succeeded in attracting to this University for providing an absolutely outstanding preparation for the career I have very much enjoyed (and recently concluded) in the Navy, and the career on which I am now embarked at Booz, Allen and Hamilton, where I head a group of people who are producing advanced software, defense studies including arms control studies, and war gaming for both education and analysis. These two careers motivated my topic, "National Defense in the Year 2005." Because it takes so long to develop and deploy new systems, we have to try to look that far into the future if we are going to make wise decisions about the things that we are developing today. And, as a result, I wanted to try to capture for you some of the insights that I have gained about what that future might look like.

To begin with, I would like to say a few things about some of the things I have done, to give a little basis for calibrating what I will say about the future. Virtually every assignment I have had since leaving La Jolla has been associated with introducing new technology into the fleet, generally, with the newest of the systems, helping to ease their transition into fleet use, both testing them and developing operational techniques for their use. After I left La Jolla, I spent some time back at sea. Then in the mid sixties, I was "invited" to Washington for the first time. Secretary Nitze, who was then the Secretary of the Navy, had formed a group of systems analysts to give him support competitive with that of the systems analysts that Mr. McNamara had brought to the Department of Defense. The Ph.D. in physics was the reason that I was ordered into this group--a Ph.D. physicist had just left, and he had done so well that Mr. Nitze wanted another. Shortly thereafter, a young Rear Admiral by the name of Zumwalt showed up and formed a similar group for the Chief of Naval Operations. He borrowed me for six weeks and kept me for two years doing studies of various naval systems. During that era, we conducted definitive studies that led to many of the new ships and airplanes in the Navy today. In our models, which I had a great deal to do with, we had the ability to "fight" a 90-day World War III, on land and sea and in the air. We were doing it with an expected value model so that we could make small changes in anything anybody wanted to vary, whether it be the threat or the characteristic of the weapon systems, and we could calculate meaningful differences that such variations would make in the ability of the Navy to fight the whole war. It was on the basis of our calculations that we persuaded Mr. McNamara to permit the Navy to build several new classes of ships and the F-14 aircraft, with characteristics based on the study results.

I then went back to sea for several more years of introducing new systems to the fleet. Eighteen months of that was in command of the USS Preble, which some of you may remember. We came through San Diego on our way to Pearl Harbor and some of you came for lunch. I remember, in particular, Maria Mayer who came aboard that day. It was a particularly interesting day because we were replacing a defective missile booster and remating the new booster with its missile. After lunch, I invited whomever had enough time to stay to come back and watch the evolution. We went back into the missile house, and little did my crew know that this very attractive lady who came to watch this evolution could probably have designed the warhead! I went on to Pearl Harbor with the ship, and later deployed to Viet Nam with it. After that tour, I went back to Washington and was involved (among other things) in managing the development of weapons systems. You probably saw the recent news of the Army's DIVAD gun that was canceled. Well, we developed a gun like that ten years earlier, and had it deployed since 1980. Ours had a different mission, and has been very successful. I wanted to show you two pictures of what our system, the Phalanx can do. What you see in the first photograph (Figure 1) is actually a missile that was fired at a ship in which we had the Phalanx installed. As you can see, the missile warhead was blown up. Had the missile hit the ship, I can assure you that it would have made a large hole. The Phalanx is an automated gun system with its own radar. It is totally automatic. The system looks for an incoming missile. When it detects one, its Gatling gun fires a stream of depleted uranium penetrators which are about the size of the end of my finger. These hard, massive penetrators will dig into the head of the missile coming in and explode its warhead, which is what you see happening in the photograph. The Phalanx is capable of seeing its rounds go past the target and spotting and correcting the aim of the gun. To give you a further example of the capability of this system, the next photograph (Figure 2) shows a shell that we fired at a Phalanx. That shell is about seven inches in diameter and was traveling at more than twice the speed of sound. The shell did not have

Fig. 1. Phalanx killing an incoming missile.

an explosive in it, because we wanted to see how many times we could hit it. As you can see, we were reasonably successful! This system has also been tested against the Exocet which gave the British such fits in the Falklands campaign.

These pictures are one indicator of the technology that is going into the fleet today. Further, Phalanx is but one of a family of systems. It is the shortest range of all the systems we are installing -- what we call terminal defense. At the other end of the spectrum are missile systems, such as those on the Preble. The next viewgraph (Figure 3) shows a sister ship of the Preble, with its missile system on the after end of the ship. When originally introduced into the fleet in the early 1960's, Preble's missiles had a range of twenty miles and much of the command and control was manual, with analog systems. When I commanded the Preble in the early 1970's, we had increased the missile range to about 40 miles and we had increased the automation of the command and control with digital systems. Today, as a result of some systems that groups working for me developed in the 1979-82 time frame, we now have almost fully automated the entire combat system and we have increased the missile range again, to well over 100 miles. We can now have multiple engagements going on simultaneously with missiles flying out to their targets and switching into what we call semiactive terminal homing in the last 10 or 20 seconds of their flight.

More significantly, we have a family of ships going into the fleet, that are designed from the bottom up with new technology, that literally are capable of fully automatic operation from front to back. This viewgraph (Figure 4) gives an indication of the extent of the testing that these systems go through in their developmental phases, shooting at everything from fast, high altitude Air Force BOMARC strategic missiles to some very low altitude missiles. The bottom line is that the Navy has capable systems countering hostile aircraft and missiles.

Fig. 2. An artilliery shell engaged by Phalanx

Let me talk a bit about my insight into the other services. My last job in uniform was as the chief war gamer for the Joint Chiefs of Staff. I headed a group of officers from all of the services, doing everything from political military gaming--how do you avoid going to war in the first place--to what happens if the war gets as bad as a war could get. It was a phenomenal opportunity to understand how the rest of the services operate and how their systems work and to gain some perspective of where they are all headed for the future. This perspective has been further broadened at Booz, Allen and Hamilton where I have actually been gaming further in the future than I was in the JCS. At Booz, Allen we have some highly aggregated models, most of which operate on IBM PC's, with which we can examine the full spectrum of warfare, from low level confrontations to an all out war with various levels of strategic defense systems and various levels of offensive systems. We have been using these models, both at the National Defense University in the education of officers in trying to learn about warfare with combined arms, to analysis work in the Department of Defense, where people are really trying to look into the future with an open mind as to how we might evolve to a world of stable deterrence. I will return later to some of the directions which might lead to a more stable world.

First, though, I would like to digress to another point: the historical importance of being able to operate effectively first on land, later on the sea, most recently in the air, and soon, I believe, in space. The two things that have made it important for a nation to be able to

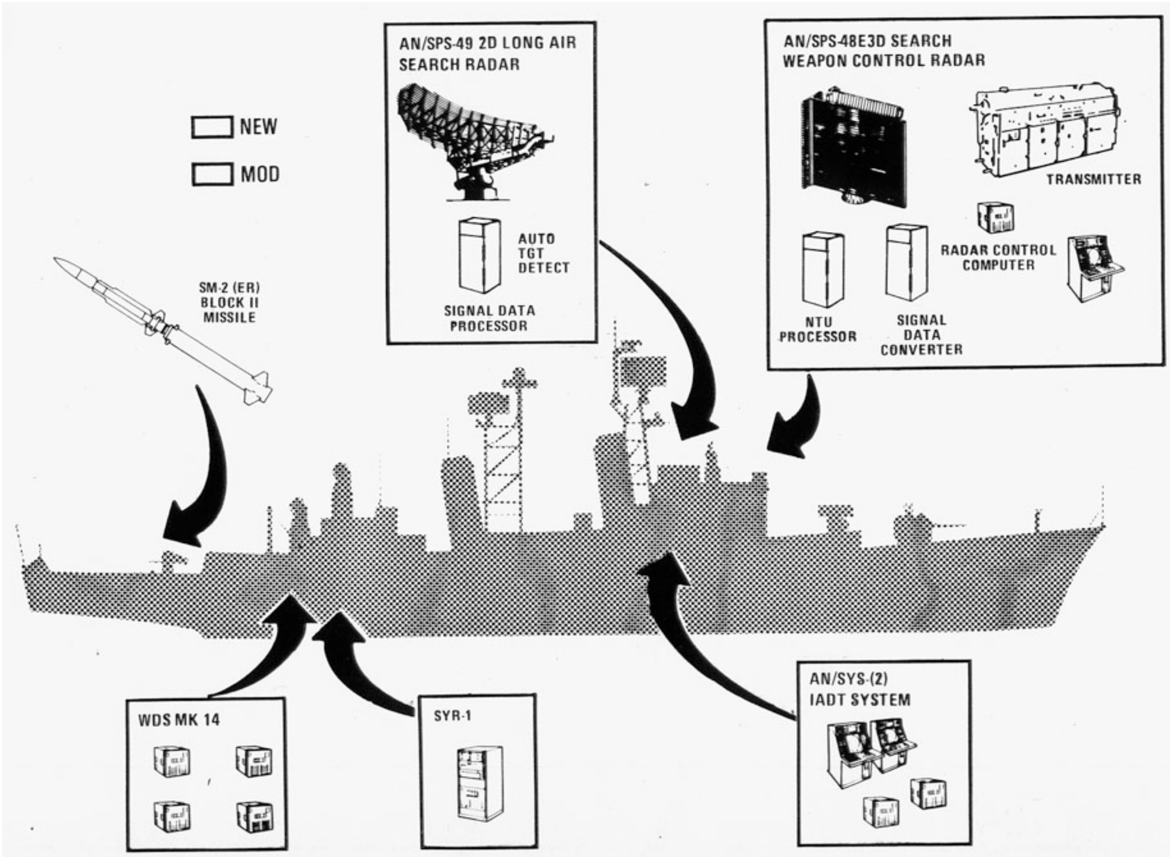


Fig. 3 USS Preble class ship and elements of latest modernization.

operate in each of these regimes, have been speed of movement and penetration beyond enemy lines. In the days of land dominance, ground armies were limited to very slow movement and shallow depths of penetration behind enemy lines because of the technology available to them. When technology permitted navies to move much faster and farther than armies, to out flank them, and to project beyond their lines from the sea, the world entered an era in which Britiania ruled the waves and much of the world. It still took the man on the ground to win wars. Control of the sea alone couldn't guarantee a war, but failure to control the sea might doom one to losing the war. As aeronautical technology grew, speed and depth of penetration beyond front lines increased an order of magnitude. The superior use of air power helped make the US the dominant power in World War II and in the years immediately following. We have now entered the age where space systems will play dominant roles. One of the first of these is the intercontinental ballistic missile. It gets there faster and it reaches deeper. Thus, I submit we have already begun the race for space.

However, I believe it is still the case that any future war, should it happen, is going to be won by somebody on the ground saying "This is my land." And so, let me start with ground warfare, and what we find when we do our gaming. But first, let me say something

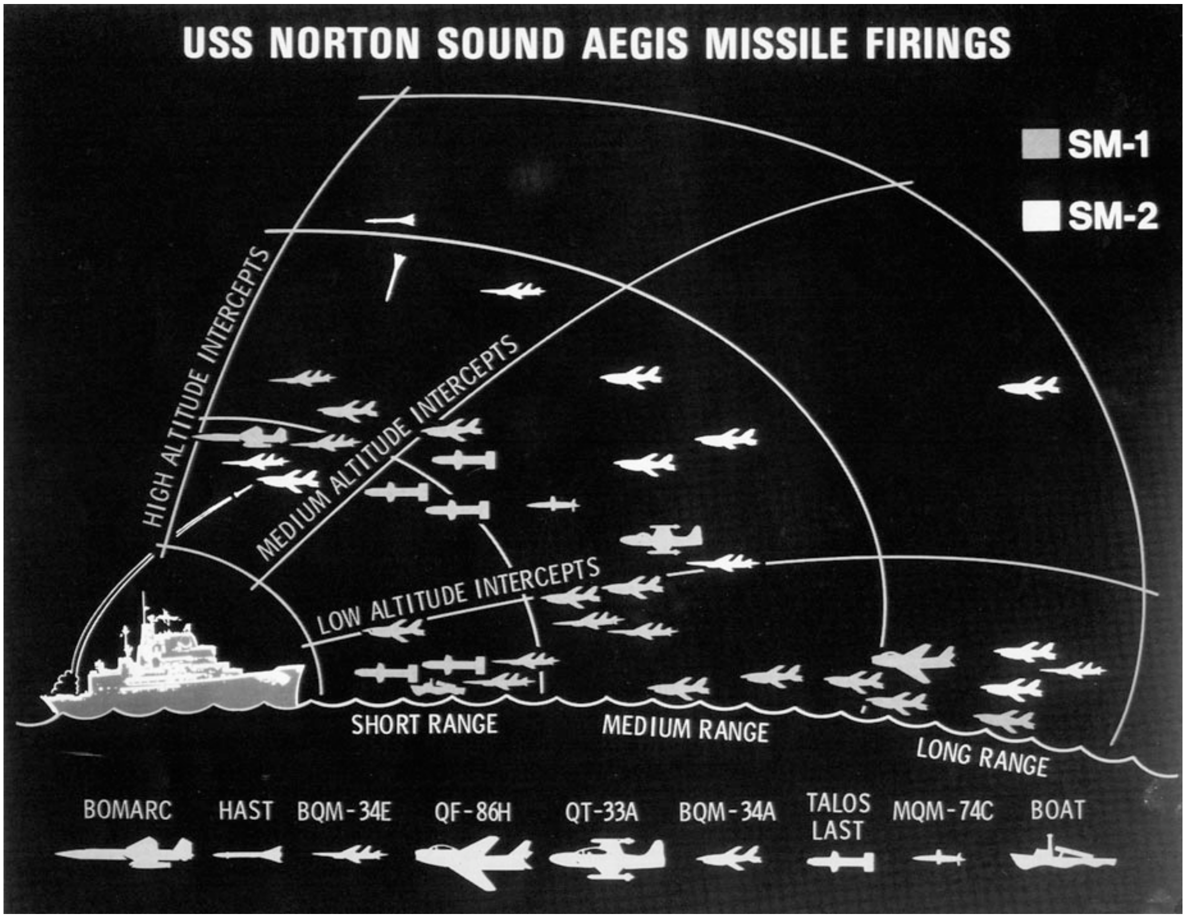


Fig. 4. Early testing of the Aegis surface-to-air missile system.

about deterrence. Most of us in uniform believe that deterrence comes from the other side believing you are too strong to tangle with--period. I think perhaps what is more important as a concept of deterrence--is to be confident that when the other side looks at us--and asks himself what would happen if he were to fight--that he will conclude that the risk of loss outweighs expected gains. And, I believe that gaming is a useful tool with which we can understand how we look to a potential adversary. That is part of the insight we try to gain when we do it. One of the insights we gain is that the issue of deterrence starts on the ground in Europe. Europe--particularly the industrial plant of West Germany--is a prize the Russians would dearly like to have. But it is also a prize that today, they believe, they cannot achieve at acceptable cost. However, from our vantage, the problem with the defense of Germany from the ground point of view is that there is virtually no buffer area between the "inter" German border and heart of Germany. So our concern is that if the Russians mounted an offensive, and gained a significant amount of territory in Germany, with the NATO Alliance not having built armies designed to mount a sustained counter offensive, we might not be able to regain the territory that was lost, and that any such loss would be disastrous for the West.

Further, NATO has historically considered a strong conventional deterrence to be too expensive an option, principally because we believe the technology favors offensive systems in conventional warfare. World War I was the last war in which technology favored the defense: in that war artillery, machine guns and barbed wire made it impossible for the infantry to get across the lines before being decimated, and there occurred that long stalemate with its no-man's lands and the trench lines. After World War I, the development of the tank and the radio, which made it possible to coordinate the tank with artillery and infantry, led to a "rock, paper, scissors" combination which were very effective when used together as combined arms. Each alone had vulnerabilities to one or more of the others. But, when used together skillfully, they yielded a net advantage for the offense. As airplanes became more capable, they could complement or substitute for artillery. However, about the time we were all in college, NATO discovered a technology that seemed to switch the advantage back to the defense, and that was battlefield nuclear weapons: weapons with a hundredth the yield of the weapons used on Japan, but which appeared to be capable of really stopping an army in its tracks. An army on the offense had to be in the open and concentrated, and thus particularly vulnerable. And, so we thought, we had found a inexpensive solution for the defense of Europe, namely a technology that gave the advantage to the defense. The disadvantage came shortly thereafter, when the Soviets developed the ability to make nuclear attacks throughout Europe and then on the U.S., and asserted that nuclear use at the battlefield would likely escalate theater wide and to homeland war. It raised the stakes quite a lot, indeed, to the point where I submit that we are deterred. The problem is, are the Soviets deterred? Today, we believe they are. We believe they are because they see a massive strategic armament on our side that would destroy them, as we would be destroyed. The problem is that the Soviets are building massive defenses. They are building active defenses and extensive passive defenses. And, in an arms control environment, if we significantly limit the number of arms in each side, there is real concern that we would reach a point in which the US, which remains virtually naked, would still be vulnerable and could be destroyed, but in which the Soviets might think, "We now have enough defenses, and your inventories are small enough that we could survive a war." Would deterrence still work? It might. But, to a lot of us, it looks like a shaky form of deterrence.

However, two things are happening that may change that paradigm by the year 2005. On the ground, we are seeing the advance of technology--smart weapons technology--that offer an opportunity to attack attacking armies before they get to the front and thin them out so that even with conventional weaponry we might be able to think about a serious defense of Europe which could be based principally on conventional weapons. The assault breaker program is one program demonstrating some of the potential technologies. It consisted of a radar that could detect and identify tanks up to 200 miles behind lines, something we can't do today, and missile technology that could fly a missile over the tanks and dispense submunitions with homing warheads that can find tanks, identify them from decoys and other vehicles, and strike them from above in vulnerable spots. That and other smart munitions technology might indeed bring an era where technology favors defensive systems because the side on the offense still has to move in the open. He still has to mass his forces if he wants to be successful. If that were the case, you might see a decoupling of our strategy from nuclear weaponry.

At the other end of the spectrum is the possibility of strategic defenses. Now, again I tend to be optimistic about what smart engineers can do because they have done so much in programs I have managed. Strategic defenses probably don't have to be perfect to have a very, very strong deterrent effect. Furthermore in a world in which both sides had defenses, each side might feel that because of the defenses of the other side, the probability of punching an attack through would be relatively slight and the advantage from it even slighter. Further, in an arms control environment, the more you reduce the arms, the more you enhance the power of the defenses. So the plus side is that you could then see both sides motivated to negotiate reductions in offensive arms because they realized such reductions enhance the security provided by their defensive weaponry. The minus side is that it may be that for those defenses to be effective, at least part must be space-based, and that would mean taking war fighting potential to space. That is a longer term argument I leave to others, other than to say that I think that race for space has already begun, and the Russians are working at it harder than we are. I think technology is going to make it essential for a major nation to be capable of exerting its power in space. And, just as in the past, as a Naval officer, I would rather have seen the battles happen at sea than in my homeland, so in the future I would rather see battles happen in space than in our homeland.

I believe, at this point, that I have succeeded in restoring some of today's schedule. Let me close with a quotation from Sun Tzu going back to the 6th century BC. I will paraphrase him a bit: "Attack your enemy's mind; after that, attack his strategy; as third priority, attack his alliances; attack his army only if you must; and, never, ever, attack his cities."

More of our energy should be focused on the business of attacking the Soviets' mind. We have much to gain as a Nation, I believe, if we could succeed in bringing them closer to our way of thinking than we have so far.

INTRODUCTION OF ALLAN S. (BUD) JACOBSON

by Professor Laurence E. Peterson

Department of Physics, University of California, San Diego, La Jolla, California

Keith has gone out of the room, but I wanted to say anyhow, how the start of the UCSD Physics Department really happened, and the methods that were used here during that time. Keith, I think, was doing something called upper atmosphere research at that time. Therefore, he had some knowledge and control of the weather. And, when he asked me to come out and look the place over, it was in the dead of winter and I was at Minnesota, and he conjured up a terrible blizzard the day I had to leave. The plane barely got off the ground. I came here with an overcoat and boots and long underwear and was terribly out of place. Keith and Jim Arnold sat me down on the beach by Scripps and watched the rolling waves the waving palms and the bikini girls, and here I am! All, this business about buildings and academia and everything was totally irrelevant from then on.

Anyhow, it is my pleasure today to introduce Bud Jacobson. Actually, Bud came here about the same time I did in the fall of 1962, and he was one of the schools second or third entering graduate classes, and I was sort of in the second wave of people that came here to UCSD that eventually formed the faculty. Bud worked with me in the formation of the X-ray/gamma-ray astronomy group here. We were all part of the formation at that time and he got his Ph.D. in 1968, spent a year here in a Postdoctoral position, and then went on to the Jet Propulsion Laboratory, where he is now. His thesis was mostly concerned with what was very much a dream at that time, and that is trying to detect nuclear gamma-ray lines from astrophysical processes, which had been postulated by various theorists and which we all hoped, in fact, would really occur in nature. Bud formed a parallel group up at JPL and went on not only to do balloon experiments, but to perform, I think, essentially the first definitive spacecraft experiment that has ever been accomplished to search for cosmic gamma-ray lines. Bud is presently the supervisor of high energy astrophysics at JPL. He holds a position of a senior research scientist which is sort of the equivalent of a tenured faculty position at JPL. And now, I want Bud to tell you a little bit about what he did, and how he did it.

ASTROPHYSICAL GAMMA-RAY SPECTROSCOPY*

by Allan S. (Bud) Jacobson

Jet Propulsion Lab, Caltech, Pasadena, California

Thank you very much, Larry. I wish somewhat that the coffee break had happened after the last speaker because I've got the job of switching around from some very heavy subject material back to some specific scientific activities I have been doing since I left UCSD. To just set the environment, I am going to talk specifically about nucleosynthesis. I have been doing a lot of research looking for gamma-ray lines. I started doing that here, I have been doing the same thing since I left here. But, my major interest has always been looking for direct evidence for nucleosynthesis. There is a great deal of work that has been done in the past several decades on developing all of the processes that would give rise to the synthesis of heavy elements. It is generally thought that explosive processes give rise to materials of mass number greater than four. In any of these processes, one would expect that there would be radioactive debris left at the site and, of course, then there would be gamma-ray lines. Any direct observation of such lines would be a test of the nucleosynthesis calculations. It has long been recognized that this would form a new cornerstone to further development of the theory.

When I was here as a graduate student, what really turned me on to this was mainly the work of the Burbidges, Fowler and Hoyle, and their monumental paper in 1957 on nucleosynthesis processes. Then, Don Clayton at Rice University did some calculations. It was generally thought that the Crab Nebula, which was the debris of a supernova explosion that had occurred some 900 years ago, would have radioactive materials in the vicinity. So, as a thesis project, I went looking for these. At the time, this sort of work was done with scintillation detectors which had rather poor energy resolution. Germanium detectors were becoming commercially available at the time. So, I built a balloon system and put, what at the time was a very large germanium detector, 5 cc's, in a cesium iodide anticoincidence shield, using many of the techniques that were developed by Larry to do this, and went off looking for radioactive gamma-ray lines from the Crab Nebula. Well, I didn't find any then.

I guess I am here now to report that I finally did my thesis right! It has only taken me twenty years, but I have succeeded in finding radioactivity! I did this with an experiment aboard the third high energy astronomy observatory spacecraft. It was a very large germanium detector (Figure 1). There were four crystals, each of which were about 100 cc's in volume. They were surrounded by a cesium iodide shield that provided some

* Bud received the Rossi Prize in Astrophysics in 1986 for the work described here.

rejection of the background and collimation. Because the radiation is highly penetrating, one doesn't really collimate these instruments very well with this technique. This had about a thirty degree aperture. Over the aperture was a thin plastic scintillator to discriminate against charged particles. These detectors have to be maintained cold at about 100 degrees Kelvin and this was done with a solid cryogen refrigerator. The tank contains blocks of frozen ammonia and methane, and the idea was it would last for a year. It actually lasted for eight and a half months. It was a scanning mission and we did a full scan in six months. Then the scintillation counters continued operating as a gamma-ray burst detector and a solar flare monitor, so that the entire mission was about 20 months. It was launched in 1979, on September 20th and reentered on December 7th, 1981.

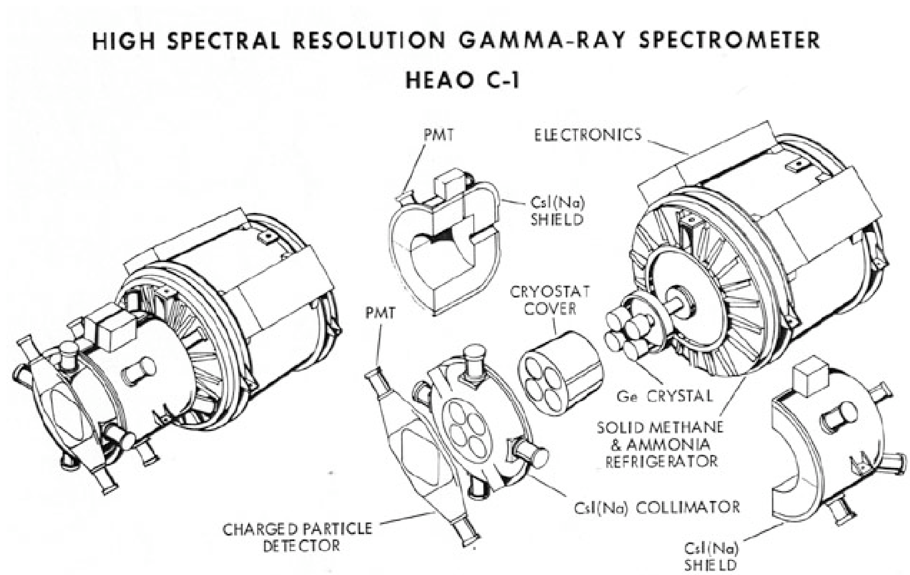


Fig. 1. Very large germanium detector.

The cluster of germanium detectors was a subject of a cover of *Physics Today* (Figure 2). These are quite large detectors and we were struggling with manufacturers right up to the very end to get the high purity detectors, and the largest ones we could. Figure 3 is a picture of the spacecraft that the instrument went up on. There were only three experiments aboard the spacecraft. My experiment was the gamma-ray detector. The other two were large cosmic ray detectors, one to measure heavy nuclei and the other to measure isotopes. The experiment sent back a wealth of information regarding a lot of studies and leading to several interesting discoveries. I am only going to talk about one of them today. It is nucleosynthesis, because I want to be able to cover it in some depth, and not just give you a run down of all of the various topics that we covered in this experiment.

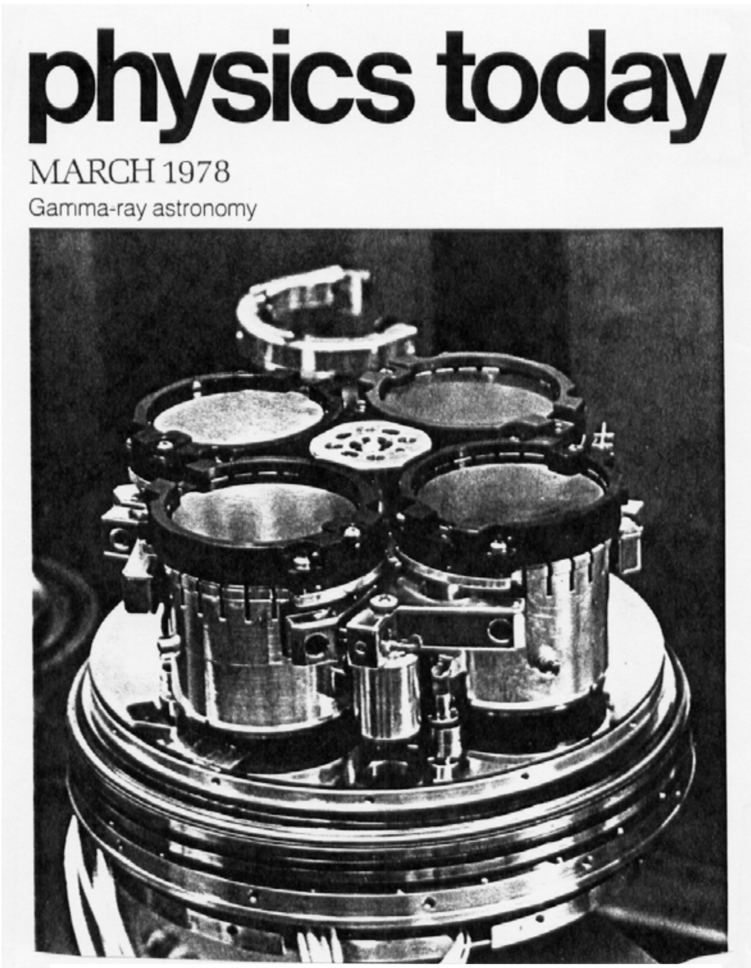


Fig. 2. Cover of Physics Today, March 1978 germanium detectors.

In considering the best way to look for the products of nucleosynthesis, it was pointed out some time ago, mainly by Richard Lingenfelter, who is here at the University, and Reuven Ramaty from Goddard Space Flight Center, that the most likely candidates for discovery were those that had a high yield in any process that created the materials, and secondly a very long half life. The reason for a long half life is that it gives the material a chance to expand into the interstellar medium, and slow down, so that essentially it shares the ambient velocity of the interstellar medium and is thus not Doppler broadened. Additionally, one would have perhaps a chance of seeing the cumulative product of a lot of events. So, with that in mind, the key candidates were, iron-60, aluminum-26, both of which have a million year half life and were thought to be produced mainly in supernova explosions. Another candidate is sodium-22, which is thought to be an abundant product of nova explosions.

In our preliminary search, we looked for all of these and saw no sign of iron-60 or the sodium-22 (see Figure 4). We certainly see aluminum-26 in our spectrum, but our spacecraft was built of aluminum, and so that wasn't surprising! Therein is one of the additional major problems in this kind of research and perhaps why my beard has turned prematurely white! Figure 4 shows a spectrum measured in space. You can see there is an abundance of lines. There are something like 140 lines which are mainly background lines. 60% are produced by the instrument, alone--cosmic rays hitting the instrument and causing secondary radiations Trying to take all of this confusion and pull something real out of it, is a problem. We spent literally years understanding the systematics of this. (Answer to a question: This is a gamma-ray spectrum, Jay. We do individual photon counting. So, basically, it is the number of photons we have observed at any particular energy. OK, so

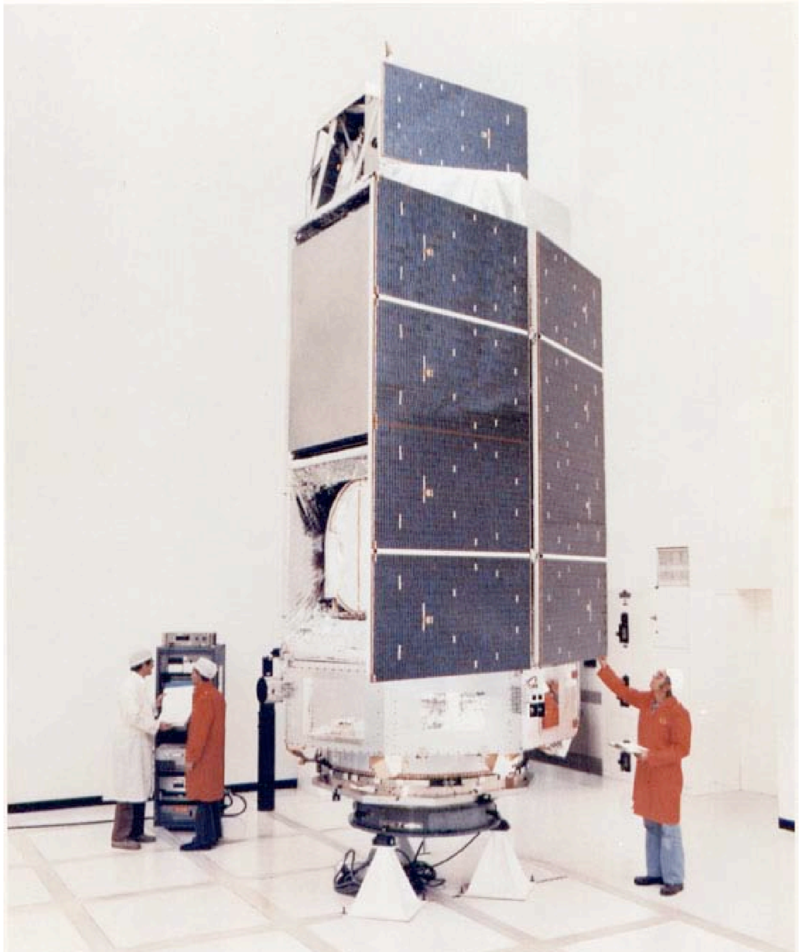


Fig. 3. Satellite that carried the detectors.

this is a spectrum from about 50 keV up to 2 meV and these are characteristic gamma-rays from various materials. I believe this is half an meV here--there is a strong half meV atmospheric component. Many of the lines down in here are nuclear excitations in germanium, and then there are excitations in cesium iodide, and as I said before, aluminum. This is the aluminum-26 line that we know is traced in part to the space craft and the materials in the instrument.)

Figure 5 shows a close up the spectral region around aluminum-26. There is the aluminum-26 line at 1809 keV with another aluminum line close to it. Both of these are produced by neutron interactions with aluminum-27 and it was actually a lucky coincidence that they are so close because we could use one as a control. The line at 1778 keV is not expected to be strong from astrophysical sources. The line on the far left is from natural uranium. The instrument is very sensitive so we have a problem with natural radioactive contamination in any of the materials that we use. In trying to draw out the cosmic component at 1809 keV, we assumed a distribution for the source (see Fig. 6) We assumed that the source was distributed like a high energy gamma-ray at around 100 meV. Essentially this is like the distribution of hydrogen. We then took that distribution and folded in the response of the instrument as it scanned the galactic plane. The block of data we took was a two week segment in which the spin axis of the spacecraft was aligned with the spin axis of the galaxy, so that we were scanning in the galactic plane. The radiation from the galactic plane was in turn modulated by the existence of the earth. Basically, the earth chopped the beam. We next normalized the assumed distribution to the flux radiated from the direction of the galactic center.

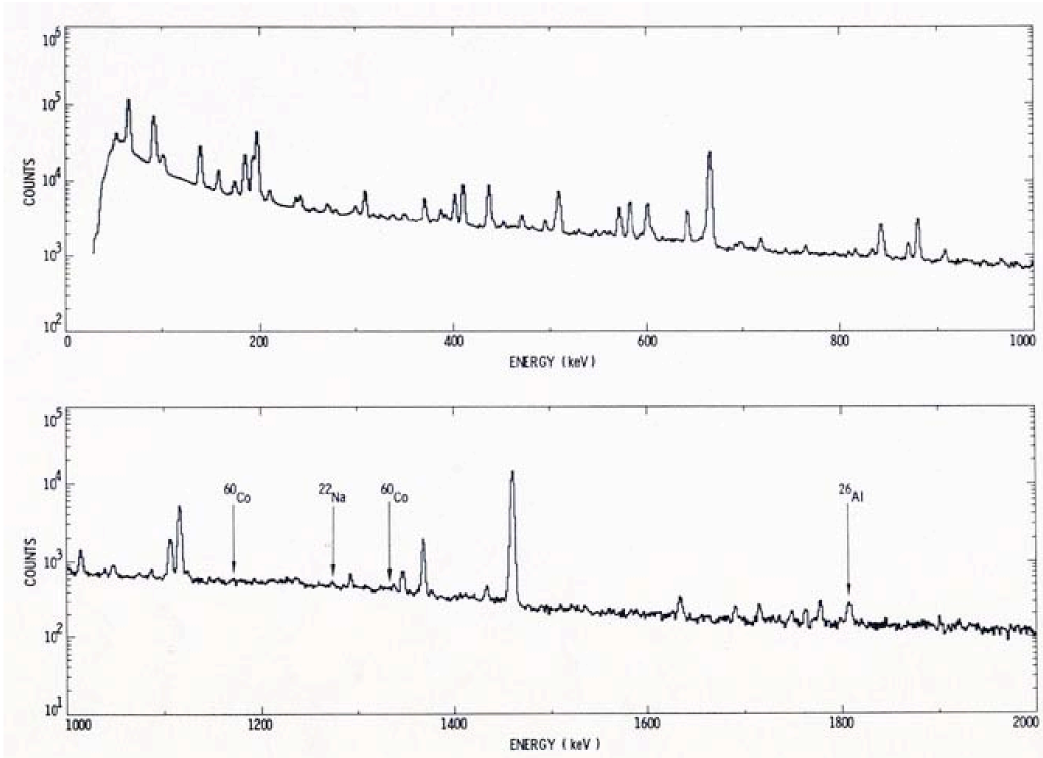


Fig. 4. Gamma spectrum measured in space.

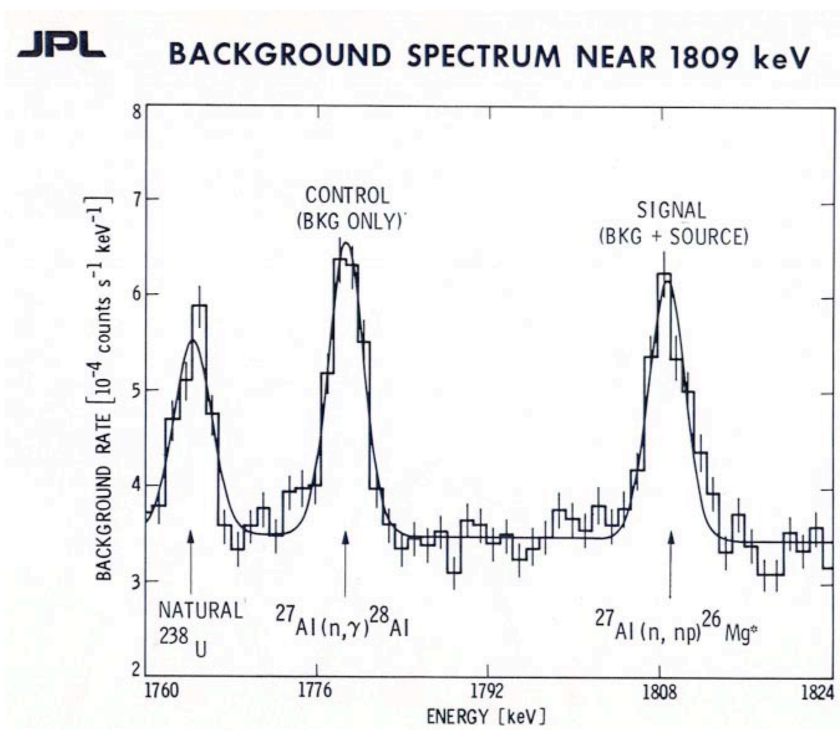


Fig. 5. Details of the spectrum near the Al-26 line.

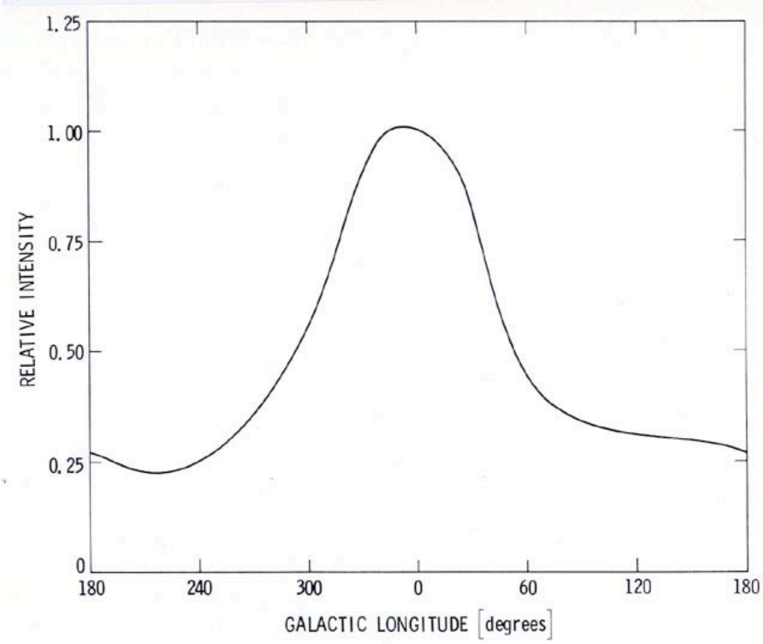


Fig. 6. Assumed source distribution.

Figure 7 shows the net result of this process. We had removed both the naturally occurring radioactive line and the other aluminum line, and were left with the net excess at 1809 keV. This was what we were looking for, the aluminum-26. To convince ourselves it was really coming from the direction of the galactic center, we assumed that the center of the distribution was not at the galactic center and we tried to fit it at off center positions. The net result is shown in Figure 8. This is the flux value that we derived from each fit. It maximizes toward the direction of the galactic center. Figure 9 gives some details about the line characteristics. You can see that the energy we measured, and we have very good energy resolution, is very close to the expected energy measured in the laboratory. The width of the line was less than 3 keV, and this is consistent with the Doppler shift due to the rotation of the galaxy which should be less that about 3 keV. The flux was about 5×10^{-4} photons per cm^2 per second per radian. This implies a mass of aluminum-26 of about 3 solar masses. We arrive at this from the theoretical work done on the high energy gamma-ray measurements. It was shown that the flux F, from the direction of the galactic center is linearly related to a uniformly distributed source function, Q. This relationship gives us a measure of how many decays, or how many gammas we expect from the galactic disk. From that, we conclude that there are about 3 solar masses of aluminum-26 in the galaxy. Another gamma-ray detector has been in orbit for quite some time. It is a solar maximum mission. It is the one that not too long ago was repaired and reactivated. The gamma-ray detector on board, because it didn't require a high degree of pointing accuracy, actually remained active for many years and I believe it is still active. It is composed of a cluster of seven sodium iodide detectors. After we made our announcement, they went back to look, and verified that in fact they also see aluminum-26 and their flux value agrees very well with ours (see Figure 10). This gives us additional confidence that in fact, it is real.

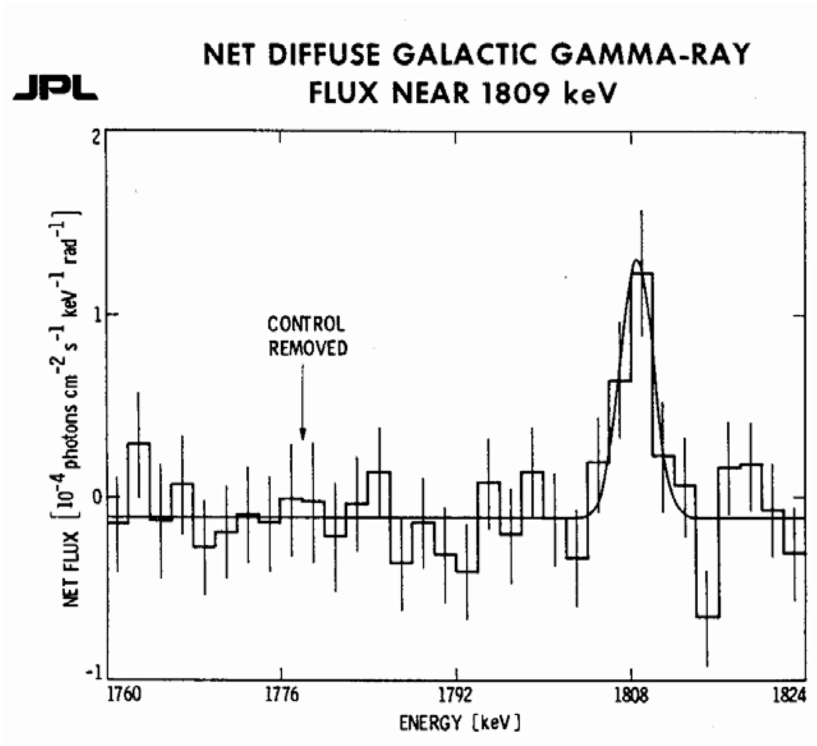


Fig. 7. Reduced spectrum.

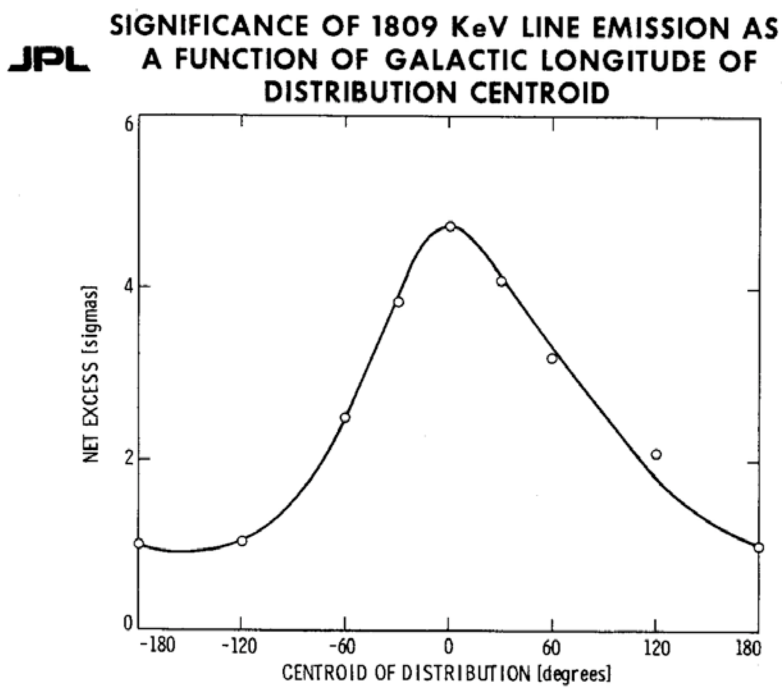


Fig. 8. Fit to angle from galactic center.

JPL

$^{26}A\ell$ LINE CHARACTERISTICS

ENERGY	
MEASURED (HEAO 3)	1808.49 ± 0.41 KeV
FROM DECAY OF $^{26}A\ell$	1808.65 ± 0.07 KeV
WIDTH	<3 KeV FWHM
FLUX	
FROM VICINITY OF GALACTIC CENTER	$4.8 \pm 1.0 \times 10^{-4}$ PHOTONS/CM^2-S-RAD
IMPLIED MASS OF $^{26}A\ell$ IN PRESENT ISM	$\sim 3\ M_\odot$
$\bar{F} = 1.0 \times 10^{-46}$ Q	Q = SOURCE (γ's/s)
$Q = 4.8 \times 10^{42}\ \gamma$'s/s	$\Rightarrow 1.4 \times 10^{37}$ ERGS/S
DISTRIBUTION CENTERED AT	
$\ell'' = (-6 \pm 22°)$	

Fig. 9. Details of the Al-26.

JPL

SMM OBSERVATIONS OF $^{26}A\ell$ GAMMA RADIATION

EXPERIMENT: ACTIVELY SHIELDED CLUSTER OF 7 3"x3" NaI CRYSTALS; ALWAYS ORIENTED WITHIN 10° OF SUN. MEASUREMENT SPANNED 3-1/2 YEARS

RESULTS:

FLUX FROM GALACTIC CENTER: $(4.0 \pm 0.4) \times 10^{-4}$ PHOTONS/cm^2-s-rad

ENERGY: 1804 ± 4 keV

LINE WIDTH: 38^{+21}_{-38} keV

GALACTIC CENTER/ANTICENTER FLUX RATIO: $\gtrsim 2.5$

PEAK OF DISTRIBUTION CENTERED AT: $\ell^{II} = (345° \text{ TO } 25°)$
(99% CONFIDENCE) $b^{II} = (-15° \text{ TO } 10°)$

G.H. SHARE, R.L. KINZER, J.D. KURFESS, D.J. FORREST, E.L. CHUPP, AND E. RIEGER, SUBMITTED TO AP. J., DEC. 1984, REVISED FEB. 1985

Fig. 10. Independent confirmation of Al-26 line.

JPL

^{26}Aℓ PRODUCTION BY SUPERNOVAE

$$\frac{X(^{26}A\ell)}{X(^{27}A\ell)} = \frac{P(26)}{P(27)} \; \frac{\tau}{T}$$

X = ISOTOPIC MASS FRACTION IN THE ISM
P = ISOTOPIC SUPERNOVA YIELD
τ = MEAN LIFE OF ^{26}Aℓ (1.04×10^6 YR)
T = TIME SINCE BEGINNING OF HEAVY ELEMENT PRODUCTION IN GALAXY UNTIL FORMATION OF SOLAR SYSTEM (7.4×10^9 YR)

TO PRODUCE OBSERVED ^{26}Aℓ, THE SUPERNOVA YIELDS REQUIRED ARE

$$\frac{P(26)}{P(27)} = \frac{7.4 \times 10^9}{1.04 \times 10^6} \; 1.0 \times 10^{-5} = 70 \times 10^{-3}$$

CURRENT CALCULATIONS ON SUPERNOVA YIELDS GIVE

$$\frac{P(26)}{P(27)} \sim 10^{-3}$$

THUS SUPERNOVA YIELDS APPEAR TOO SMALL BY A FACTOR OF 10 TO 100

Fig. 11. Theoretical estimate of Al-26 from supernovae.

Now, we turn to trying to understand where this material comes from. As I say, initially it was expected that supernova explosions would produce aluminum-26. It turns out that they don't quite produce enough. In Figure 11, the mean isotopic mass fraction is related to P, the production yield, and T, the time over which that production takes place. When one looks at this ratio, it gives a value for the production ratio of aluminum-26 to aluminum-27 of about 70×10^{-3}. The current theoretical calculations yield no more than about 10^{-3}. So, it appears that what we see is about 10 to 100 times too much to be explainable on the basis of supernova synthesis.

However, when we look at production in novae (see Figure 12) it is a different story. With the rate of about 40 per year, and the mass fraction that is expected from calculations by Hillebrandt and Thieleman, one might expect about a solar mass of aluminum-26. Therefore, we conclude that at least on the basis of what is understood about novae today, that novae could provide all of the material we observe. I must say, that a lot more work has gone into the calculations of expectations from supernovae, than from novae. Thus it may simply be that novae look so good because we are more ignorant about novae at this time.

Several other possible sources have been suggested. Recently, it was found that there is a low energy resonance in the production cross sections for aluminum-26, and in fact one could produce profuse amounts in pulsating red giants, as well as in the very early stages of the very bright stars, the Wolf-Rayet stars. This observation also has some implications with respect to the solar system. In the Allende meteorite, it was found that there were inclusions that had ratios of aluminum-26 to aluminum-27 that were 5×10^{-5}. At the time that this was found it was considered to be extraordinarily high, and thought to be evidence that the vicinity of the solar system had been salted by a nearby supernova and, that furthermore, perhaps it was the supernova event that triggered the collapse of the solar nebula into the solar system. What we have found is that the $^{26}Al/^{27}Al$ ratio in the meteorite is not an untypical value for the entire interstellar medium. We find a galactic average ratio of about 10^{-5}, and even if you consider that the rate of nucleosynthesis has been constant over the last few billion years, that is still not a large enough difference to require that the vicinity of the solar system was unique as far as the interstellar medium was concerned.

JPL

$^{26}A\ell$ PRODUCTION IN NOVAE

CURRENT $^{26}A\ell$ MASS IN ISM = $M_N \, A_N(^{26}A\ell) \, R_N \, \tau$

τ = MEAN LIFETIME OF $^{26}A\ell$ = 1.04×10^6 Y

R_N = AVE RATE OF NOVAE $\cong$ 40/Y

M_N = MASS OF EJECTA $\cong 10^{-4}\ M_\odot$

$A_N(^{26}A\ell)$ = MASS FRACTION OF $^{26}A\ell$

= 2.6×10^{-4} (HILLEBRANDT & THIELEMAN, 1982)

GIVES $M(^{26}A\ell) \cong 1.0\ M_\odot$

THUS NOVAE COULD PROVIDE ALL

Fig. 12. Theoretical estimate of Al-26 from novae.

So in closing and in summary, we have found an extended source of aluminum-26 radiation. We have a confirming observation by the Solar Maximum Mission (SMM). We find about 3 solar masses of aluminum-26. In the present interstellar medium, the average ratio of aluminum-26 to 27 is about 10^{-5}. We don't really have good directional information, but we have fit a lot of other distributions to it and the only one we can rule out is one which is very highly peaked at the galactic center. In other words, there is no point source in the galactic center.

Questions: How much would you expect from the Crab?

Bud: From the Crab? Well, at this point, I don't think very much at all.
At the time that I first did the work, it was thought that Californium-254 powered the light curve of the Crab and hence would give rise to a lot of very long lived isotopes. Right now, the thinking is that it is more likely that cobalt-56 powers supernovae and that these materials have short enough half lives that they are fairly well eaten up by the cloud. I think at this point, no one is predicting that there are really abundant radioactive materials in the Crab.

Question about thesis: inaudible.

Bud: I wish you hadn't asked that! We are actually developing some new detector technologies. There is kind of a limit that one runs into with the use of germanium. Individual crystals can only get so large. So what you have to do is then put larger and larger clusters of these together. There is quite a large instrument being built here now in Larry's group. But, there is a limit to how many you can put in a cluster because of the reliability of the whole system. What we are doing at JPL is taking lessons from high energy physics and are developing liquid argon and liquid xenon time projection chambers. We have an operating detector now in the laboratory made of argon and we are measuring gamma-ray spectra with it. We have also just recently successfully developed--we have been working on this for several years--a segmented germanium detector that gives us some position sensitivity within the germanium, which allows us to further defeat some of the internal background in the detector. So, we are basically trying to find better ways of doing these things.

Question: inaudible

Bud: Yes, I don't think it is really the limitation of the crystal growth that is the problem. Once you have very large crystals, in order to collect the charge liberated by gamma-ray interactions, you must have very large electrostatic fields. At some point you reach a size where it--well, I suppose if the materials were pure enough, and the mobility was up high enough, it might help, but I don't know.

INTRODUCTION OF WILLIAM A. PROTHERO, JR.

by Professor John Goodkind

Department of Physics, University of California, San Diego, La Jolla, California

I came to UCSD myself in 1962, at about the same time that most of you did as students. I was quite a bit younger then Keith Brueckner, in fact, and was not in possession of the self confidence either, but I was in possession of a great many ideas, many of which were never to be realized. But, I was quite fortunate in that the graduate students at that time were very good, and one in particular, Bill Prothero was almost ideally suited to one of the ideas that I had, and has been realized since then. That idea was the development of a new tool for geophysics using some of the techniques of superconductivity. Bill developed that for his thesis. I remember some misgivings at the time that I was a young faculty member bringing a very bright young student to his final exam with no accomplishments in physics. Bill had done mostly instrument development and I was apologetic to my colleagues who reassured me that it was perfectly all right since he had done a good job of it.

Since that time, Bill has gone on to--ahh he worked at IGPP for a few years after he got his Ph.D, and got more into geophysics than we had been able to do with the development for his thesis. After that, he went to Santa Barbara to serve on the faculty in the geology department, which was even further removed from physics, and went on to realize the potential he showed here. He has been involved with development of a major new tool for geophysics, which has been the measurement of seismology on the bottom of the ocean. This is a formidable technical task which he has dispatched with great skill and there are many new results coming out for geophysics as a result of it. So, I would like to introduce him now.

SOME INTERESTING PROBLEMS IN GEOPHYSICS/A PHYSICIST'S VIEW

by William A. Prothero, Jr.

Department of Geophysical Sciences, University of California, Santa Barbara
Santa Barbara, California

Well, it's really a pleasure to be here. For me it's almost an unexpected pleasure because, as I was driving down to San Diego I had mixed emotions about my experience here. I think all of us as students have mixed emotions about our experience here. It was tough, really tough. It was a time when I was just seeing how far I could go in a hard-assed, cruel world. I think a lot of us probably felt that way. [yes, ed.].

But when I came into the reception last night at the top of Tioga Hall, feelings just kind of overwhelmed me--a warmth and a feeling of how important a time this was in my life. Having gotten away from the place for a while; I can understand that better.

One thing that I realized too, looking back over what I have done in the last twenty years, is that I got a hell of a good education here. A lot of things contributed to that: the students, the faculty, the tightness of the structure (we weren't spread all over the place). People like Jim Frohman, too. As far as one person who educated a hell of a lot of experimentalists, Jim Frohman was the key person, and a task master. If you got rust on his drill press, you knew it the next day. That shop was really a focal point for the experimentalists.

At the time, unbeknownst to us, in the areas of geophysics and earth science, there was a real revolution brewing. The understanding of the skin of the earth, the crust, was changing rapidly. I wasn't too aware of it; I was a little aware of it because of a seminar. I don't know how many of you remember the seminar.

It seemed like a good idea to have one of the people from Scripps give a seminar to this new physics group. After all, UCSD was at Scripps. Vic Vaquier gave the seminar in Sumner Auditorium. It was mostly concerned with the magnetometer that he developed, the flux gate magnetometer, and how it worked. It was a thing that physicists would be interested in. At the end, he talked about some of the data he had. He dragged this thing behind a ship over great areas of the Pacific Ocean, and there were little wiggles in the magnetic field, ups and downs, and they were coherent over thousands of miles. People said that was kind of a side thing. What was it due to; it's a weird effect. Vic didn't know. Those observations were part of the beginning of a revolution in the view of the earth, and I'm going to talk a little about some of the ideas that have come out of that revolution.

Back in the thirties, Wigner had proposed a hypothesis of continental drift. It was based on the fact that if you cut the continents out of a map and stick them together, they fit remarkably well. And some of the paleontologists, the people who studied fossils, found that there were similarities at the boundaries on the different continents. So the hard part about continental drift was not that it happened, but how could it possibly have happened. You know the earth is solid, the ocean floor is solid. How did these continents move around? Probably the most viable explanation, at least in my mind--it seemed viable--is that the earth was small originally and it expanded out and the continents---(laughter). But that idea was so far out and there was so much evidence against anything like that happening, that it really didn't get accepted at all. It's the kind of idea you hear every now and then in a really odd-ball talk where some guy from a city college or some way-off place thinks he's figured out something new and it's just ridiculous. I saw one last year at the American Geophysical Union where a guy had found pentagonal symmetry in the continents and was using electrostatic charges to prove where they were; it was just so far out!

But the idea of continental drift had been around a long time. It was also being discovered, from measurements like Vic Vaquier's, that the earth's magnetic field actually reverses. That's kind of scary. We know that the Earth's magnetic field shields us from a lot of radiation, so it's serious that the magnetic field almost goes to zero during the reversal. It hits home. With the continental drift and the crazy magnetic lineations and some other evidence, people started formulating, in the mid-sixties, the idea that the earth really has a very dynamic surface. The continents are kind of floating around because they have a lower density than the stuff beneath them. There is an upwelling at certain spreading centers. The skin of the earth cools and then descends, and the crust of the earth is like a magnetic tape, recording the magnetic field reversals as the new crust moves away from the spreading centers. And we were here at the time, but largely immune to what was happening in geophysics. The geologists think that geophysics is a subdiscipline of geology. I think that is really funny (laughter). But it's also sad, and that's why we didn't know what was going on.

But continental drift has become widely known and widely accepted. The view of the earth as it has evolved from the ideas that were developed in the sixties is that there is a relatively thin skin of hard stuff. It is formed at relatively well defined places in the earth called spreading centers where the hot material comes up, cools, solidifies and then moves away. As it moves, it solidifies new stuff on the bottom, essentially plating out the cool material. Eventually the part of the surface we call the lithosphere gets heavy enough so it sinks. It moves along but there is still some controversy and study about how this motion is actually driven. It is thought there is convection underneath the plates that drives their motion. It is not really known how deep this convection goes. There are great arguments at meetings about Holmel convection and shallow convection and how the whole process works.

There are many reasons for understanding the crust of the earth: economic because of minerals and oil, and a scientific curiosity which drives my interest. If you want to understand the composition of the crust and how it evolves as it moves away from the spreading center, one of the really important things is to understand the spreading center because that is where the material is coming up and where the plate is forming. There isn't much known about the details of the magma or liquid materials or slushy material that is coming up. If you get a chamber full of magma, you get a fractionation like in a still. As it starts to cool, the material that solidified first drops to the bottom, so you get a layering and fractionation. Studying the chemistry of this process is not my subject, but its an area where there has been considerable study and there is still a lot of work left to do.

One of the things I want to bring home with my talk is that in geophysics you are still exploring. Of course you are doing physics too, but sometimes you are not there in the elements exploring unknown territory.

The area where the material is coming up has been studied; some places have been studied in great detail visually. This study is one focus of my research. The Alvin, a submersible, has been used in these studies. It is capable of going down roughly three miles beneath the sea surface. It holds three people inside of a titanium sphere that is six feet in diameter. You have got to be good friends; you have got to watch what you eat the night before because things don't go away. I haven't been in this, but a lot of my friends have. They go down and look at the various part of the ocean floor in great detail. This allows them to make actual geological observations of what is there, which is a first-order thing to do.

Some of the observations they have made have been really spectacular. Everyone has probably heard about these. Right at the spreading center there are incredible hot springs. Some of these are like a chimney, called black smokers with black roiling water coming out; I think the temperature is 300 degrees C. The first time they put their probe in the chimney, it melted. They were kind of concerned because the windows in the submarine that they look out of are just heavy Lucite [laughter] and it is not so good temperature wise. They found not only these vents; they also found sea life: great six and eight foot tube worms, and crabs and clams and all kinds of things. It appears that this sea life is actually disconnected from the sea life on the surface. There isn't an interchange of food. The living things depends on the warm water in the area, on the stuff that comes out of the vents, and on the nutrients that develop around the vents. These are really exciting.

At the spreading center, where the vents are, the two halves are moving apart, material is coming up to the top and the circulation of water is driven by the large temperature difference between the ocean and the crust. Some of the questions still outstanding are whether there are actually magma chambers here which store magma, how these chambers come about, and how the geology changes depending on how fast the two sides spread apart.

The rate of spreading runs continuously from nothing up to, I think, thirteen centimeters a year. The half rate is six to ten centimeters per year. The spread would be up to twenty centimeters in a couple of years and that is quite a lot. So people study the dynamics of this system while the spreading is still going on.

Myself, I'm a little more interested in the larger scale. Since the centers spread at different rates, the effective distance of the material from the spreading center is measured by its age rather than actual distance. This helps when comparing with the history of reversals of the magnetic field for instance. Figure 1 shows a cross section of a spreading center. If you look at the time scale, ten million years at one centimeter per year is one hundred kilometers. So the times are equivalent to quite large distances: hundreds of kilometers. There is also a reasonable relation between the height of the sea floor and the distance from the spreading center. Near the center, the constant heat flux coming up causes thermal expansion and buoyancy, which give the height effect. This area is interesting to me, something I am going to study rather than something I have studied

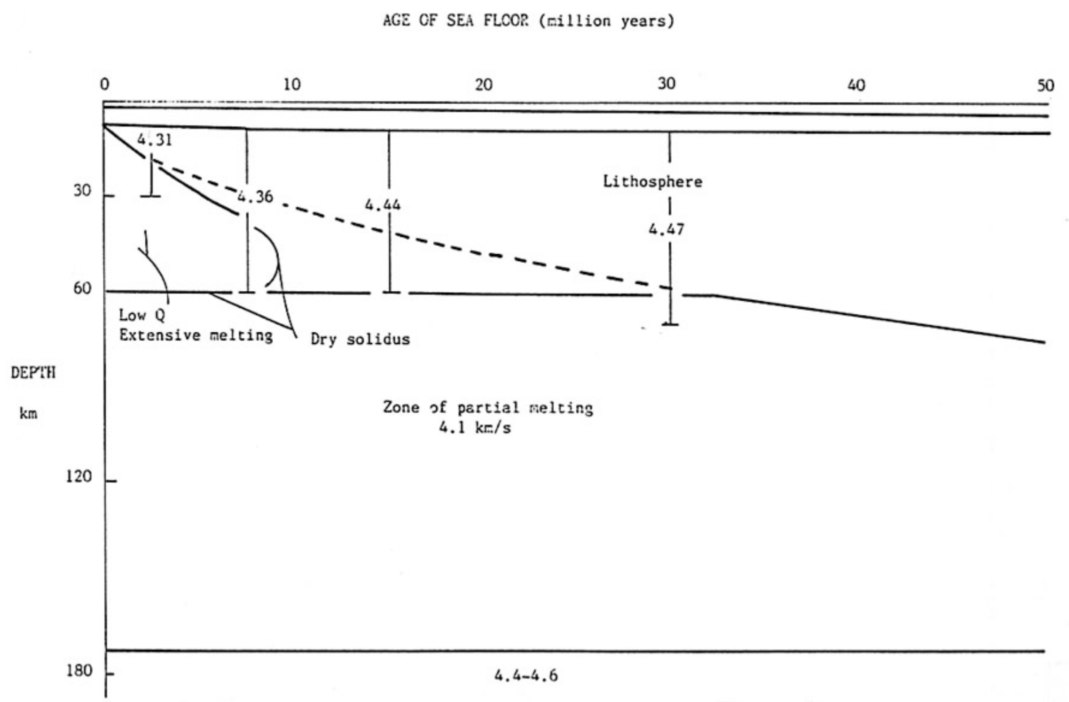


Fig. 1. Cross section of spreading center.

extensively. The question is what the structure is like within a hundred kilometers of the center. This has been looked at using surface waves which propagate from great distances. The wavelengths are 20 or 30 kilometers which doesn't give much resolution, and the propagation over large distances blurs things too. Some specific questions are what are the physical properties, what are the dynamics, what is the Q for propagating waves; are there transition layers that can be mapped, and etc.? What's needed is a method to use to study these regions. Seismology is the tool that is the most powerful and is part of my specialty.

One way of studying the earth is to shoot shots off, make acoustical point sources. Figure 2 shows the Santa Barbara channel region. You make these shots at different locations. You space the stations along the region and model the propagation of the acoustic energy using acoustic ray tracing, Fermat's principle works. The first order approach is just to measure the travel times, vary the estimated velocities until you fit the calculated travel time through the structure. This approach doesn't give a unique model. The next order is to actually use the wave shape itself. If the earth is assumed to be in flat layers, the theory is worked out and the analysis works very well.

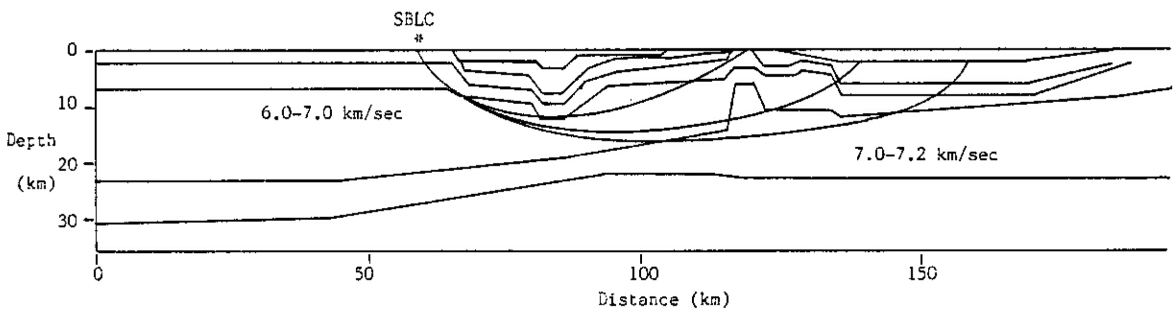


Fig. 2. Santa Barbara channel region.

When you assume the earth is two dimensional layers, you are where the state of the art is now. There is nothing unsolved about the physics of propagating energy through a solid, through pretty much any solid you want. You can always do some kind of numerical analysis that will completely solve the problem. But we all know about the NCAR [National Center for Atmospheric Research] computers and the atmospheric modeling, the numerical modeling. The largest computer is not quite ever enough to handle your model. That means you have to think more. You apply your analytical and mathematical skills to make approximations, to try to make things work on your computer. This is definitely the way you have to go in the earth sciences. You not only have compressional waves, but also shear waves which have the vibration perpendicular to the path. At every boundary there is conversion between compressional and shear waves and it gets very complicated very fast. The mathematics for taking the data and going backwards to the model, for doing a systematic inversion, is not well developed at all. It is in a very primitive stage. It is a little bit simpler to look at very distant earthquakes. The curved lines in figure 2 are ray paths coming up through the structure and incident vertically, although the rays are not generally perfectly vertical. With this simple geometry, if there is an interface or a velocity contrast, an acoustic impedance contrast, you can get conversion and distortions in the wave front like before, but now they can be modeled. This is relatively straight forward and one of the ways that I will be approaching the problem in the future.

You might imagine that one of the really important things is to have lots of shots, lots of sources and lots of measurements. You get information where the ray paths cross and you want a lot of different ray paths. It is a complicated version of tomography, or CAT [Computer Aided Tomography], and you have to have lots of measurements from lots of different directions. Of course, some of the areas are underwater and that makes it difficult. Even on land, getting enough data points, enough stations to receive the signals, is difficult, and the geophysics community, up until recently, has been remarkably quiet in demanding enough funding to put out decent arrays of instruments to really resolve the structure. Part of the reason for this is that the first order view of the earth, the flat layer approximation, is so simple. When you get into more realistic, though still highly simplified models like the one in figure 2, the problem demands large arrays of instruments. A thousand instruments is a reasonable number to do imaging experiments on this kind of structure, and, when you think of putting out a thousand instruments, it is a major effort. It requires a lot of people, a lot of logistics and a lot of money.

When I got into geophysics, the instrumentation was not very sophisticated. My first job, when I went to the IGPP from working in John Goodkind's lab, was to put together a portable instrument for recording seismic data from earthquakes or whatever source. Such instruments didn't exist. The one I built fit in one box. It has a piece of paper smoked up over a smoky kerosene lantern and wrapped around a drum. The drum rotates. A pen moves along on a threaded rod and scratches the smoke off. It worked.

It was really nothing. I put it together and got it working in about three months. The people at the IGPP were just completely amazed. It was a trivial thing after working on the superconducting gravity meter for my thesis. That was my start, but I didn't use it very much. It was a horrible instrument to use. So I actually started working on a project that had been Bob Moore's. When I first looked at the project, I said to myself, only an idiot would do this. And then, a year later, I ended up doing it because Bob left Scripps, and I took over his project. I had missed one of the crucial things: instruments are important. This project was to make measurements at sea. There your laboratory is a small boat with waves from out of Victory at Sea and you are being sick at the same time as trying to perform an experiment. The instrument has to do more of the work since you can't. So there was a little hiatus while I learned to make instruments for the ocean bottom. The spreading centers, faults and a lot of earthquakes are located under the oceans. I was fortunate to be at the right place at the right time to be able to get funding to do some of the first array measurements of earth quakes on the ocean bottom.

When we put the instruments down, we just throw them over the end of the ship. They go down and sit on the bottom. We don't know what the bottom is like. A few years after we made the first measurement, a submarine went down and took some pictures and we realized we were throwing these things down in nasty country. Typically they would end up on a lava bed. The lava comes up solidified in a pillowy form. These pillow lavas aren't flat at all and the instruments don't stand vertical.

The instrument that I probably spent the largest share of my time dealing with and trying to make measurements with, is called an ocean bottom seismometer. It is a package of electronics, batteries, tape recorders and whatnot inside tubes. Glass spheres inside of plastic protectors give floatation. The instrument itself is positively buoyant. It is connected to a tripod weight by two explosive bolts. If either one of the bolts explode and separate, the instrument floats away and goes to the surface. So we put these things off the end of a ship, throw them over and they go down. We can communicate with the instrument through some acoustic transponder system. It is a very simple communication system, similar to sonar. An acoustic signal commands the system to release or give diagnostics. For diagnostics we just listen. We get a binary string; if the signal is two pings, it is a one; if it is one ping, it is a zero. We get back information about the status of the instrument, whether it has recorded events, etc.

There is a radio and a flashing light that turn on when the instrument comes to the surface so that we can find it. Inside, we've got a bunch of electronic cards, low power microcomputers, sensors, tape recorder, batteries and an acoustic system. We take one-second seismometers and modify them so that we can measure periods up to about 30 seconds in ground motion. The band from 30 seconds to 0.1 seconds is useful for doing the kind of seismology where you look at distant earthquakes. One of the facts in designing the system is that when you look at the drum records with this calm line spiraling around it, one thing comes to mind. And it is, boy!--there is a lot of non-data there. If you put that non-data on magnetic tape, it is really horrible because you've got to play out all that tape to find a few events. One of the first things I did was to record digitally rather than follow the standard practice of recording analog signals. This made good quality data. It also allows a microcomputer to interface with the microcomputer, the system could continually sort data in a buffer, and detect whether there was an event. If it detects an event, it can put the data on tape and get the first part before the onset of the event from the buffer. That was one of the unique things at the time.

The idea of the instrument that we have been building is to only record distant earthquakes, not local earthquakes. The distant quakes give data for the more simple form of analysis I described earlier. The instrument distinguishes between noise (the noise level on the ocean bottom is high) local earthquakes and distant earthquakes. So we get the most time on the bottom and save our tape.

That's the kind of work I've been doing. It deals with unknown territory, special instruments, and measurements out in the field. One sort of tradition in geophysics that comes with the field work is that the last slide is a sunset or some beautiful piece of scenery. And this [not shown, ed.] was at the end of one of our cruises off Mexico in one of the Mexican restaurants looking out over the ocean.

INTRODUCTION OF MARIS A. ABOLINS

by Professor Oreste Piccioni

Department of Physics, University of California, San Diego, La Jolla, California

To start with a cliché: it is a novel experience and an emotional experience to introduce this boy who came at the very first beginning of this campus, probably before the Department of Physics started being alive. Now he has become a respected Professor in an institution of outstanding stature.

You see, if a so called University with a great tradition, produces a student who honors himself and the University, they of course attribute it to that tradition, to the years during which have tested their methods of teaching: in other words to their seniority. Here I should make a distinction, which in my opinion is important, between the long range and the short range seniority. It does not do much good to the University of Padua, the fact that it was there for many centuries, but it is valuable today the fact that its Institute of Physics was started with the guiding wisdom of Bruno Rossi.

We, in 1960, had neither the long range nor the short range seniority. We started with this idea that we should have first the graduate school. We felt and were free. Many of us were simply allergic to traditions. I have in mind, for instance, a great intellectual leader by the name of Bernd Matthias.

Note that perhaps half of us had never taught at a university. I had taught in Rome, but a long time before.

So, Maris Abolins came in that situation and I really think he came for the ocean. He was just too clever fishing those abalones. However, he found himself confined to a laboratory, and he just grew in an untraditional campus. We talked to him, we discussed with him, all without any prescribed educational program. He wrote his thesis with Xuong on those "imperscrutable things" that still furnish the justification for building bigger and bigger accelerators, like the one at Fermilab, where Maris does his work.

In fact, he is so productive that it is hard to summarize his rich scientific production. There are 55 good papers in which he is one of the major authors.

One of them deals with the polarization of particles called lambdas and of their anti-particles, anti-lambdas. It is a marvelous example of perfect experimental work. By the way, to do such work not only must one be clever with machines, counters and computers, but one also has to get a good grasp on the theories. One must really understand at least the essentials of complicated particles. Well, clearly I am very proud of Maris, and I am not alone. He has been honored by his peers by being chosen as chairman of many committees with a strong voice in the future of physics.

Our experiment in nontraditional education was, at least in the example of Maris, a clear success.

As to his biography, he was born sometime in Leparja, Latvia. In 1960, he came here from the University of Washington, and in 1965 got his Ph.D. I was his advisor, but much of the credit goes to Xuong. Maris then went to Lawrence Radiation Laboratory, was made an Associate Professor at Michigan State University in 1968, and was made Professor in 1973. He visited CERN, he visited Saclay etc. etc. In his talk, he will show you how one can gain a perception of the "imperscrutable," and how one can reduce the infinitely large things to the rather finite size of our human mind.

FROM THE IMPERSCRUTABLE TO THE INFINITE

by Maris A. Abolins

Department of Physics and Astronomy, Michigan State University, East Lansing, Michigan

I should start by trying to explain what this title is all about. When Brian Maple kindly invited me to give a talk at this Symposium before my former classmates and professors, I readily agreed but could not immediately give him a title. I agonized over it a long time. Clearly it would have to have something to do with my work which is experimental elementary particle physics, but I found it difficult to come up with a title that would be interesting and yet not sound pompous. I was sitting in my East Lansing home having Mai-Tai's with Jim Ball, who was visiting, when the call came from Brian Maple with an ultimatum to give him something immediately. The word "imperscrutable" had always stuck in my mind as representing something indescribably small and it seemed to me that it was exactly the right word to describe one aspect of our current view of elementary physics today: that it is a discipline devoted to the study of the very smallest dimensions of matter. The infinite refers to the close connection that elementary particle physics has with cosmology and the very largest features of the universe.

I want to make a few remarks about the state of elementary particle physics from the view point of an experimentalist who is still toiling in the trenches. I cannot hope to say anything wise as I am not nearly old enough for that. Although I have been working in this field for twenty-five years I like to think that I am still a young man and have many good experiments ahead of me. We experimentalists, unlike our theoretical colleagues, do not end our careers before the age of forty. When the subject of premature senescence comes up I like to quote Oreste Piccioni who, when questioned on this subject, pointed out that we physicists are not "...like the inhabitants of certain kinds of houses who lose their beauty with age and are forced to retire."

In thinking about what to say I could not help reflecting back to 1960 when I arrived here as a very green graduate student and started working for Oreste. Our view of the universe was very different then. We knew about the neutrino which had just been discovered in 1959 by Reines and Cowan, and we knew there were electrons and muons and, of course, their anti-particles. In the strong interaction sector we knew of protons, neutrons, of pions and kaons, and we believed that in some sense the particles were made of each other. This was the notion of nuclear democracy. The number of different types of forces was generally accepted to be four.

In our courses in particle theory, Bill Frazer and David Wong taught us all about the S matrix couched in the language of complex numbers. Wong kept referring frequently to the famous Chinese mathematician Cauchy. From today's perspective much has changed and much that we learned is not particularly relevant anymore. Today, twenty five years later, instead of the one neutrino we know that there are at least three kinds, even though one of them, the ν_τ, has not been observed yet. There appears to be a strong symmetry (Figure 1) uniting the leptons on the one hand, and the basic sub-constituents of hadronic matter; the quarks, on the other. There are six of these objects, the up, down, strange, charmed, bottom and top. The last object has not been definitely established although there is currently a lot of argument on this point. There has been a definite change in our point of view, the notions of nuclear democracy are not very fashionable anymore; we tend to think that there really are basic particles, the quarks, the leptons and the gauge bosons. That is not to say that the sub-structure of quarks is not being investigated. It is one of the forefront areas of research.

$$\begin{pmatrix} u \\ d' \end{pmatrix} \quad \begin{pmatrix} c \\ s' \end{pmatrix} \quad \begin{pmatrix} t? \\ b \end{pmatrix}$$

$$\begin{pmatrix} v_e \\ e \end{pmatrix} \quad \begin{pmatrix} v_\mu \\ \mu \end{pmatrix} \quad \begin{pmatrix} v_\tau\ ? \\ \tau \end{pmatrix}$$

Fig. 1. Leptons and Quarks.

The most remarkable achievement of recent years has been realization that the weak and electromagnetic forces arise from the same gauge theory. A "standard model" has been constructed and all experimental results involving the weak interactions of quarks and leptons are compared to this model. There is even an attractive model for the strong force: quantum chromodynamics (QCD). Twenty-five years ago we could not calculate much in strong interactions with the models that we had and in that regard nothing much has changed. But there is a general feeling among theorists that we are on the right track. This is partly due to the compelling beauty of the structure of the theory. Efforts are under way to unify the electroweak with the strong into one overall theory. This would leave the big problem of gravity to be solved. There are people who think that they have even solved that one. A great deal of work is being done on one of the candidate theories that has recent successes: the theory of super-strings. Time will tell whether the much-sought-after ultimate theory has finally been formulated.

Perhaps the most important change in our conception of elementary particle physics in the last twenty-five years has been the realization that it has some very profound things to say about the very early moments of the universe. In order to understand the evolution of the universe, one has to first understand elementary particle physics. We think now of the various forces in nature as crystallizing out of a single force, one single type of interaction which was all pervasive at the high energies and temperatures associated with the earliest moments of the universe. As the universe cooled the forces started differentiating themselves. Virtually all high energy meetings today have sessions with astrophysicists discussing dark matter, the early universe and things like that. One of the most important questions is whether the universe will continue expanding forever or whether it will one day stop its expansion and enter a period of contraction. The amount of dark matter in the universe is central to this question and whenever particle physicists propose a new kind of massive neutral particle, be it massive neutrinos, axions or whatever, astrophysicists attempt to estimate its effects on the future course of the universe. Leon Lederman likes to tell a story of a graduate student sitting in a pit somewhere at Fermilab in the middle of the night analyzing data that was just being recorded, looking at a sheet of paper and saying: '...ahh, the universe is closed!" Who knows, it may actually happen that way.

Let me expand a little bit on our present situation in particle physics. Where are we and what is there to do? At the present time all of the existing experimental data is in agreement with the standard model. This was not the case a few months ago when it appeared that some fundamentally new kinds of phenomena were being observed at the CERN collider. Unfortunately, these new hints of physics beyond the standard model, which whetted everyone's appetite, have gone away. So what are the big questions that we should be attempting to answer? Apart from the fact that the standard model has a lot of parameters that cannot be explained from basic principles, there are a number of specific items that have to be investigated. First of all there is the question of the Higgs boson which is responsible for the symmetry breaking that mixes the Z^0 and the photon. The mass of this object is not fixed by the theory and this fact has a certain appeal. Any bump in a mass distribution that does not fit in any conventional scheme can be called a Higgs without fear of contradiction. Last year we saw an example of this phenomenon from an experiment at the German storage ring PETRA where a bump with a mass of 8.3 GeV had been seen. I happened to be at a meeting in Santa Fe where a whole session was devoted to this particular object. Some new experimental data was to be presented followed by a couple of theoretical talks purporting to place the object in a proper theoretical context. Unfortunately, the experimental talks presented evidence against the existence of this effect and I don't remember whether the theorists had the courage to continue or not. If nothing else, this anecdote pays tribute to the fecundity of our theoretical colleagues.

Another open question today is whether the neutrino has a mass. A large number of experiments are being performed looking for neutrino oscillations and hence evidence for a mass.

Another active field of research involves the search for magnetic monopoles. There is a large effort underway here in La Jolla to search for them in cosmic rays using a clever technique invented by Kroll and Drell. To summarize this field, I think it is fair to say that apart from the one monopole that hit Palo Alto a few years back, no others have been detected. Well, you have to be lucky!

The question of whether quarks have a substructure is one that may never be definitively answered. All that an experimentalist can do is to put smaller and smaller limits on their size. Most theorists that one talks to find it absolutely distasteful to consider that one must continue peeling never-ending layers from an onion like object. It would be nice to see this thing end somewhere! So far, there is no evidence that quarks are not pointlike objects.

A number of schemes have been advanced that seek to extend the symmetries that we perceive to be operating at the level of elementary particles. Some, such as supersymmetry, attempt also to incorporate in a natural way the gravitational forces. All of these models invariably predict new families of particles that, in principle, could be detected in the laboratory. At this time there is no evidence for any of these symmetries nor for the particles associated with them.

The ultimate goal of all theorists is to find a grand unified theory, a theory of "everything" which would explain all the interactions in terms of, perhaps, one single parameter. It would unify in a single scheme the interactions of the leptons and quarks. The only experiment that bears on this question is the measurement of the lifetime of the proton. So far, despite heroic efforts, the proton appears to not co-operate and remains stable with a lifetime in excess of 10^{32} years or so. If it does decay, its lifetime may be beyond our abilities to measure it. Some of us always knew that the proton was stable but it had to be measured anyway.

Perhaps because of these tantalizing questions and perhaps because of the great progress that has been made, the field of experimental high energy physics is at an extremely high level of activity. The past ten or fifteen years certainly deserve the much overused title: "the golden age". Since 1974 we have seen a frenzy of experimentation and discovery in our field. This is likely to continue at least for the next decade with many new accelerators and experiments coming on line. I will allow myself to digress a little here for the benefit of colleagues who have not followed this field for the last twenty five years and try to show you how things have changed in the way we do physics. Figure 2 is a picture of Fermilab, a place a number of people in the audience know and love--or maybe not love so much. The scale here, given by the radius of the Main Ring is one kilometer. When I entered graduate school here in l960, the Berkeley bevatron was one of the largest machines and its size could be compared to the booster in Figure 2 with a radius of perhaps 10 meters.

With the increased size of the accelerators we need larger and larger detectors. Fortunately all of them do not scale linearly with energy. If they are calorimeters, that is detectors that absorb the total energy of the particles, their dimensions grow only logarithmically. Even with that the detectors are getting big. As an example, Figure 3 is a photograph of a detector we are currently using in a neutrino experiment at Fermilab. The relevant dimensions of the detector are about 20 meters from one end of the calorimeter to the other. There are six to seven hundred individual detectors with as many as 500,000 channels. As you can see, things get big and as you might imagine take a long time to assemble. This particular detector took us nearly three years to put together. The calorimeter weighs about 400 tons and the iron toroids, used for measuring muon momenta, another thousand tons or so. It is a big industry.

You might ask what is it that we are trying to measure with this apparatus? The answer is that we are measuring parameters of the standard model in order to search for disagreements with its predictions. One important question is: do the neutral currents "see" the same structure of the nucleon as the charged currents? In the standard model the couplings are completely calculable in terms of one constant, θ_W, the Weinberg angle and given

Fig. 2. The Fermi National Accelerator Laboratory.

Fig. 3. Neutrino detector in laboratory C at Fermilab.

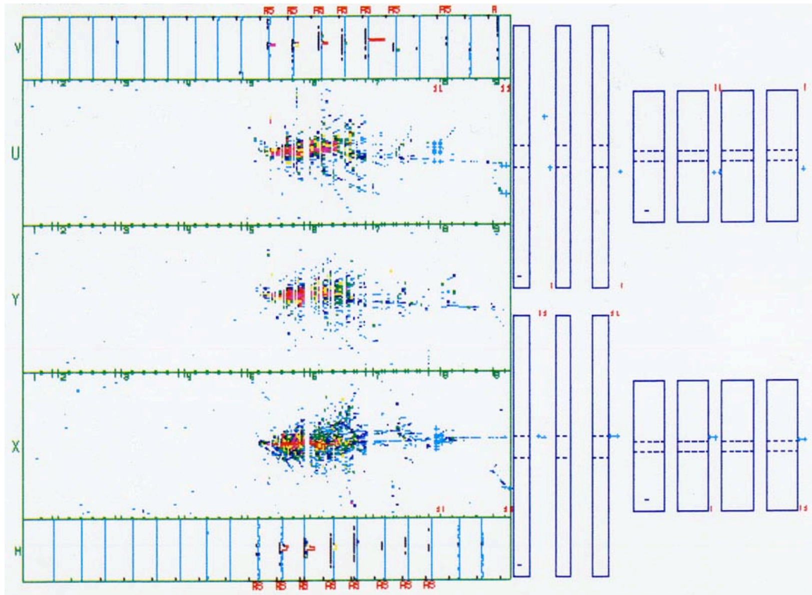


Fig. 4. A typical neutrion event in Lab C detector at Fermilab.

the charged current results the neutral current structure functions should be completely predictable. Figure 4 shows a typical event in our detector caused by an incident 150 GeV neutrino. The energy deposited in the calorimeter is color coded to indicate its density and the penetrating muon is indicated by hits in the chambers in the toroids. Events such as this are produced every spill and sometimes I cannot help reflecting on the original experiment of Reines and Cowan who first discovered the neutron. What a contrast was their patient wait beside a tank of cleaning solution outside a power reactor attempting to discover the tell-tale scintillation signals from a few evanescent interactions. Anyone doubting the reality of the neutrino need only to observe a few of our events. A detailed analysis of events such as this reveals that at the present level of precision, the standard model prediction of the neutral current interaction is in good agreement with our data. The structure of the nucleon as seen by the weak neutral current is exactly the same as that probed by the weak charged current or the photon.

Another measurement that we can make involves the observation of two muons with the same charge coming from the interaction. The rate of known sources of events is easy to calculate and it appears that there is a source of these events that we do not understand. The measurement is extremely difficult, complicated by various backgrounds but, nevertheless, it is one that must be made as it is one of the few reactions that may give a hint of physics not predicted by the standard model.

Leaving today's experiment let us look at what the future has in store for us. Figure 5 shows a picture of the first large colliding beam detector at Fermilab the so-called Colliding Detector Facility or CDF. This experiment will analyze the debris from 1000 GeV protons striking 1000 GeV antiprotons. It is a more sophisticated version of the famous UA-I detector at CERN, which discovered the Z^0 and $W^{\pm}$ gauge bosons. While I am on the subject of Fermilab collider detectors, I have to show a drawing (Figure 6) of another apparatus, that of the second large Fermilab detector in the DO area. Listening to Skid Masterson's talk reminded me of this detector. We now have three uses for depleted uranium. Besides making bullets that can penetrate tanks and missiles it turns out that depleted uranium is an ideal material for use in calorimeters. There are two reasons for it: first it is extremely dense and its use results in a very compact calorimeter. The second reason is perhaps the more important of the two: when a nucleus is struck by an energetic particle it flies apart and the resulting debris carry off varying amounts of energy which is often invisible to the active medium in the calorimeter. The advantage of uranium is that it fissions readily when struck and in the process releases neutrons and gamma rays which can be detected in the calorimeter. The bottom line is that the statistical fluctuations in energy loss that limit the energy resolution in ordinary calorimetry are eliminated in uranium, event by event. Thus calorimeters constructed of uranium have optimal energy resolution. As a consequence there is a whole industry developing devoted to reprocessing uranium metal for use in our detectors. One of the problems is that there is only one supplier in town, unless one can get Russians as collaborators as some people have. Our experiment needs a relatively modest 350 tons and the total worldwide is maybe a factor of ten higher.

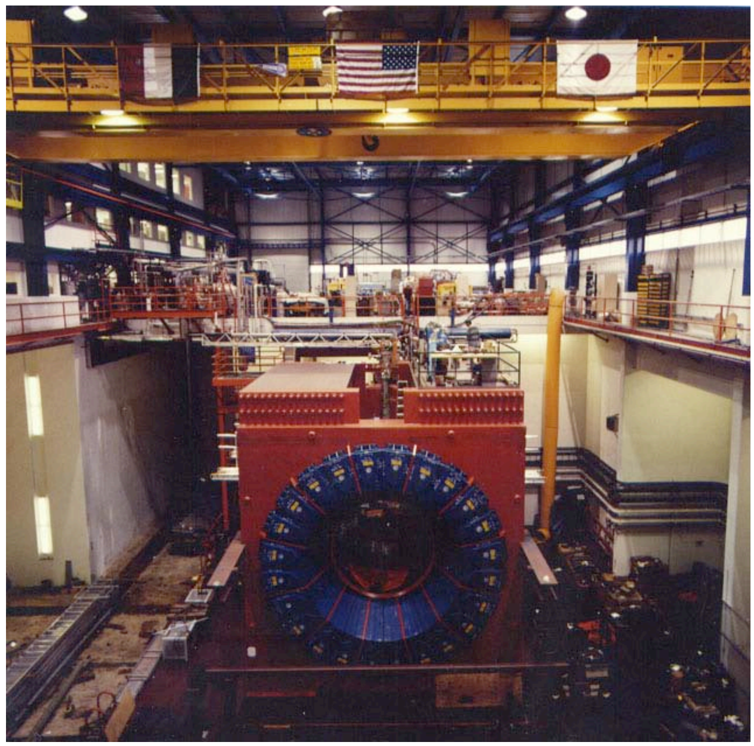


Fig. 5. The central detector facility at Fermilab.

At higher energy life gets harder in other important ways. I recall the early days in La Jolla when Phil Yager and I would scan events that had three and four outgoing charged tracks. Now with 3.2 ergs in the center of mass all hell breaks loose as can be seen in Figure 7 which depicts a Monte Carlo generated event of 1000 GeV protons striking 1000 GeV antiprotons in the CDF detector. Fortunately, we have profited from the CERN experience and have learned that one really does not have to analyze all the crummy little tracks. One simply erases them and concentrates on the jets. It turns out, and this is perhaps the most important lesson from CERN: one can do physics at these energies, one can crash these garbage cans together and yet study the physics of hard collisions of the basic partons.

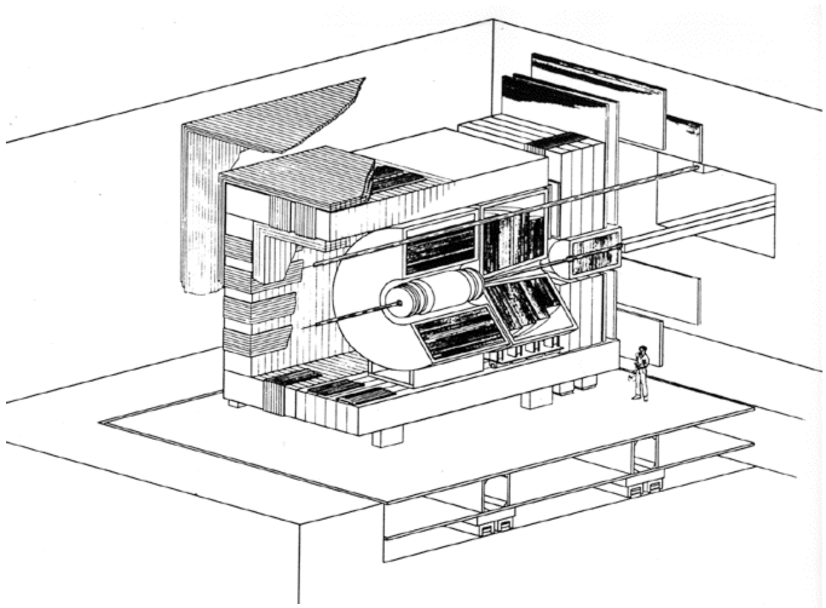


Fig. 6. The DO detector at Fermilab.

Let me now turn to the immediate future and consider the world situation. Figure 8 is a world map with all of the existing and future facilities marked. Among the highlights that we should note is the Stanford e^+e^- collider, SLC, that will probably be operational in 1987. If this technique works they will beat everyone else to the punch. It will not be easy as they are pushing a number of techniques to their limits to collide bunches of beams whose transverse dimensions are 1-2 micrometers. The Fermilab collider is in a test mode even now and can be considered nearly operational. There is an ambitious electron-positron machine being built at a Japanese laboratory near Tokyo. Due to limitations in laboratory space and RF power their beams will be limited to 35 GeV each. If we keep in mind that the point-like cross-section decreases with the square of the energy then we can appreciate that there interaction rate will be very low unless the top quark mass is sufficiently low to produce a toponium resonance in their energy range.

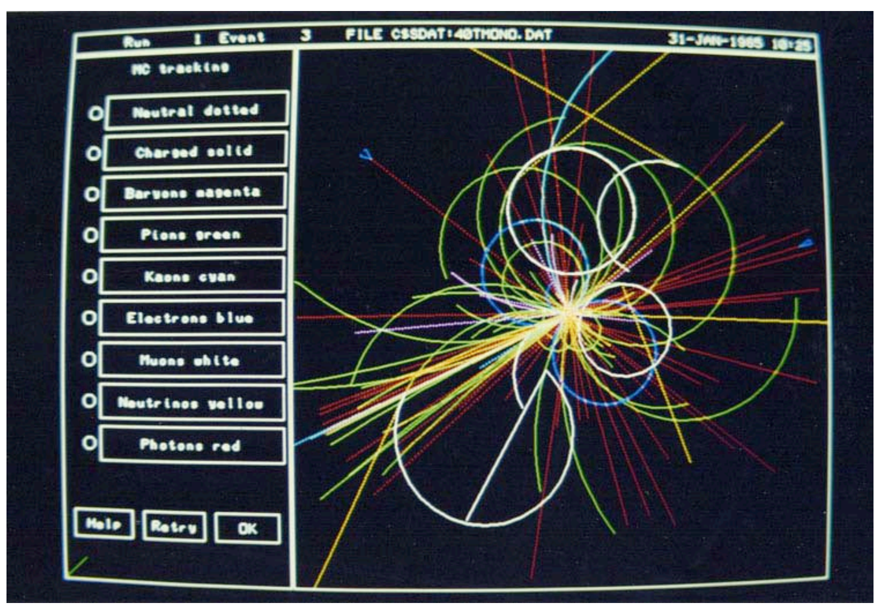


Fig. 7. A Monte Carlo generated event with 1000 GeV protons on antiprotons.

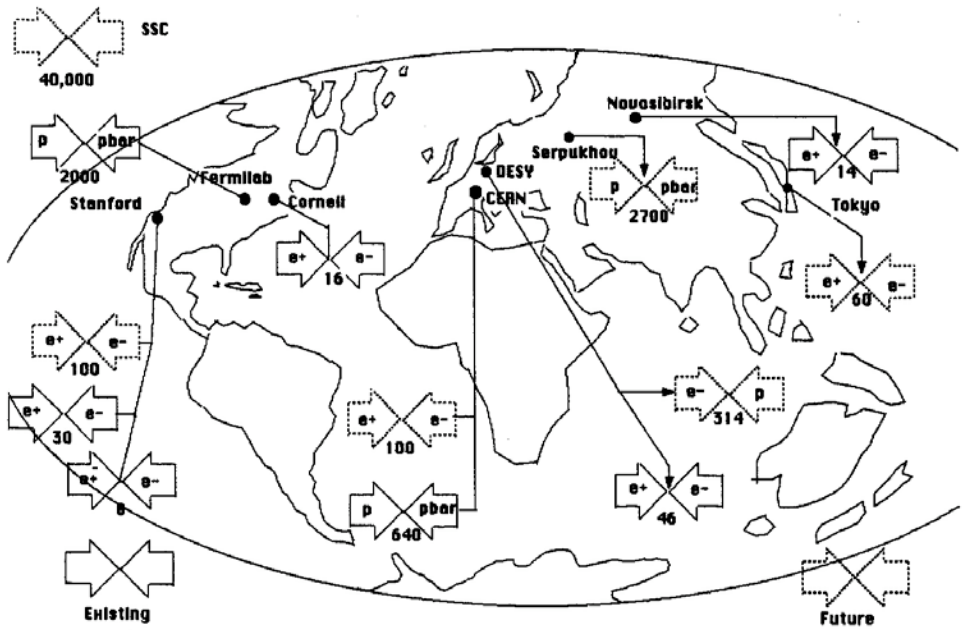


Fig. 8. World map showing new and existing accelerators.

The first accelerator built on a new scale is LEP at CERN, an electron-positron machine. Figure 9 is an aerial view of the region surrounding Geneva, Switzerland with the LEP ring superimposed. The scale of the ring is set by its radius of about 4 kilometers. One can clearly see the Geneva airport and the nearby Jura mountains which border one side of the valley. When I was at CERN I lived in France, near the Jura. Had LEP been in existence and if I had chosen my experiment judiciously I would not have had to drive to work. I do not have a scale drawing of the SSC but we can imagine scaling the LEP ring by a factor of four or five. Clearly, the narrow Geneva valley could no longer contain it and it would probably cross several borders in that part of the world, giving a new meaning to the notion of an international accelerator.

The big question in our science is: what about the future? Where can we go? When discussing the future of accelerators it is customary to show the so-called Livingston plot (Figure 10) which shows the highest accelerator energy as a function of calendar time on a semi-log scale. It demonstrates graphically the exponential increase in the center of mass energy that our accelerator builders have achieved over the years. We see new techniques being developed, losing their viability and being replaced by others in a constant quest for higher and higher energies. Going back to 1960 we can see the alternating gradient machines coming on line, increasing in energy until about 1970 when the CERN intersecting storage rings (ISR), the first proton storage ring machine, takes over the energy lead. This is the first point on a new Livingston chart for colliders, shown in Figure 11, which continues today with a point for the 40 TeV SSC being positioned somewhere in the 1990's. I turn to this curve for solace after discussions with theoretical colleagues who maintain that we are facing a great desert, that after the Z^0 and $W^\pm$ there will be nothing until we reach the Planck scale of 10^{19} GeV. We should not be too discouraged by these predictions because following the curve on the Livingston plot this energy should be achieved sometime in the 22nd century. Realistically, it is very unlikely that this exponential growth in energy can be sustained much longer.

Fig. 9. Map of the region around Geneva, Switzerland showing the location of the LEP ring.

Before quitting I should make some comments on high energy physics as a study of sociology. Everything in this field is getting bigger: the experiments, the number of participants, and the time-scales. As an example, the LEP experiment L3 is said to use more steel than there is in the Eiffel tower and all the continents of the world are represented among its participants. There are so many physicists working on this experiment that there are meeting where only group leaders participate and name tags are mandatory. As anyone who has read recent publications in this field knows, the number of authors per paper is increasing dramatically. Since my career has paralleled those of many other people of my generation I thought it would be interesting to examine this effect by taking a few of my publications and plotting the number of authors per paper as a function of the calendar year (Figure 12). This "Livingston" plot parallels nicely the energy plot in Figure 11.

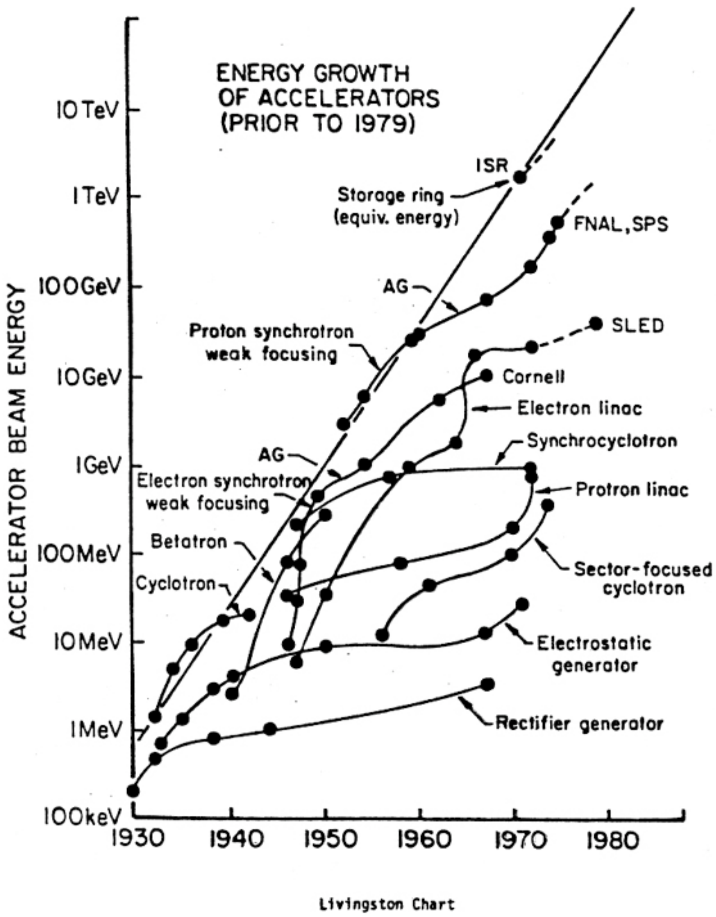


Fig. 10. Livingston plot showing, on a logarithmic scale, the highest accelerator energy versus the calendar year.

Let me end with a few comments on the Superconducting Super Collider (SSC), the proposed 20 TeV on 20 TeV proton collider. To those of us working in high energy physics it is clear that the leadership in this field has shifted to Europe. Until about 1970 all the important experiments were being done in the United States, Europe was in second place and the Russians, in Oreste Piccioni's words"...could be ignored to first order." Now Europe has surged ahead and the Russians are working hard to catch up. The question facing us is "How can we overtake the Europeans and once again become competitive with them?" The simple answer is, "build the SSC." Unfortunately, the price tag is very high, somewhere around 3 billion dollars. This is a number that only someone like Skid Masterson understands well. It will probably buy a small frigate without its missiles. It is a number sufficiently large that it cannot be hidden in the DOE budget and it will be difficult to find in a financial climate where the emphasis is on reducing deficits. Our field is balanced on a knife's edge: we either construct the SSC and regain our leadership in this frontier field or we pack up and do our next generation of experiments in Europe. With rare unanimity, the high energy community has rejected this second option and has united behind the SSC. We must now convince our colleagues in other fields of physics that the SSC will not drain away their resources and that it is in their interest that we maintain a pre-eminent position in this vital, frontier field. Let me end on this hopeful note.

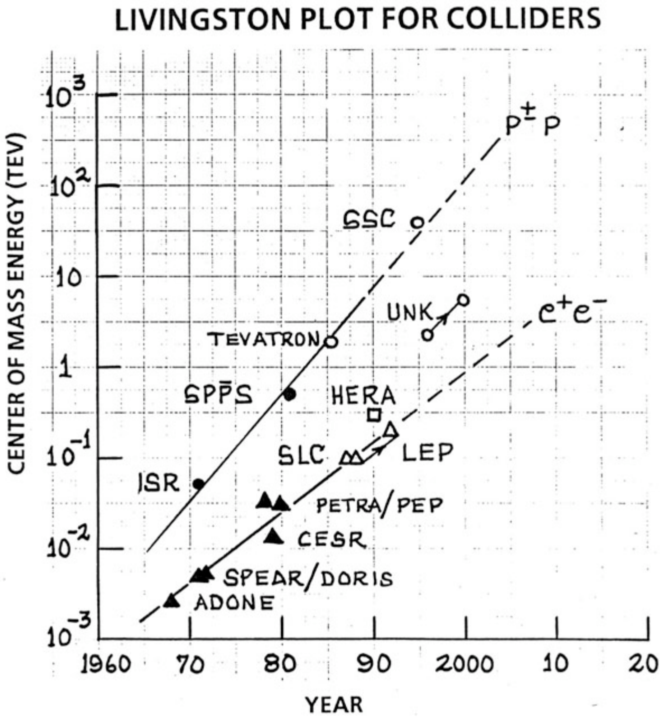


Fig. 11. Livingston chart for colliders.

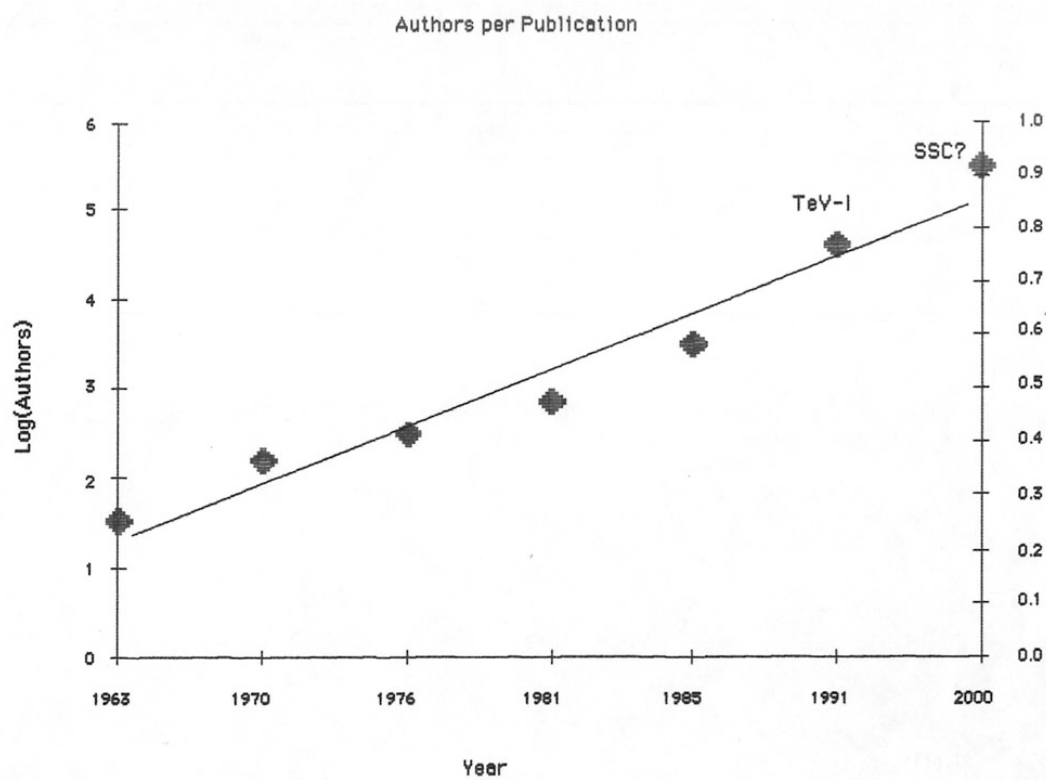


Fig. 12. A "Livingston" plot of authors per publication versus calendar year.

INTRODUCTION OF HERBERT J. BERNSTEIN

by Professor Norman Kroll

Department of Physics, University of California, San Diego, La Jolla, California

Apropos of the last talk, there are, of course, a lot of young men who are going into experimental high energy physics, nevertheless. In fact, we tried very hard to hire one last year, and he had many job opportunities. My son is also going into it. So, lots of people certainly are interested in this field.

I arrived here in 1962 having come from Columbia University. Twenty years before that, I received my Bachelor's Degree from Columbia College, so I was very interested when the year after I came here two bright young graduates from Columbia College elected to try their fortunes at UCSD. One of them is Peter Feibelman, and the other one, who is also here today, is Herb Bernstein who I am introducing now. Herb did his dissertation research with Frank Halpern, who was not able to come today. I think the title of his dissertation was "Point Interaction Theories of Relativistic Interactions" or something like that. He also wrote a number of the papers while he was a graduate student here. One of them was on interference phenomena of fermions and that is really a topic in which he is still interested in and still works on.

Well, after leaving UCSD, he went to the Institute for Advanced Study and has had a quite varied career. He spent some time in Washington advising various scientific advisory committees as a staff member. But, for a long time he has been a Professor of Physics at Hampshire College in Amherst. That is actually one of a group of five colleges at Amherst that includes Amherst itself, the University of Massachusetts, and the member of the consortium which is devoted to innovative education, Hampshire College. Currently, Herb is on sabbatical leave at MIT where he is working on interference phenomena in neutron scattering experiments. He is also collaborating with Victor Weisskopf on writing a book. This book is called "Qualitative Physics," and some of what he is going to tell us about today is related to the kinds of issues which are considered in that book.

Herb has also received a number of honors. He shared the Proctor Prize with Victor Weisskopf. That is a prize which is awarded for scientific research and presenting it in a way which is accessible to the worldwide intellectual community. He has also received a number of other honors. He is presently a Kellogg Fellow, something which he will hold for three years. But, I don't want to take the time to list all his honors, otherwise we won't have a chance to hear his talk. So, he is going to talk to us now about "The Search for Simplicity."

PURSUIT OF SIMPLICITY

by Herbert J. Bernstein

*School of Natural Science, Hampshire College, Amherst, Massachusetts and Physics Department, Massachusetts Institute of Technology, Cambridge, Massachusetts**

Hello. Thanks. I know that we are close to the time that we should have quit, in fact, there is exactly minus seven minutes left in the session. But, it is easy for me to talk that fast, I have been talking fast for a long time. (laughter) You all know that, so come along with me while we talk about the search for simplicity. I'm writing a book by that title with Victor Weisskopf and it tries to explain all different kinds of phenomena for which we can get some solid body of data, like that in the American Institute of Physics Handbook. In terms of back of the envelope calculations, our motivation is multifold (Figure 1): to understand what goes on without having to be an expert, to explain it to other people, to motivate people to try to understand things quantitatively, to educate students in the important techniques of magnitude estimation and qualitative explanation, and finally to counter the trend toward over-specialization (that current education brings) by allowing people to work on topics that are far from their field. Today I will be talking about something that is far from my fields of expertise, and that is great for me. It may be a little bit embarrassing, but I can handle that too. I appeal to you all to find ways to improve the calculation or to shoot it down or to improvise, and, in fact, let me give you two more motivations for this kind of work. "Ultimately," Feynman once said "if you had to wipe out all of modern science and save just one sentence, what you would do is save a sentence something like this,--the physical world is made up of atoms, and their behavior is governed by quantum mechanics." If you save that, put it in a time capsule blow out everything else and then go back you would find that you have saved the kind of unifying conceptual aspect which allows reconstruction. Other people might substitute 'elementary particles' for 'atoms'. Some quick phrase like that would be the thing that you would save, and the work that we are doing is to write a book that shows how, starting from something as simple as that, you might be able to cleverly figure out how things work in the world. *Finally*, we are really aiming to reconnect the work that physicists do to everyday phenoma, to see, ahhhh--- Well, it doesn't spell right, but you know what I mean, (laughter) to see phenomes. I mean phenomena of course; to reconnect physics from what we do in our very arcane laboratories and studies to such things as why the sky is blue; how come insulators in general are pretty opaque, and yet some insulators like glass are transparent; why metals are shiny; why things are as hard as they are, or soft for those materials that are soft, and so

* Acknowledgement: Thanks to Professor C. G. Shull for support and hospitality at the MIT n^{o} diffraction lab. Work supported in part by NSF and DMR.

on. All the things that you can make very quantitative, but are also qualitative and graspable in the everyday.

Search for Simplicity

with Victor Weisskopf

Qualitative Physics for

understanding
explaining
motivating
countering over specialization
unity of conceptual grasp
reconnection to everyday phenomena

Project is a book from A.J.P. column

Topics include

Atoms
Molecules
Chemical Bonding
Solids
Metals
Heat and Temperature
Electrical Properties
Optical Properties
Sun's Energy

Fig. 1. Goals and topics of the Search for Simplicity.

Here, below, still on Figure 1, are the topics we have in mind to do in the book-- there are two aspects to the project. One is writing the book, and we have been working on that for about a year or two. The second is a column Viki signed up to do in the American Journal of Physics; and as we put out each new installment, something at least has been agreed upon, and we have a start for a Chapter on that topic. The topics that we envision seeing in the book are an introductory chapter, probably elaborating a little bit on "what is quantum mechanics?" more than just the sentence like the one I gave from Feynman. Then: atoms; molecules; chemical bonding, where we might go into the shape of molecules and the strength of the bonds plus a little bit of qualitative chemistry--why you get so much energy from a jelly donut and so on. Then solids; metals; a chapter on heat and temperature; the electrical properties of matter; optical properties; and finally a qualitative calculation of the sun's energy-- a very rough model for the sun.

I would like to appeal to everybody in the audience to contribute to the work in a certain way by telling me of their own pet qualitative calculations of any of these phenomena, or even, maybe some things that aren't so ordinary. If you know a quick way to prove that superconductivity really occurs at a low temperature because of pairing, and you don't have to go through a whole detailed calculation with the k vectors going this way and that way, then I would like to hear about it and I would like to be able talk about it, and maybe by the time the book comes out superconductivity will be an every day phenomenon and it will just fit right in naturally.

What I want to talk about today is an example of the kind of work that we have been doing. And, it is the example that appears in the December I985 column in the American Journal of Physics, on linear thermal expansion coefficient. Now, our rules for all this stuff are (1) you set $2\pi = 1$ wherever you must. (2) You get close enough to understand why the answer is what the answer is. And (3) you must know when you are getting a general pattern of numbers that are too low or too high, or some that are too low, and others that are too high,--why they go in that direction and how you could improve them. So, those are the rough rules. (Figure 2, upper left corner). Otherwise, we do the famous old order of magnitude estimates, the back of the envelope calculation. The task for linear expansion coefficients is, of course, just trying to explain how come, when you heat ordinary objects they expand. So you take a rod of length L, there is a little green magnifying glass up there in the top of Figure 2, it looks like an upside down "Q" or something, and if you heat it, the end gets red and it expands. Well, the whole rod might get red. The red part, ΔL, is the increase in length as you increase the temperature. Qualitatively, what you think about, when you think of this happening is this: inside that magnifying glass there is a bunch of atoms in the rod and they are all wiggling about, jiggling like this, back and forth, up and down, every which way--I should put arrows on, I suppose, so you can see what I am talking about. You imagine that as the temperature goes up, you are adding energy. Their vibrations are getting stronger and stronger, and they wiggle faster as faster, and they move into bigger spaces. But, of course, there are problems with that picture as some of you may know. I'll come back to that in a second. We define the coefficient that I am talking about, linear expansion coefficient. One of the tricks is to always use a different symbol than the physics books do, right, so instead of calling it α it is τ. But the coefficient of linear expansion is defined as: the change in length, ΔL_l, when you change the temperature by one degree, to divided by the length. And we observe that ΔL is approximately proportional to the temperature in a region of very many degrees, maybe 100 to 150 degrees above room temperature for most simple substances. Let's assume

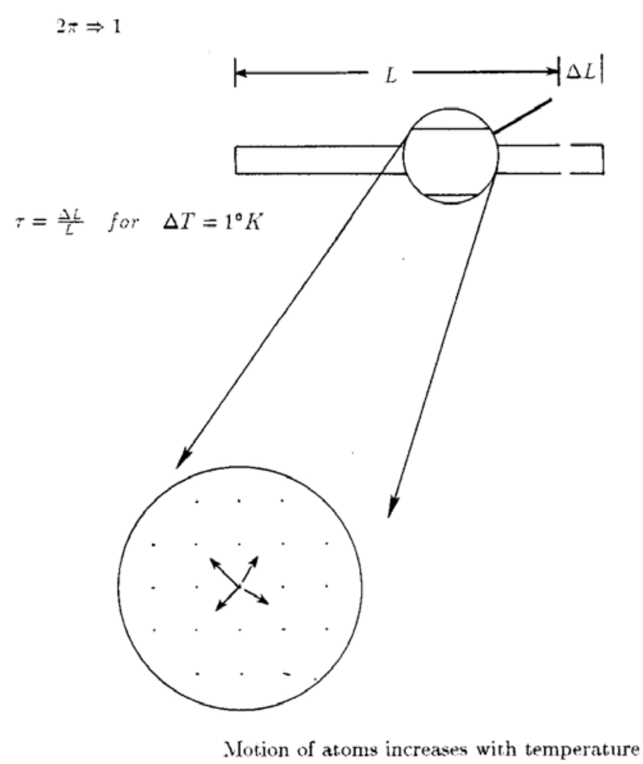


Fig. 2. Example: Estimate of the coefficient of linear expansion by considering atomic motion.

that relationship, which is only true in that limited temperature range, actually worked for all temperature, or for a very great range of temperature. Then we can get a very quick and dirty, but not very far off, qualitative estimate of the coefficient of linear expansion.

Here is how you do it. Assume that this relationship holds to such high temperature that you have actually given enough energy to the individual atoms to equal the binding energy. That temperature would be such that kT binding equals E_B, the binding energy. Since that binding energy is the amount of energy necessary to liberate an atom from the rod, if you heated it to that degree, the rod would fall apart. Let's assume now that falling apart corresponds to something like, give it enough heat so that it would actually double in length. You know that these thermal increases are very small, they are actually about the order to 10^{-5} per degree--the fractional length change. OK. If that means that doubling the length would make it fall apart, then we would get an immediate relationship that ΔL, when you have raised it to this temperature, (the binding temperature), would be equal to L. That can then be substituted into the formula above--i. e. the definition. And you very readily get that α is equal to, $(\Delta L_1)/(\Delta L)_B$, the change in one degree, divided by the change of length for the binding temperature. That equals Boltzmann's constant k, which I used before, divided by the binding energy. So, you get very simply, as the rough qualitative estimate, that α is equal to--since Boltzmann's constant 8.6×10^{-5}-- is electron volts per degree kelvin,-- that α is 10^{-4} divided by the binding energy expressed in electron volts. Now, to make that estimate hold, what we really have to show is that the expansion is indeed linear, ΔL is linear in T, at least for small changes in temperature.

Well, there is a problem with the picture that I have given you, as I already hinted. If the cause of the expansion is the increased motion of these atoms in their places as you raise the temperature, then why does it make an expansion at all? The atom vibrates about a position, which is an equilibrium position (I see one or two of you are nodding your heads). And if you assume...let's just go through what that calculation amounts to.

Let's just take a pair of atoms in the rod. If you assume that they are originally set apart by, a length d, and then the vibration makes them wiggle back and forth like that, then your model really amounts to having a potential well, with the positions of the atoms just d apart from each other. And focusing on one of the atoms with respect to the other, and the wiggles will make it go back and forth, and raising the temperature simply makes it go up higher and higher in the well so it wiggles at a bigger and bigger amplitude. The problem with it is that the center of all of those oscillations always remains exactly d apart for each of the atoms. So that you can make it wiggle a lot, like this, right, and the atom at the end of the rod will wiggle a little bit more--maybe a tenth of an angstrom more, and atom at the other end of the rod will also wiggle a tenth of an angstrom more, and all the rest are just jiggling in their positions, and there is no net expansion. So, the problem is, that the idea of just looking at the vibrations, very simply, doesn't really explain or give you a value, for the coefficient. It gives you some idea for what the order of magnitude will be, which is correct,

10^{-4} over the binding energy in electron volts. But it doesn't explain it, so it doesn't fulfill the criteria for the kind of qualitative physics that we want to do.

On the other hand, there is a fix for that which is (in fact) also well known and still fits the qualitative model. If we still focus on just a pair of atoms, as they move towards each other (you are moving towards a negative x because of the definition of x as a deviation from the equilibrium separation) and they will eventually come so close to each other that they repel even stronger than the harmonic repulsion. As you pull them apart, they will be moving to a further and further distance and they will go into the region where they will attract even less than the harmonic potential. That means that instead of harmonic potential you have to have an anharmonic term that has to carry a negative coefficient. Because as this graph (Fig. 3) shows in green, we now have more repulsion than the parabola when the distance is coming closer together--when the atom is displaced this way--and less restoring force as you try to pull it apart with the thermal vibration. The effect of the green anharmonic term is to make the larger and larger vibrations occur higher and higher and shift ever further out what the center of the vibration is: exactly the effect that we were looking for.

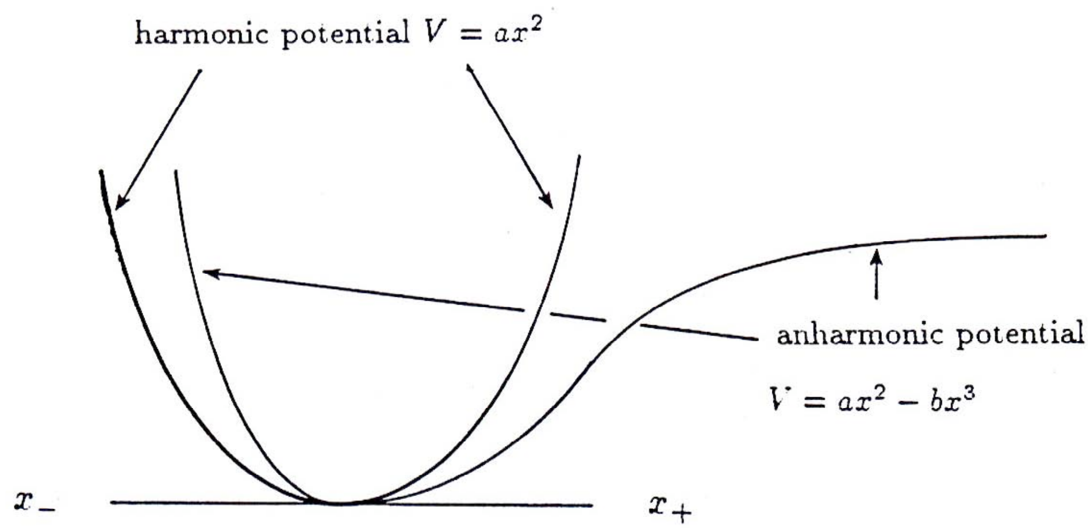


Fig. 3. Anharmonic potential compared to harmonic potential.

So now, I will do the mathematics just like that by flashing it on the board (Fig. 4). If you start instead with a potential that is written just as a quadratic in the separation variable, you write the potential as a quadratic and a cubic term. Then if b = 0, you see the effect of increasing temperature is only to give a symmetric + or -. Defining x_- and x_+ as two extremes of the vibration you can set the energy equal to the potential energy at the extreme. When b is 0, if there is no anharmonic term, the extremes are equal and opposite. You see + and -kT/a because the original equation is just ax^2. If b is not equal to 0, then you can put the original value, the b = 0 value, into the first term and solve using the fact that b is small. Then x+ is shifted as you see in the graph--x+ is shifted a little bit to the right and so is x- and the deviation from the usual distance of d between them, is $b/2a^2$ times kT. That proportionality to T is just what we were looking for, and it just remains for us to estimate what the values of b and a are.

$$V = ax^2 - bx^3$$

$$kT = ax^2 - bx^3$$

$$if\ b = 0 \quad x = \pm\sqrt{\frac{kT}{a}}$$

$$if\ b \neq 0 \quad x^2 \approx \frac{kT}{a}\left(1 \pm \frac{b}{a}\sqrt{\frac{kT}{a}}\right)$$

$$x_+ \approx +\sqrt{\frac{kT}{a}} + \frac{b}{2a}\frac{kT}{a};$$

$$x_- \approx -\sqrt{\frac{kT}{a}} + \frac{b}{2a}\frac{kT}{a}$$

$$\delta = \frac{b}{2a^2}kT$$

Fig. 4. Mathematics of the solution to the anharmonic potential.

Now remember that when I talked about giving it enough energy to take it apart, I spoke of raising the temperature to the binding energy. Once again, that would mean that □ was equal to L, so we look at deviations where, in $ax^2 - bx^3$, the value of x is equal to d, you have doubled the distance, and we plausibly can set each of these terms roughly equal to the binding energy. Then a can be estimated as the binding energy over d^2, b can be estimated as the binding energy over d^3, and we get our final qualitative estimate, □ equals kT/2Eb times the distance. □ was defined as the fractional change □/d for one degree, and that means that our qualitative estimate is k over 2Eb which is 4.3×10^{-5} divided by the binding energy in electron volts.

Now if you look at the actual table of values, (Fig. 5), you can run down all the substances that are listed in Kittel's Solid State Physics (and some of them I guess we got out of the American Institute of Physics Handbook) and you see that the qualitative value is very close to the experimental value as far as order of magnitudes go with the exception of two cases, carbon and silicon. Of course, we talked about the thermal vibrations as if we only had a pair of atoms wiggling together, and those are not independent by any means. No single atom vibrating and no pair of atoms vibrating is an independent motion. Instead, what really make up the independent vibrational motions are the sound waves that are inside the solid. In Fig. 6, I have depicted a transverse mode, where the blue dots are the places that the atoms should be, and the green are the displaced positions. And here is one fairly long wavelength vibration. It is the same wavelength of vibration, except redrawn instead of having the nodes at the end, the antinodes at the end, and I used that as a way to draw what is much harder to depict--a longitudinal mode. In a longitudinal mode, the atoms

Comparison with Experiment				
Substance	$\tau_{\text{exp}} \times 10^5$	$\tau_{\text{qual}} \times 10^5$	$(\varepsilon)_{eV}$	$(T_0)_{\text{K}}$
Li	4.7	2.6	1.6	344
Na	6.9	3.8	1.1	158
Al	2.3	1.3	3.4	428
C*	0.10	0.58	7.4	2230
Fe	1.2	1.0	4.3	470
Ni	1.3	1.0	4.3	450
Si	0.25	0.9	4.6	645
Zn	3.0	3.0	1.4	327
Cu	1.6	1.2	3.5	450
Rb	9.1	5.0	0.85	56
Pb	2.9	2.2	2.0	105

*Diamond

Fig. 5. Comparison of qualitative estimates (qual) with experimental values (exp).

originally at the blue position are displaced either positively or negatively from their equilibrium positions-- here (at the right end) it is positive so the green one is over there. That is a little less positive, just as above. There is no extra displacement, and so on down the line. But, you notice that what happens--once again, exactly in the case where you dealt with the two isolated atoms--is that if you take a perfectly sinusoidal, perfectly harmonic motion, there is no change in L. The same L occurs in a displaced position, even in a longitudinal mode, as would have happened in the absence of any vibration because there are just as many regions here where the atoms are closer together and they are compressed, as there are regions where the are further apart and there is dilation. If you go through the calculation exactly, you find out that the value that we had before (from just looking at pairs of atoms) is the same to within 20%. The value you get when you consider the normal modes of solid are 20% higher. But this is not the place or the time to do that. Just notice one thing.

The last picture here (Fig. 7) tells you there is a shortest wavelength, therefore a highest frequency, the Debye frequency. This is because you cannot make a displacement of the atoms which has a wavelength shorter than one where each atom is out of phase with its next neighbor. The highest frequency //, implies that there is a Debye temperature, ⊓ = h⊓/kand when you look again at the agreement and we see the two places where the figures are way out of line, we notice that both for diamond and silicon where we have a qualitative estimate that is much too high--the rest of them are generally too low and the 20% that I spoke about brings it very close to agreement. We notice that those are the two with the very high Debye temperatures, much higher than room temperature. All these are listed for room temperature, so the explanation goes through not only for why there is thermal expansion (basically that there is an anharmonic term) but also for which ones deviate and so we have fulfilled the terms of the program!

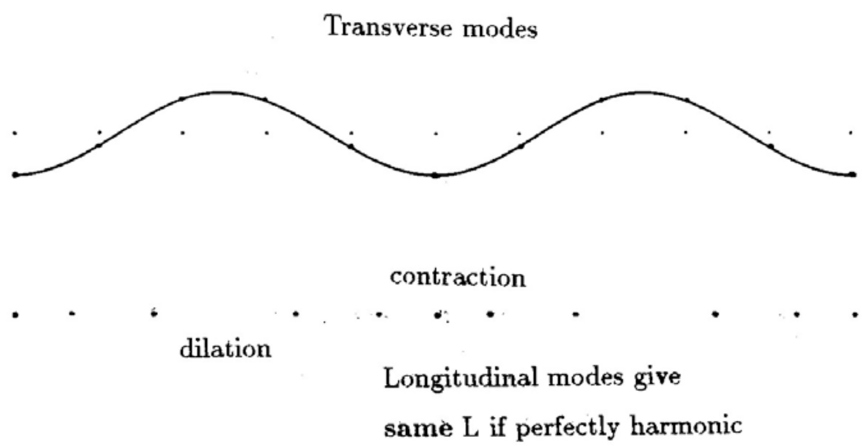
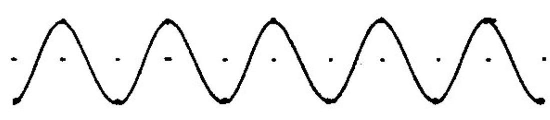


Fig. 6. Transverse and longitudinal vibration compared.



Note that $\exists$ a shortest wavelength $\rightarrow$ highest frequency, ω_D

$$T_D = \hbar\omega_D / k$$

Fig. 7. The shortest wavelength has two atoms per wavelength, with a corresponding highest frequency.

What I would like to do with this--I guess I did talk very fast--is to stimulate you to come up to me and tell me that you have similar things that you have worked out for conductivity, or for plasma physics, or for any everyday phenomena. We have got some very nice things worked out about the topics that I mentioned before, but we are still hungry for more information. That's the end of my talk and I made it exactly one half hour too late.

La Jolla Physics Symposium

Sunday, September 8, 1985

Session I I

GENERAL INTRODUCTION TO SESSION II

by Richard L. Morse

Department of Physics, University of Arizona, Tucson, Arizona

While we wait for a few more people to drag in, let me bore you with a few words of recollection of my own. Friday night, I sat in frozen apprehension, fearful that one of the organizers was going to ask me to get up and say some funny things. Fearful because I couldn't think of anything funny to say. I very much admire Jim Benford and Bill Frazer and others for doing so well at that activity.

For me, the time at UCLJ was a very important time and a very valuable time, but not always a very funny time. I was pleased yesterday, when Bill Prothero, good morning Bill, are you here?--when Bill Prothero expressed what I think was a similar sentiment--just trying to hang on and see if he could get through it. I find that the pressure and intensity that necessarily goes with an outstanding educational experience have left its mark even on the humorous recollections that I do have. And, in this spirit permit me to recall one incident at the expense of my thesis advisor. We noticed after a few semesters here that Marshal Rosenbluth advisees were all taking five courses, while most of the other students took only three or four per semester. This was sometimes a bit of a burden and one day I confronted Marshal on the subject--asked him why he made us do this. Well, he explained that when he was a grad student at the University of Chicago, the curriculum was pretty loose. He wasn't required to take many courses, so I guess didn't and figured that this had retarded his intellectual development. He wasn't going to let this happen to us! Well, please understand that I am not complaining, having survived it. Far, from it. It was a great education, very well administered and very much appreciated. Moving right along, I would like to introduce Tom O'Neil. Let me introduce the guy who terrorized me for an hour on my orals, Norman Rostoker.

INTRODUCTION OF THOMAS M. O'NEIL

by Professor Norman Rostoker

Department of Physics, University of California, Irvine, Irvine, California

I came here in 1962, and the first student that I had was Tom O'Neil. It was a great pleasure working with him. Since then I have had twenty three students who have graduated and obtained their Ph.D's, and I have learned a little bit. First of all, I've learned that he was a very good student. When you have only had one, it is hard to tell! Secondly, I learned that it is very difficult--it is really quite difficult to get very good Ph.D. students. And it is a highly desirable thing to do, because the amount of work and agony involved is considerably greater if you don't have such a good student. Also, I wondered how come it was so easy to get good students here because everywhere else I have had to be very tricky, very underhanded, and even to do a great deal of traveling to find good students. And, I have to think back as to how it happened that I got such a good student to begin with, and I generally had the best students here. Well, I suppose that was probably because Marshal was here and attracted a lot of good students and wasn't willing to take them all himself. But, at any rate, I did enjoy very much working with Tom. I had a plan of what a Ph.D. student should do. He should first work with me on something I am interested in and write a paper in collaboration, then he should originate his own problem and solve it rather independently. And, this plan was carried out completely with Tom. I have never been able to carry it out since. At any rate, he wrote his thesis on "Nonlinear Plasma Waves," and it turned out to be a very well known piece of work, and for a number of years afterwards he worked on this subject, which is rather dark and mysterious, but he was still one of the leading experts in the world on it for a number of years. And, that is not what he is going to talk on this morning, he is going to talk on the subject of pure electron plasmas, and I'll make a few remarks about that, since that is also one of my interests.

This is probably the world center of study on pure electron plasmas. There is one other though, and that is up the road at UC Irvine. I think that this is a subject with a future. It has a lot of applications actually to accelerators. Most of the people in accelerator physics aren't aware of this yet, because they usually work at sufficiently low current densities that plasma effects aren't very important. But the subject of high current accelerators is growing now, and the importance of its application is growing.

The work here is mostly on basic physics, and as a basic subject in plasma physics it is very attractive. One of the difficult things about plasmas is that it has particles in it of very different masses, and as a result it is even hard to write a computer code for it because of the very different time scales. So, if you get rid of the ions, there are some other complications introduced, but it becomes a much more beautiful subject to study. Tom

O'Neil in collaboration with John Malmberg here, have done a beautiful job of developing this subject in the past ten years, and that is what we will hear about today.

PURE ELECTRON PLASMAS

by Thomas M. O'Neil

Department of Physics, University of California, San Diego, La Jolla, California

Thanks, Norman. Norman was a wonderful thesis advisor, and I want to start by saying a few words about my experiences as his student. I loved the fact that Norman was straight from industry and held academic niceties rather in contempt. For example, when I handed Norman the first copy of my thesis, he didn't read it. He hefted it, eyed its thickness and then handed it back saying, "add another chapter."

Of course, Norman was aware that his approach was somewhat off color and this worried him a little. You probably recall that there were faculty members who did maintain academic decency and one of them was Walter Kohn. When Norman asked Walter to be a member of my thesis committee, I began to worry. Remember, Walter was known to ask very penetrating questions. Norman said, "Look, Tom, I know that you are worried about Walter, but I want him to be on the committee for a good reason. With Walter on the committee, anything that we can get away with is okay." So, Norman and I proceeded to cut academic corners with a clear conscience.

Well, I could carry on reminiscing for hours, but I had better not. I should get on to physics, or I will run out of time in the end.

Early in the planning for this Symposium, various people suggested that there should be some discussion of current research in the UCSD Physics Department: so Brian Maple and I rather immodestly asked ourselves to speak. I am going to tell you about plasma physics research and I will start by giving you an overview.

Figure 1 presents a list of faculty members whose research involves plasma physics. Keith Brueckner works on inertial confinement fusion, Norman Kroll works on free electron lasers and advanced accelerators, Ralph Lovberg works on plasma propulsion engines, John Malmberg and I work on pure electron plasmas, Carl McIlwain works on space plasmas, and Bill Thompson works on everything really--inertial and magnetic confinement fusion, space plasmas and astrophysical plasmas. In addition to work in the department, there is a lot of plasma physics research in local industry. There is a huge fusion group at GA Technologies, Inc. and there are smaller groups at SAIC, Maxwell Labs, JayCorp and Physical Dynamics. In this large community of plasma physicists there are many distinguished plasma physicists, and some of them have joined our faculty as adjunct professors. These professors take graduate students and from time to time teach courses, usually courses on advanced research topics. So, I think you can see that there is a lot of

FACULTY WORKING ON PLASMA PHYSICS

Keith Brueckner	Laser Fusion
Norman Kroll	Free Electron Laser
Ralph Lovberg	Plasma Propulsion
John Malmberg	Pure Electron Plasmas
Carl McIlwain	Space Plasmas
Tom O'Neil	Pure Electron Plasmas
Bill Thompson	Fusion, Space Plasmas, Astrophysical Plasmas

ADJUNCT FACULTY

Henry Abarbanel	Nonlinear Phenomena
Edward Frieman	Fusion
John Green	Nonlinear Phenomena and Fusion
Nick Krall	Fusion
Tihiro Ohkawa	Fusion

Fig. 1. List of faculty members of the UCSD Physics Department who are engaged in plasma physics research.

plasma physics research here and that there are many opportunities for graduate students to study plasma physics.

Now I want to narrow the focus and talk about the research that I do in collaboration with John Malmberg, namely, the work on pure electron plasmas. What I want to do today is to tell you why we think these systems are interesting and why we think they are good vehicles for training graduate students.

Figure 2 is a picture of our group. The group is about half graduate students and half research staff. I think you can recognize some of the students who have been taking pictures and helping with the Symposium. All "volunteers," of course. The fellow standing as close as possible to our pretty secretary is John Malmberg. John looks after the experimental end of the program and, together with Bill Thompson, I look after the theoretical end of the program.

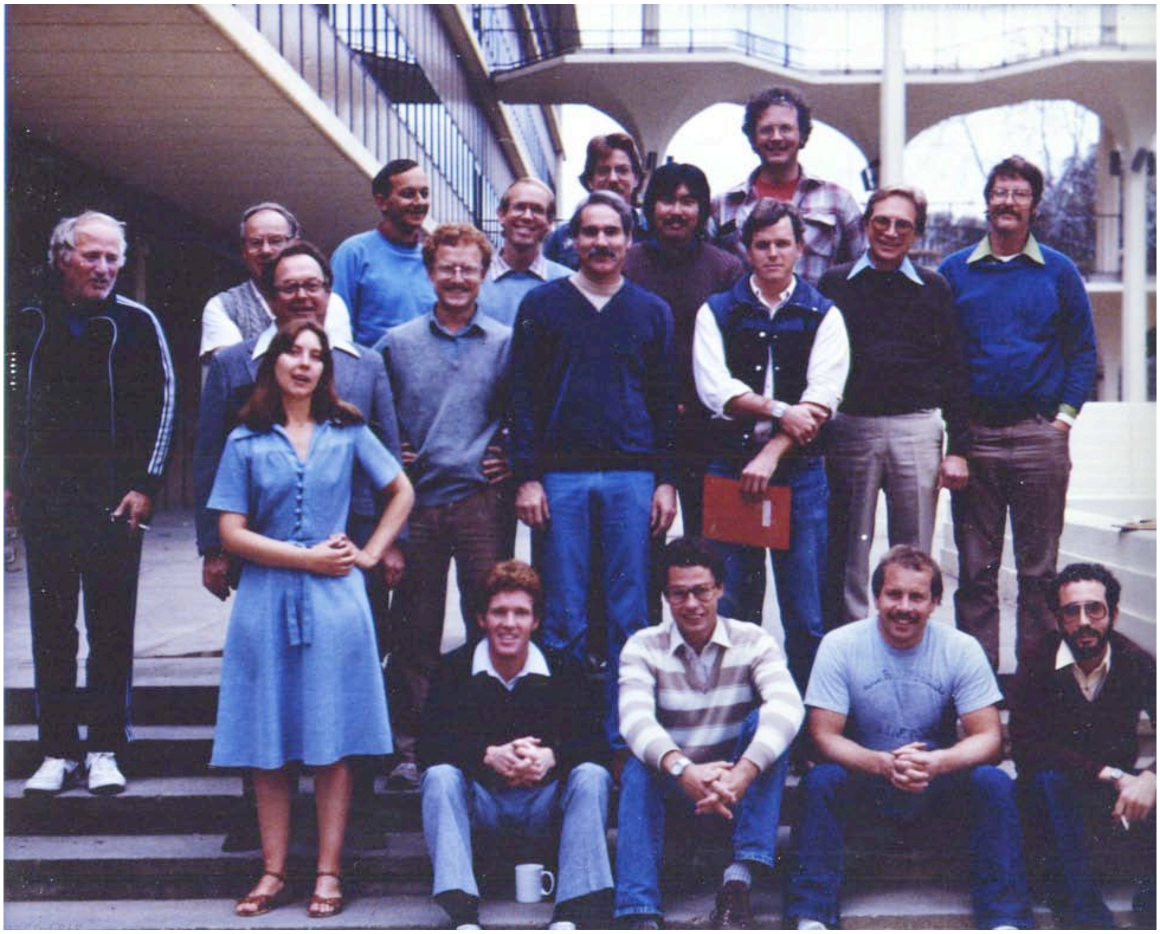


Fig. 2. Picture of the Malmberg-O'Neil Plasma Physics Group

The confinement geometry for the pure electorn plasma is very simple. A conducting cylinder is divided into three sections, and there is a uniform axial magnetic field (Figure 3). During the injection phase, cylinders A and B are held at ground potential, and cylinder C is biased strongly negative relative to ground. To the left of cylinder A there is a hot negatively biased filament. Electrons boil off this filament and spiral down the field lines until they reach the region of cylinder C and are reflected electrostatically. We then switch the bias on cylinder A to negative; this interrupts the flow of electrons from the filament and traps a section of the electron column in the region of cylinder B. The electrons are confined radially by the magnetic field and are confined axially by the electrostatic fields.

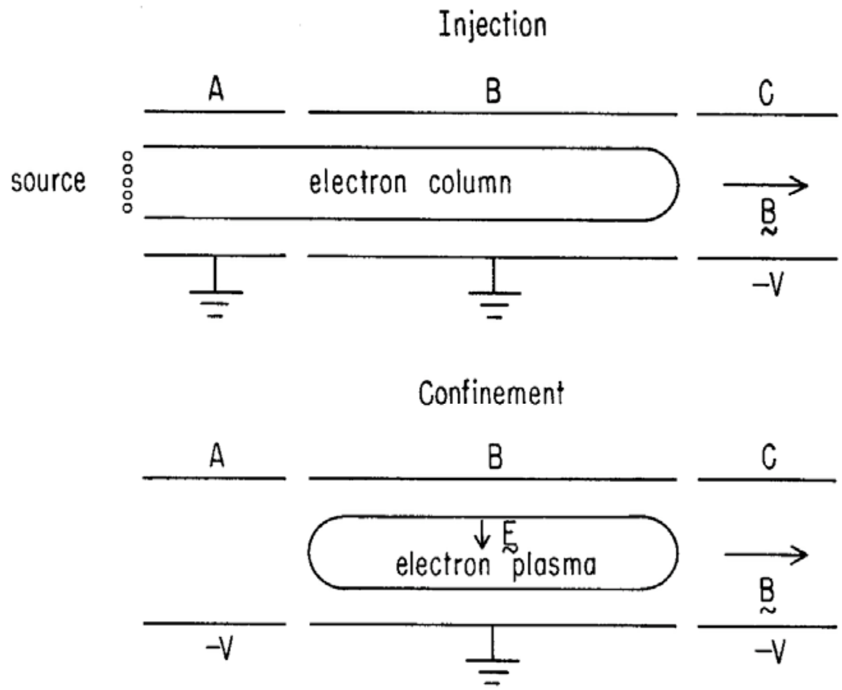


FIg. 3. Confinement geometry of pure electron plasmas.

Since this is an unneutralized column of electrons, Gauss's Law requires that there be a radial electric field. The existence of a radial electric field together with an axial magnetic field means that the electrons experience an E x B drift. For the cylindrical geometry, this drift is an E x B drift rotation of the column as a whole. Another way to think about the rotation is to realize that in the expression for the Lorentz force, the radial electric field is nearly cancelled by the v x B term where v is the rotational velocity. In other words, rotation through a magnetic field is in this sense equivalent to neutralization by a positive charge.

One of the first questions that you might ask is why do I insist on calling this unneutralized collection of electrons a plasma? And the basic answer is that it exhibits much of the collective phenomena that we associate with an ordinary neutral plasma. For example, these systems exhibit electron plasma waves, and if you measure the dispersion relation for these waves, as John Malmberg has done many times, you find that the dispersion relationship is essentially the same as for electron plasma waves in a neutral plasma.

The reason for this is quite simple. You may recall that electron plasma waves have such a high frequency that in a neutral plasma the ions don't participate in the motion, they just provide a neutralizing background charge. Consequently, the high frequency response in a neutral plasma is just the same as it is in a pure electorn plasma. Likewise, any mode in a neutral plasma that involves only electrons will have an essentially identical counterpart in a pure electron plasma. Also, pure electron plasmas exhibit Debye shielding, Landau damping, plasma waves, echoes, etc.

The fact that these plasmas exhibit such phenomena is one of the reasons that the plasmas are good vehicles for training graduate students. Any student who wants to work on these plasmas must first learn plasma physics in general. The second reason is that these plasmas differ from neutral plasmas in a few important ways, that is, they have a few unique properties. By focusing our research on these properties, our students are not in competition with the hords of plasma physicists at the large fusion labs.

What I want to do next is to tell you about a couple of these unique properties. The first of these has to do with confinement. In principle, a pure electron plasma can be confined by static electric and magnetic fields forever; whereas, a neutral plasma cannot. In practice, confinement times of a day are routinely obtained. Malmberg puts electrons in at night and they are still there when he comes back the next morning. He often talks about assigning each new graduate student in the group his share of electrons, and if the student loses them, it's too bad, he or she doesn't get anymore.

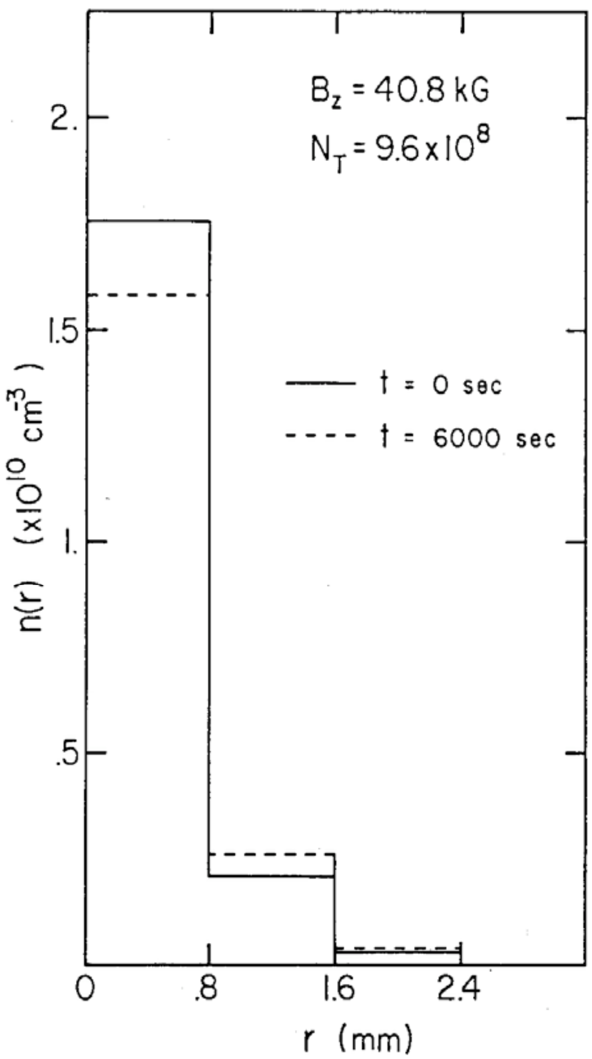


Fig. 4. Radial density profiles at t = 0 and at t = 6000 seconds.

Let me show you radial density profiles for the plasma taken at two different times. To obtain such a density profile, Malmberg switches the voltage on cylinder C to ground potential, and the electrons stream out along the magnetic field lines. Just beyond cylinder C, the field lines pass through three Faraday cups shaped like concentric rings. From the total charge collected on a particular ring, one can determine the density of electrons on the field lines that intersect that ring. Figure 4 shows a histogram of the number of electrons collected on the first ring, the second ring, and the third ring. The solid curve is obtained by dumping the electrons immediately after injection and the dashed curve by dumping after six thousand seconds have elapsed. You can see that only a few electrons have moved from the center ring to the outer rings; this translates to a confinement time in the range of 10^5 seconds.

Next, let us look at the confinement from a theoretical point of view. Why do we obtain such long confinement times, and why do I claim that it is in principle possible to confine a pure electron plasma forever? It is useful to introduce the total canonical angular momentum for the electrons

$$P_\theta = \Sigma_j \, m v_\theta \, r_j - (e/c) A_\theta (r_j) r_j \qquad (1)$$

Here, I have a sum over all of the electrons of the mechanical part of the angular momentum plus the vector potential part of the angular momentum.

I realize that some of you who haven't been working in physics may have to stretch here. Remember when you were reading Goldstein as a graduate student and ran across a momentum with a vector potential in it; you probably wondered what possible use this could have. Well, here is an application.

For a uniform axial magnetic field (**B** = zB) the θ- component of the vector potential is given by $A_\theta(r) = Br/2$, and for a sufficiently large magnetic field the vector potential terms in the angular momentum are much larger than the mechanical terms. Malmberg's experiments typically operate in this parameter regime. Thus, the total canonical angular momentum reduces to the form

$$P_\theta \sim \Sigma_j \quad (-eB/2c) r_j^2 = (-eB/2c) \Sigma_j r_j^2 \qquad (2)$$

Now, recall that the confinement geometry has cylindrical symmetry, at least nominally. Such symmetry implies that the Hamiltonian for the electrons is invariant under rotation, which in turn implies that P_θ is conserved. Setting P_θ equal to a constant implies a constraint on the allowed radial positions of the electrons, $\Sigma_j r_j^2$ = constant; if some electrons move out, others must move in.

This is a very powerful constraint and I will illustrate it with a simple example. Suppose that the cylindrical wall is located at a radius of l0cm and that I inject the electrons in a ring at a radius of 1 cm. (Figure 5). It turns out that a plasma in the shape of a ring is unstable; it is unstable to the diocotron mode. Consequently, what must happen is that this mode will grow up, nonlinear effects will come into play, and the plasma will become turbulent and start thrashing about. However, all of this complicated behavior involves only interval interactions and P_θ is conserved. Since $\Sigma_j r_j^2$ = constant, only 1% of the electrons could ever move from r_j = 1 cm out to the wall at r_j = l0 cm; the other 99% of the electrons are confined forever. How is this argument changed in a neutral plasma? The point is that e takes both positive and negative values and cannot be taken out from under the sum in Eq.(2). The constraint becomes $\Sigma_j e_j r_j^2$ = constant. Electrons and ions can move to the wall

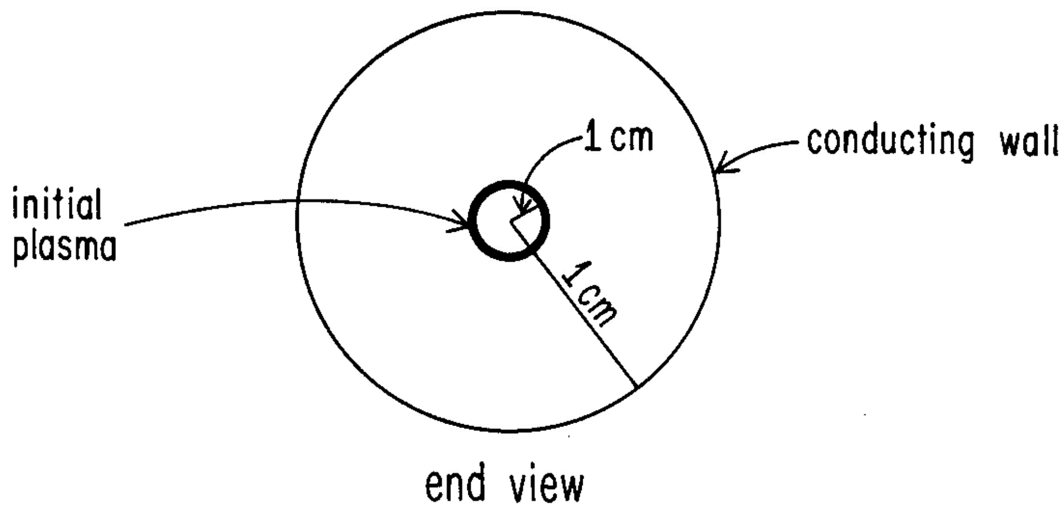


Fig. 5. Simple example illustrating the constraint on allowed radial positions.

together and preserve this sum, and this is just what happens in electron-ion collisional transport and in many instabilities. So you can see that it is much easier to confine a pure electron plasma than it is to confine a neutral plasma.

Of course, in Malmberg's actual confinement apparatus, the cylindrical symmetry is broken by small imperfections such as field errors and construction errors. These result in a small torque on the plasma and lead to a slow plasma expansion, that is, the expansion over a period of a day. Nevertheless, the constraint has a major influence on the confinement and transport properties of a pure electron plasma, and we focus a large part of our theoretical and experimental research effort on these issues.

Another way in which a pure electron plasma differs from a neutral plasma is that the pure electron plasma can be cooled to a very low temperature, say, to the range of a degree Kelvin, without the occurrence of recombination. There simply are negligibly few ions in the confinement region. Let us ask what we expect to happen when a pure electron plasma is cooled. When kT drops below e^2/a, where a is the mean distance between electrons, the electrostatic interactions between electrons establish strong electron-electron correlations. At a sufficiently low temperature one expects to obtain the short range order characteristic of a liquid, and at an even lower temperature one expects a phase transition to a state with long range order, a crystalline state. So, by cooling a pure electron plasma, one expects to obtain a pure electron liquid and a pure electron crystal, and that is one of the things that Malmberg is trying to achieve in the laboratory.

If you write down the Gibbs distribution for a pure electron plasma, you can see that

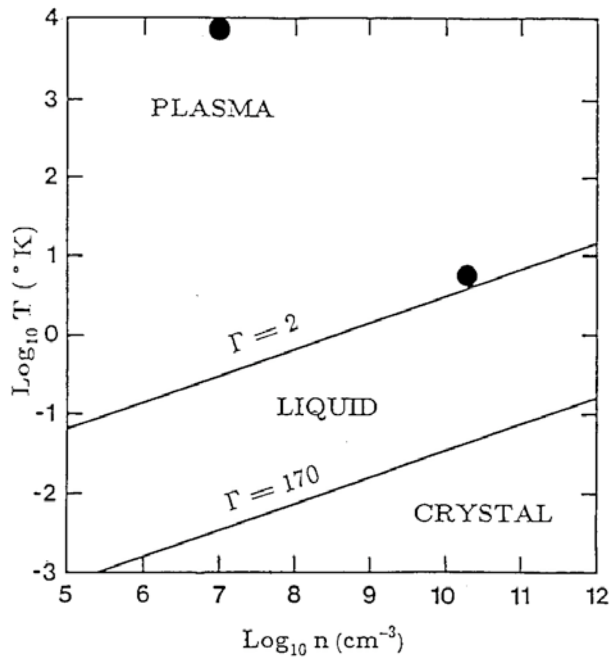


Fig. 6. Phase diagram showing the plasma, liquid and crystalline states.

correlation properties depend only on the parameter $\Gamma = e^2/akT$, which is the ratio of the electrostatic interaction energy to the mean thermal energy. The condition $\Gamma \ll 1$ corresponds to a weakly correlated plasma, that is, to an ideal gas of electrons. When the temperature drops to the point where $\Gamma = 2$, the plasma begins to exhibit the local order characteristic of a liquid and for $\Gamma = 170$ there is a phase transition to a crystal.

Finally, let me exhibit this information on a phase diagram and show you the current status of the experiments. In Figure 6, the ordinate is the log of the temperature in degrees Kelvin, and the abscissa is the log of the density in particles per cubic centimeter. The degree of correlation increases as one moves diagonally from the upper left to the lower right; the onset of the liquid and crystal states is indicated by lines at $\Gamma = 2$ and $\Gamma = 170$.

The experiments are performed using a cryogenic apparatus with the walls of the vacuum vessel at the temperature of liquid helium (4°K), and the magnetic field is produced by a superconducting coil; so quite large fields are available. The dot at density $10^7/cm^3$ and temperature 1 eV ≈ 10^{4}°K indicates the weakly correlated plasma which Malmberg is presented with upon injection. By manipulating the plasma, he takes it to the conditions indicated by the second dot, that is, to a density of a few times $10^{10}/cm^3$ and a temperature of about 6°K.

This is done in the following way. He injects into a magnetic field of about I kilogauss and then ramps the field up to about 40 kilogauss, compressing the plasma by a factor of 40. He then compresses the plasma axially by a little more than a factor of 10. To understand how the axial compression is done, you must realize that the apparatus has more cylindrical sections than the three shown in Figure 3. By varying the voltages on these sections, the plasma can be squeezed from a long section into a short section.

The temperature is reduced by simply allowing the electrons to undergo cyclotron radiation. The temperature of 6^{o}K is not a direct measurement; it is an inference obtained by equating the cyclotron radiation rate to what we believe must be the dominant heating rate. The dominant heating process is almost certainly associated with the large amount of electrostatic energy stored in the system; this energy is many orders of magnitude larger than the thermal energy. As the plasma expands slowly, because of the small imperfections in the confinement geometry, the radial electric field does work on the electrons and heats them. In other words, the electrostatic energy is gradually changed into heat. By measuring the plasma expansion rate, Malmberg determines the maximum heating rate, and the 6^{o}K temperature is obtained by equating the heating rate to the cyclotron radiation rate. The measured density and the inferred temperature imply a correlation strength of $\Gamma = 1.2$, that is, just above the liquid regime.

We are considering various schemes to increase Γ further. For example, you can imagine a rapid but nevertheless adiabatic expansion of the plasma. It turns out that an expansion by a factor of 10 in plasma length leads to an increase in Γ by nearly a factor of 50. Also, we are working on various diagnostic techniques. Determination of the temperature by an inference is not at all satisfactory; one would like a direct measurements. For example, we might be able to measure the degree of correlation with Bragg scattering. The inter-particle spacing is about right to match the wavelength of CO_2 laser light; so we are considering Bragg scattering with a CO_2 laser.

How am I doing on time?

You are pretty close to the end.

Alright, thanks.

INTRODUCTION OF RICHARD MORE

by Richard L. Morse

Department of Physics, University of Arizona, Tucson, Arizona

It is my pleasure now to introduce Dick More who is sitting apprehensively up there, eyeing me. When I began thinking about what I would say, I saw perhaps why the organizers asked me to introduce Dick. There is almost nothing of a cruel, humorous sort that I could say about him that wouldn't backfire! I would be slightly awkward to kid him about right wing politics, or about multiple marriages--or even about coming from a low class place like San Bernadino. Hey, did you know that San Bernadino is the birth place of Hell Angels and McDonald's hamburgers? Real pride! But, in that last connection this Reunion has helped clear up a little mystery for me. San Bernadino is right on the San Andreas fault and both Dick and I lived only a mile or two from it, and felt it shake many times. And, we were office mates for awhile in what's now the Mayer Building. And, one warm summer morning we were standing at the blackboard with a third student, we remembered he was a tall, slender New Yorker, when a very noticeable earthquake hit and without looking at one another, or saying a word, why Dick and I shot through the door and into the open leaving the New Yorker not knowing what was going on or where to go or what to do--and sort of standing in a puddle. Well, until last night during one of the roasts, I still had not known who the New Yorker was, when someone recalled, and I then realized it was Sid Coon, from Iowa who had read enough liberal journals to pass as a New Yorker.

Well, Dick did his thesis here under Harry Suhl in statistical mechanics and then went on to the University of Pittsburgh. And, after getting enough of academia, then went to the Lawrence Livermore Laboratory where he has worked in the laser fusion program. There he built on his statistical mechanics background, and I am very pleased to say has come to be known as a national leader in the very important and challenging area of calculating equations of state and opacities of plasmas under the relatively low temperature and high density conditions encountered in the laser fusion program. He will be talking to you on--let me get it right here--the "Statistical Mechanics of High Density Matter".

STATISTICAL MECHANICS OF HOT DENSE MATTER

by Richard More

Lawrence Livermore National Laboratory, University of California, Berkeley

At the beginning let me say that I could have been introducing Dick Morse and in that case the introduction would have been longer because his contributions to laser fusion are surely larger than my own. When the Los Alamos laser program started, Dick headed their theory effort. He wrote an early laser-target simulation code.[1] Dick and his group identified one of the main laser absorption mechanisms, called resonance absorption,[2] and they were the first to detect the anomalous heat conduction inhibition in laser plasmas as well as other phenomena in hydrodynamics and implosion symmetry.[3] Many other La Jolla people participated in laser fusion. In ~1972 Keith Brueckner and his collaborators at KMS fusion published the first laser implosion experiments and Keith wrote two massive review articles summarizing those early efforts.[4] Chuck Cranfill, Yim Lee and I have also made some contributions.

Why work on laser fusion, large lasers and hot matter made with lasers? For myself there are several motivations, one is simply that these large lasers are beautiful things, and it is a pleasure to work around them. I've seem them evolve from table-top arrays of prisms and lenses to huge machines like accelerators; we keep hearing about new physics, new experiments and novel ideas.

Another reason to work in this area is that our laboratory, which invests a lot of resources in building lasers, is determined to be the world leader in the technology, and so one is automatically pushed toward the front of the competition.

You can see another reason by looking at our latest laser, NOVA, and its target chamber (Fig.1). If you compare it to the people, you see the scale is large. When a laboratory spends $200 million dollars on building a laser, then they believe they should spend a few dollars for theorists to tell them what targets to point it at. That means the theorists can think about physics and not worry about raising money.

Now these are reasons which got me into the subject, but actually things have changed in the last few years. It has become cheaper to build high-power lasers, and so we are seeing them pop up all over the world. There are productive laser research groups in France, England, Japan, Germany, Canada, and smaller efforts in Spain, Italy, Algeria, China, and the Soviet Union. It looks like we will soon have vigorous competition from University researchers who work on weekends and work late at night.

Apart from any practical application to generating energy, laser-heated targets generate some very interesting physics (Fig. 2). There is some material at high temperatures and low densities - material that is exploded out of the target and heated by the laser - this is the

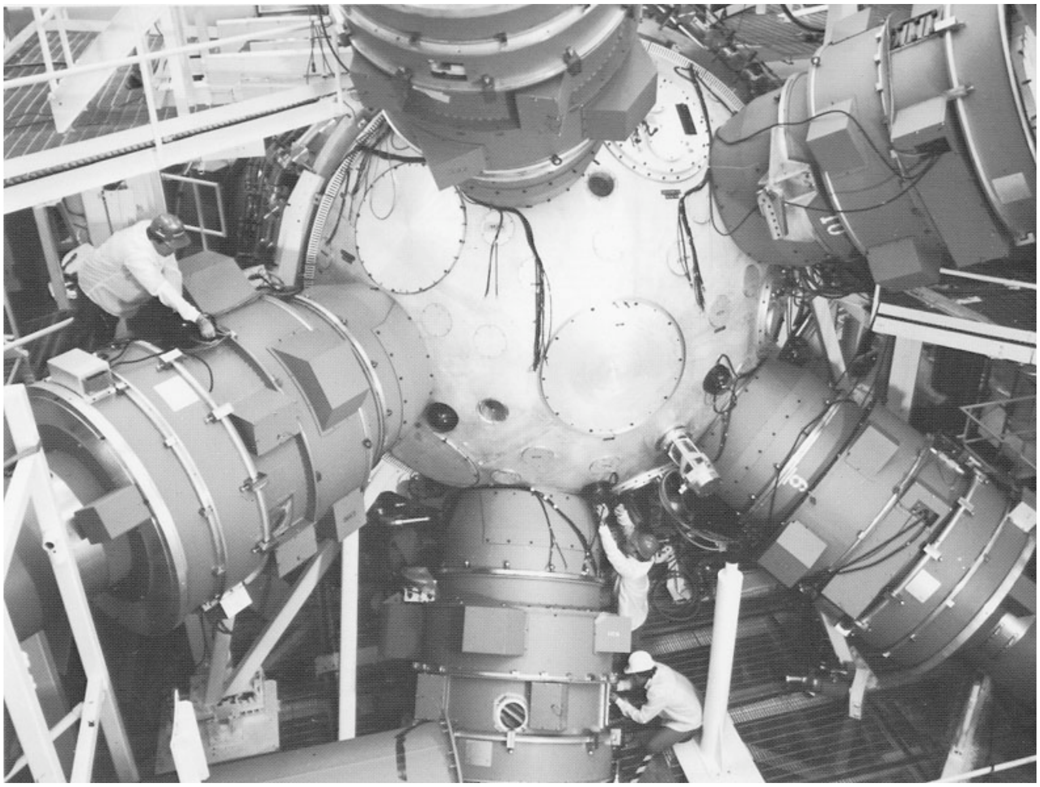


Fig. 1. Target chamber of the NOVA laser. Five large beam-lines are easily seen; these contain frequency-conversion crystals which convert infrared (1.06 μ) laser radiation to visible (0.53 μ or 0.26 μ) light by nonlinear harmonic generation. The target, suspended in the center of the chamber can be as small as a pinhead and is heated to enormous temperature and pressure.

classical plasma. There is a dense plasma region that gets very hot by laboratory standards and is very dense by plasma standards -- densities like 10^{22} electrons/cm^3 or 0.1 g/cm^3, temperatures like 10^6 K or 100 eV. Finally, there is a remarkable region of material compressed beyond ordinary solid densities to form high density matter in which atoms are pushed together and inner-shell electrons are perturbed. With lasers, we can make shock waves of 10-100 Mbars, and by conventional standards of high pressure physics that is very high pressure indeed.

I have a density-temperature phase diagram, and it shows some lines of the gamma parameter that Tom O'Neil mentioned (Fig. 3). This diagram shows you two things: first, there is a range of densities and temperatures in the targets. There is an ablation region; there is a compression region. And second, other plasma technologies also make dense

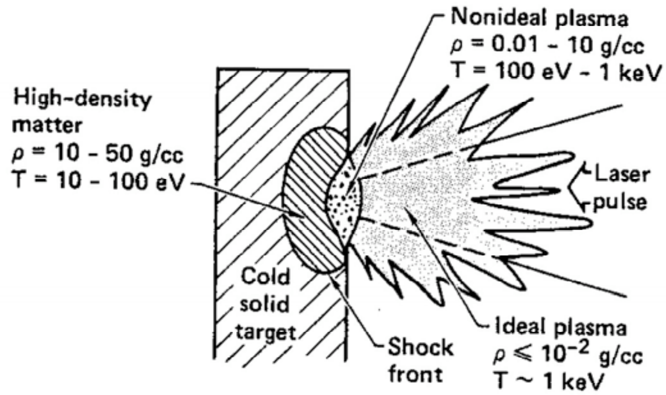


Fig. 2. Schematic laser-target interaction showing the different plasma conditions which are created.

plasmas. Of course, interiors of stars and large planets also reach conditions on this chart, so we have lots of interaction with other pure and applied science research.

Now, atoms in a hot dense plasma are squeezed, and their properties are modified, and that is the game (Fig. 4). Many atoms get pushed so close together that their outer edges touch; photons and free electrons scatter and perturb them. The game is to understand that complex situation.

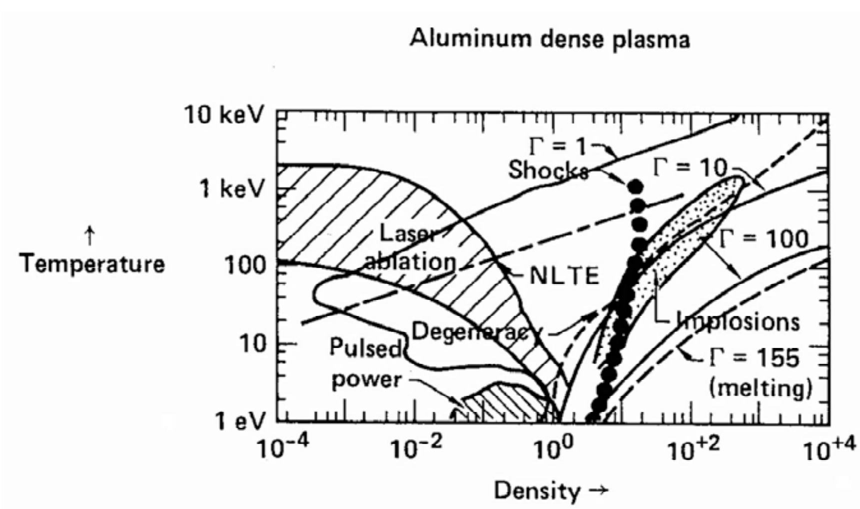


Fig. 3. An approximate phase diagram for matter at extreme high energy density, showing conditions reached in various plasma sources as well as some relevant theoretical parameter values.

Actually, the hot dense plasma is not only statistical mechanics, it involves every area of physics. We have a strong interest in the generation and propagation of radiation, especially X-rays. Also we combine ideas from atomic structure physics, statistical mechanics, and the physics of condensed matter. You can see connections to X-ray spectroscopy, radiation flow, calculation of the structure of highly charged ions, electron populations and X-ray laser action. Then again we study the ionization state and electron-ion scattering which enables us to calculate electrical conduction phenomena. To understand shock waves, we need to understand liquid metals, and that traces back to solid state physics.

I can only say a few words about each of these subjects. Let's start with the basic theoretical pictures. These are models which take the real situation and abstract out of it an idealized description that we can play with.

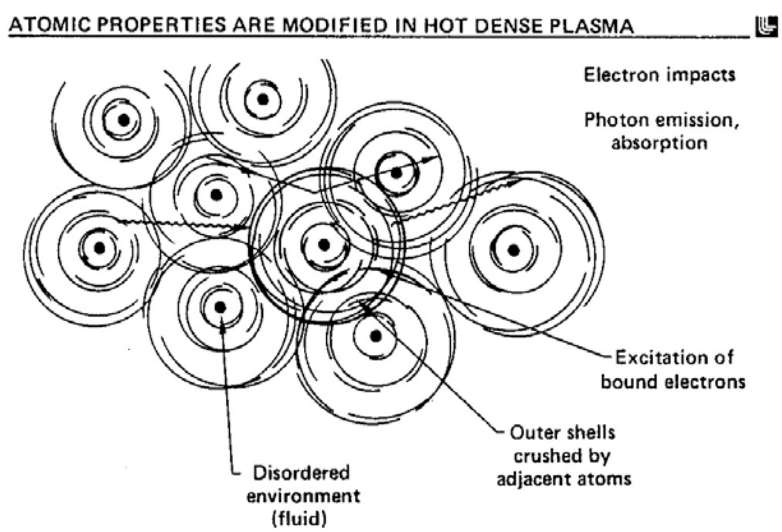


Fig. 4. Schematic overview of atomic phenomena in a high-density plasma. The circles surrounding the point nuclei are intended to represent the shells (1s, 1s-2p, etc.) of bound electrons. We have theoretical models that focus on the positions of atoms in the disordered environment and models that focus on the electrons of one compressed atom.

The first model is very heavily used; it is the model of point charges in a neutralizing background.[5] (See Fig. 4 again.) Of course, the point charges repel one another. One can follow them by the Monte Carlo method, invented by Marshal Rosenbluth among others,[6] which samples many pictures of the spatial location of the ions and decides which patterns are most likely. There is also the molecular dynamics method which treats the ions as point charges that move in time under Newton's law of motion with electrical forces and samples the time history of those motions for as much computer time as one can afford. These computer techniques help to answer the question, "Where are the ions?"

The gamma parameter that we just mentioned is

$$\Gamma = Z^2 e^2 / R_0 kT \tag{1}$$

where Z = ion charge, $R_o = (3/4\pi n_i)^{1/3}$ = the average distance between neighbor ions, T = temperature. Depending on the value of the Γ parameter you have very different physics ranging from ideal gas physics ($\Gamma \ll 1$) to crystalline state physics ($\Gamma > 178$). At typical laser-plasma conditions Γ is ~1 to 10 and the computer results show that the ions are repelled enough to make a sort of empty cavity of radius ~R_o around each ion.

The second model to describe a dense plasma is to focus on one atom (ion) and look at the physics of that atom by self-consistent field calculations. You would use this model if you were more interested in the electrons. The equations to solve are

$$-\frac{\hbar^2}{2m}\nabla^2\psi_s + V(r)\psi_s = \varepsilon_s\psi_s \tag{2}$$

$$f_s = \frac{1}{1+\exp\left(\varepsilon_s - \mu\right)/kT} \tag{3}$$

$$n(r) = \sum_s f_s \left|\psi_s(r)\right|^2 \tag{4}$$

$$\nabla^2 V = -4\pi\left(\rho_+(r) - en(r)\right) \tag{5}$$

There is a nucleus at the center of a cavity, and electrons around it and then you have an external environment which is a uniform positive and negative charge density that squeezes the atom. ($\rho_+(r)$ = constant for $r > R_o$; $\rho_+(r) = Ze\delta(r)$ for $r < R_o$).

You first guess $V(r)$ and solve the Schroedinger equation, Eq. (2), for all the electron wave functions ψ_s and energies ε_s. You assume the electron states are populated by Fermi statistics, Eq. (3), a basic law of statistical mechanics, and you add up the densities of those electrons in Eq. (4). Then you compute a new potential $V(r)$ by solving Eq. (5). The problem is solved when the input potential in Eq. (2) agrees with the output potential from Eq. (5), and that's the calculation.

There are several things to debate in that calculation. One is whether you've got the plasma environment correct outside the atom ($r > R_o$). Obviously, you don't and so the question is what can you do about that? This question would take you back to the point charge models.

Instead of talking about that, one can ask about another assumption, the Fermi statistics, Eq. (3). Can that equation ever fail? [See Eqs. (9), (10) and Fig. 6.]

The most important quantity to calculate is the charge or ionization state of the atoms. Right now, I am thinking about the dense plasma where the ionization is strongly influenced by compression of the atoms; the outer bound electrons become free as a result of pressure. We want to describe that phenomenon with this self-consistent field model.

One of the things to examine is how bound electrons become free in terms of the self-consistent potential. Figure 5 shows a sample potential, V(r), and the same potential with the angular momentum barrier $h^2 (l + 1)/2 mr^2$ added to it. You see there is a pocket in the total potential for $l = 2$. If the density is low enough, then that pocket is at negative energies and can hold a bound state, a 3d shell. If the density is higher the 3d shell becomes a <u>resonance</u> state. And if the density were still higher, the pocket would go away and one would have only free electron states for $l = 2$.

The resonance state corresponds to a quantized positive energy but that energy is not real, it is a complex energy $E_s = \square_s + i\square_s$ because the electron in the resonance has a probability to tunnel out of the pocket and escape to infinity. How should you count resonance electrons? What is the Boltzmann distribution or Fermi distribution for resonance

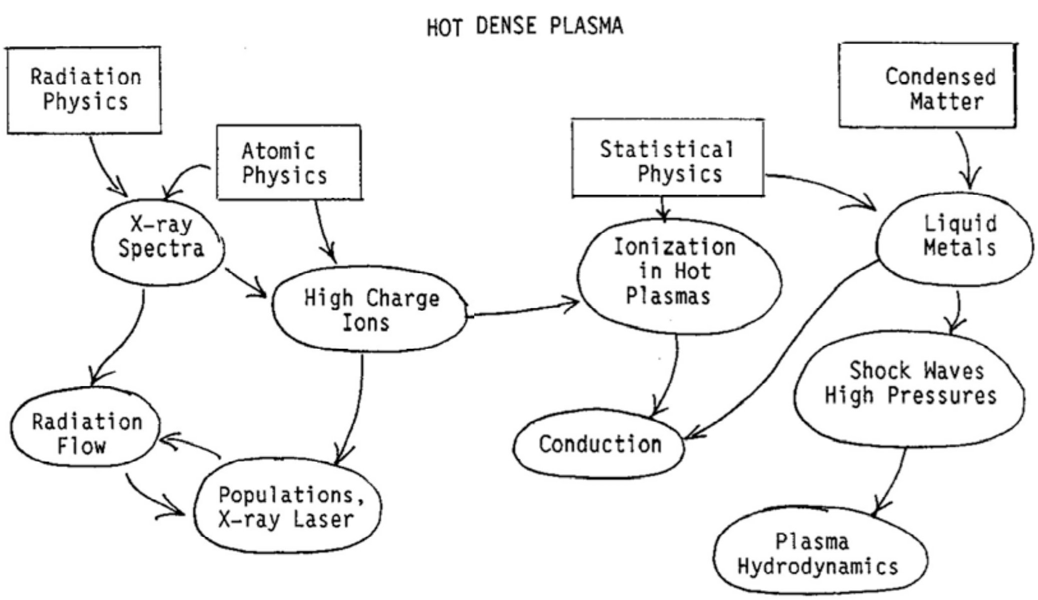


Fig. 5. Areas of physics important in describing a hot dense plasma.

electrons? If you just plug a complex number into the usual formula you will get a complex probability. That doesn't sound right. Should you take the real part of the complex exponential or do you take the exponential of the real part of the energy or what?

$$\text{Prob} = \exp\left(- \text{Re}\left[\frac{\varepsilon_s + i\Gamma_s}{kT} \right] \right) \; ? \tag{6}$$

$$\text{Prob} = \text{Re}\left[\exp\left(- \frac{\varepsilon_s + i\Gamma_s}{kT} \right) \right] \; ? \tag{7}$$

$$\text{Prob} = \exp\left(- \frac{1}{kT} \sqrt{\varepsilon_s^2 + \Gamma_s^2} \right) \; ? \tag{8}$$

Of course it doesn't make much difference if the width is very small, but what's the correct formula? We have done some study of that question and derived what we think is the exact answer. [7] This is a rigorous answer in a one-electron theory like the self-consistent field model and it generalizes Fermi statistics to states that have complex energies $E_s = e_s + i\Gamma_s$:

$$\text{Probability} = \text{Re}\left[F\left(\tilde{E}_s \right) \right] \tag{9}$$

$$F\left(E_s\right) = \frac{1}{i\pi} \int_0^{\infty} \left(\frac{\varepsilon}{\tilde{E}_s} \right)^{1/2} \frac{f(\varepsilon)}{\varepsilon - \tilde{E}_s} \, d\varepsilon \tag{10}$$

$$f(\varepsilon) = \text{Fermi function} = \left[1 + \exp\left((\varepsilon - \mu)/kT \right) \right]^{-1}$$

The equation says that the probability of finding the resonance state filled is the real part of a function $F(E_s)$ which is an integral over the Fermi function, containing the complex function $f(\varepsilon_s)$, but if the width is large, it doesn't. Now with an exact formula like this you ought to say precisely what the probability means, and here is an example: if you want to calculate the electron density n(r), you must add up contributions of the following form, one term for each resonance state:

$$n(r) = \sum_s \text{Re}\left[F\left(\tilde{E}_s \right) \psi_s^2(r) \right] \tag{11}$$

The "resonance wave functions" $\square_s$ (r) have their own precise definition.[8] These formulas are examples of some exact theorems for the self-consistent field model of resonance states.

There is another question of a statistical character about the uses of Fermi statistics, Eq. (3), for the self-consistent field model. Fermi statistics are derived for non-interacting electrons. We have done some model calculations to examine the question whether Fermi statistics can be used for interacting atomic electrons.[9] I could try to explain the details, but it would take too long so I'll just show you an example. We find that Fermi statistics fails by about ten percent for an exactly soluble model system of many electrons, a model that is semi-realistic for complex atoms in hot dense plasmas. Figure 6 is a calculation for dense niobium plasmas with 200 volt temperature and gives the percent error in Fermi statistics for two of the shell populations. You see that Fermi statistics gets more accurate as you go to higher densities, and is poorer at lower densities. This behavior can be understood in terms of changes in the electron-electron interaction that is causing the effect.

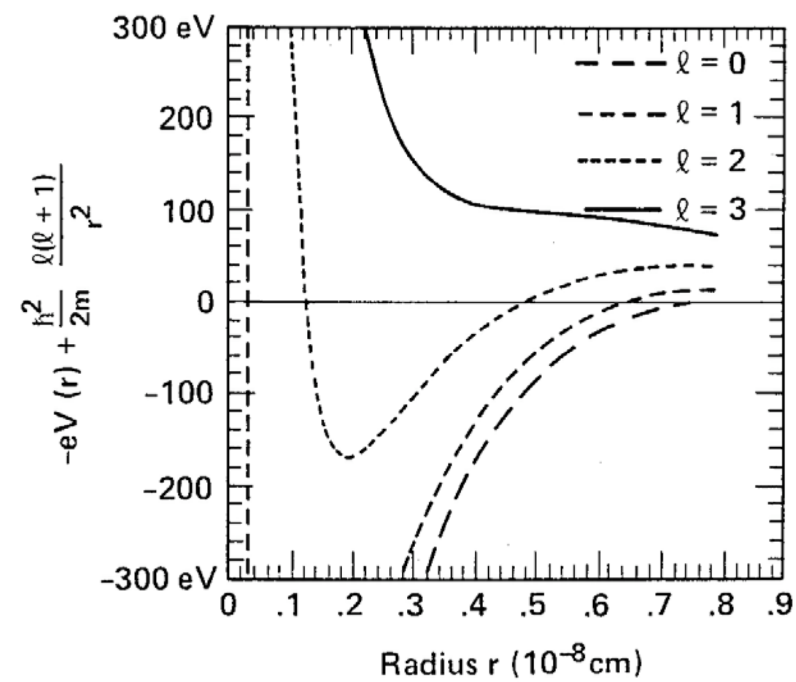


Fig. 6. Numerical self-consistent potential for argon in a plasma of density 30 g/cm^3, temperature 1 keV. The ℓ = 2 potential supports a low-energy continuum scattering resonance which has evolved through compression of the 3d shell of the isolated atom. The text quotes some statistical properties of such resonances in a finite-temperature electron system.

Now, these are examples of statistical mechanics in the theory of dense plasmas. If I were giving this talk at Livermore, people would start to agitate in the audience and say, well, can you do anything useful? We've tried to do some useful things over the years. One is to look at the physics of energy loss of fast charged particles in hot partially ionized matter. Slowing down of fusion reaction-products is very important to thermonuclear burn. If you think about an ion or an electron moving through matter, the energy loss appears

$$\frac{dE}{dx} = -\frac{4\pi Z^2 e^4}{mv^2}\left[n_I \log\left(\frac{2mv^2}{\bar{I}}\right) + n_e \log\left(\frac{2mv^2}{\hbar\omega_p}\right)\right] \tag{12}$$

to depend on the plasma temperature. There are several reasons, but the big effect is caused by ionization. When a charged particle moves past a neutral atom with bound electrons, the bound electrons cannot be excited or ionized unless they receive a certain minimum energy-transfer. The average energy transfer is the Bethe-Bloch mean excitation energy $\bar{I}$. In Eq. (12), Z and v are the projectile ion charge and speed; n_e, n_I are target electron and ion densities and ω_p is the plasma frequency.

When a target is heated, it gets ionized. Its outer electrons become free and they can accept much smaller energy transfers ($\hbar\omega_p << I$), so they can accept energy even at larger distances. Since there are many more electrons at the larger distances, this should raise the energy-loss. At Livermore, we calculated the quantity $\bar{I}$, which is one of the key physical parameters in this effect.[10] Our result for $\bar{I}$ is rather accurately reproduced by

$$\bar{I}(Z,Q) = 10\,\mathrm{eV}\cdot Z \cdot \frac{\exp\left[1.29(Q/Z)^{0.72} - 0.18(Q/Z)\right]}{(1-Q/Z)^{1/2}} \tag{13}$$

where Z = nuclear charge, Q = ion charge.

Our calculation is done by a very simple method, Thomas-Fermi theory, but the results agree nicely with elaborate quantum mechanical calculations done by Gene McGuire of the Sandia Laboratories (Fig. 7). I don't have a picture of experiment versus theory, but there have been experiments to measure this. The experiments are not very precise, but they roughly agree with the theory.

Here is an example that shows how big is the predicted effect (Fig. 8). For a cesium projectile stopping in an aluminum target at three different temperatures you see that the predicted stopping power (the energy loss per path length) changes by more than a factor of two as you heat up the target to plasma temperatures. That's a big change and it affects the design of devices using fast ions to heat targets. The Thomas-Fermi formula for $\bar{I}$ is simple enough that we use it as a subroutine in our big laser-plasma code LASNEX.

OK, another subject, plasma hydrodynamics. One question of great interest is how much pressure can you make by shining your laser on a target? The answer will be sensitive to many pieces of the physics in the calculation. It is measured by looking at the speed of a shock wave as it crosses the target in one or two nanoseconds. One can measure that speed very accurately with fast streak cameras and then infer what was the plasma pressure at the point of laser impact. Here is a theoretical formula for the pressure which we worked out in 1979 using the LASNEX computer code[11]:

$$p = 8.6\,\mathrm{Mbar}\left(\frac{I_{TOT}}{10^{14}\,\mathrm{W/cm^2}}\right)^{0.82} \tag{14}$$

I_{TOT} is the total laser intensity and about 30% of it is absorbed. Experimental data have been taken at a number of laboratories over the years, and it turns out that LASNEX is pretty close to the experiments (Fig. 9). Now other people calculated $p(I)$ -- Ray Kidder calculated it. Claire Max calculated it using a theory that originally started with Dick Morse.[12] But those calculations got the power-law wrong; with LASNEX we got the right answer.

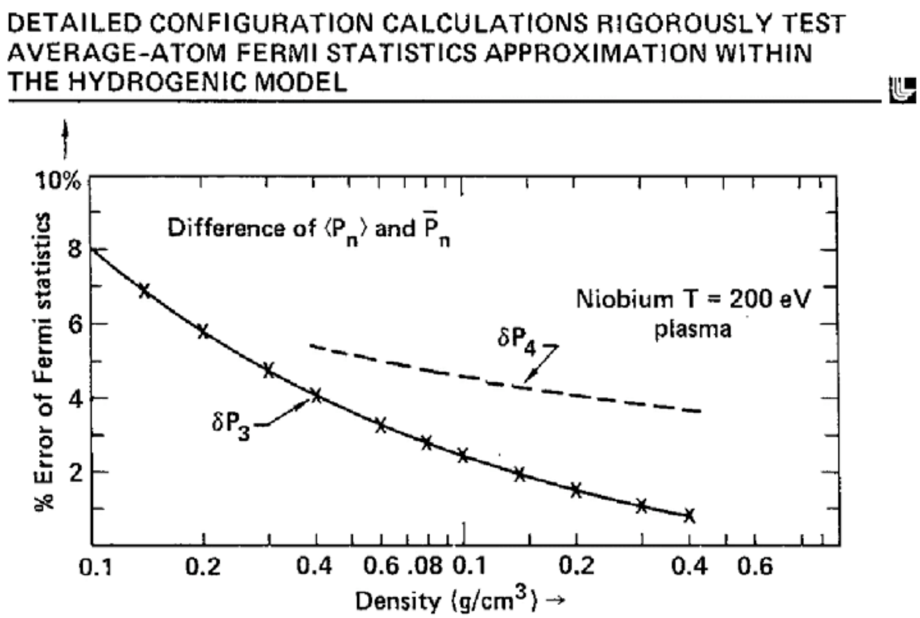


Fig. 7. A numerical test of Fermi statistics for interacting electrons in atomic levels $<p_n>$ is the actual average population of the n^{th} shell (n= principal quantum number); p_n is the corresponding prediction of Fermi statistics.

Large electric and magnetic fields are generated in laser targets, and so one is interested in the electrical properties of the plasmas. Now, of course, electrical properties of ideal plasmas are well known. Some people here in the room performed plasma conductivity calculations back in the fifties.[13] But we are thinking about dense partially ionized plasmas, and in that case the physics changes a bit. Over a period of years, we have done electrical conductivity calculations, stapling together ideas from plasma theory, atomic and solid state physics, and hopefully including enough physics to get the right answer.[14,15]

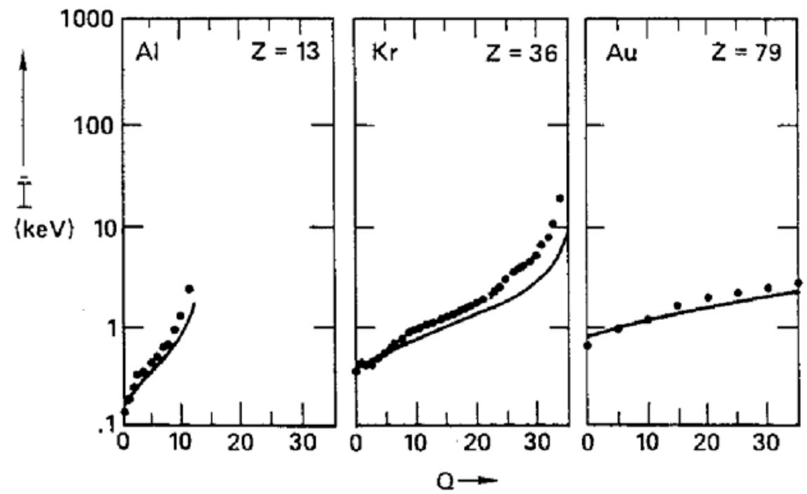


Fig. 8. Theoretical calculation of the average ionization-excitation potential I (Z.Q) for ions of Al, Kr, and Au. Points are calculations by E. J. McGuire; solid curve is the Thomas-Fermi result of Eq. (13) and reference 10. The agreement is adequate for practical applications.

This is an example of the conductivity of aluminum (Fig. 10). The solid line is our result. It matches solid state data and theory down at low temperatures and at high temperatures it joins the ideal plasma theory, and in between, it predicts the electrical and thermal conductivity and their dependence on magnetic fields. I won't describe the calculation in detail, but I can give you an idea of what the ingredients are: we have Maxwellian electrons or Fermi statistics depending on the density of the plasma. We have Coulomb scattering but also scattering by the ion core. There is provision for scattering by neutrals when they occur. We calculate the structure factor for dense plasma screening which converts over into Debye screening in the low density plasma. The electron mean free paths are sometimes as short as an atomic diameter. We use formulas that reduce to the Bloch-Gruneisen law for cold metals. However, we don't do a very good job on electron-electron scattering, and we don't include the process in which an electron hits an atom and excites or deexcites it and emerges with a different energy. We don't use the fanciest possible theory of screening, but you can't do everything. That is the TKN conductivity model, also part of LASNEX. It has been tested recently in experiments at Los Alamos and the University of British Columbia in Vancouver.[16] The code sees a lot of practical application in the study of exploding wires, fuses and so forth.

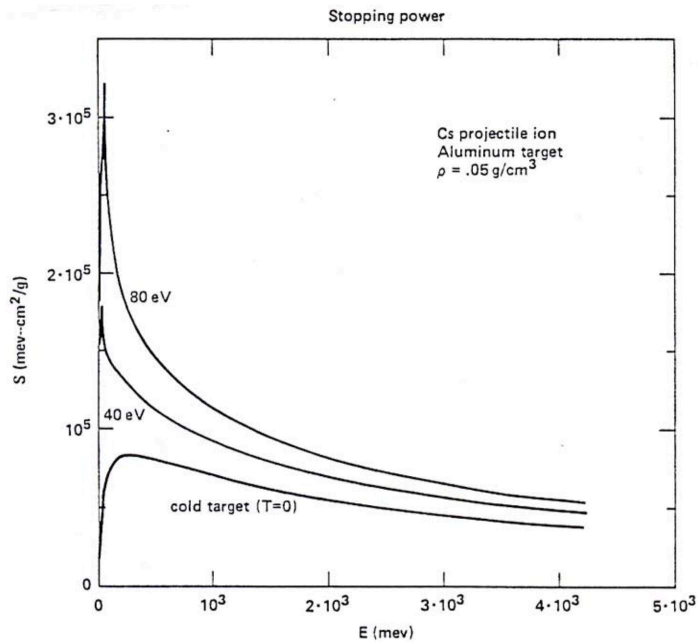


Fig. 9. Theoretical energy-loss (stopping power) of an aluminum target for Cs ion projectiles of various energies up to 5 GeV. The three curves are identified by the assumed target temperature. The temperature dependence shows the increased stopping effectiveness of free electrons produced by the thermal ionization of the target plasma. The calculations are performed by a method described in reference 9.

This conductivity model illustrates an interesting trend in large-scale computational physics, the need to build subroutine packages which represent a whole class of phenomena (e.g., conductivity) in a robust, broad-range fashion. Then the large code treats this package as a specialized expert on electrical properties.

Let's see. One more subject involving statistical mechanics: our laser plasmas are believed to be in a special kind of nonequilibrium state where the electrons have one temperature T_e and the ions have another temperature T_i. The temperatures are unequal because the large mass difference impedes heat transfer. For dense plasmas, one can develop a special new statistical mechanics, a canonical ensemble based on the idea that particles (electrons, ions) interact strongly with each other but the temperatures are unequal.[17] Then the thermodynamic relations become a bit more complicated and interesting. For example, we find a modified formulation of the first/second laws of thermodynamics:

$$dE = T_e dS_e + T_i dS_i - pdV \tag{15}$$

We find that the specific heat becomes a sort of matrix -- if you add heat to the electrons, then you automatically change both temperatures (T_e and T_i) because you change the ionization state and the ion pair-forces. All this gives a very pretty theory, which has not yet had any real experimental test.

We want to know the ion temperature because it controls the rate of thermonuclear reactions in fusion plasmas, and it also determines the Doppler linewidth, which controls the gain of X-ray lasers.

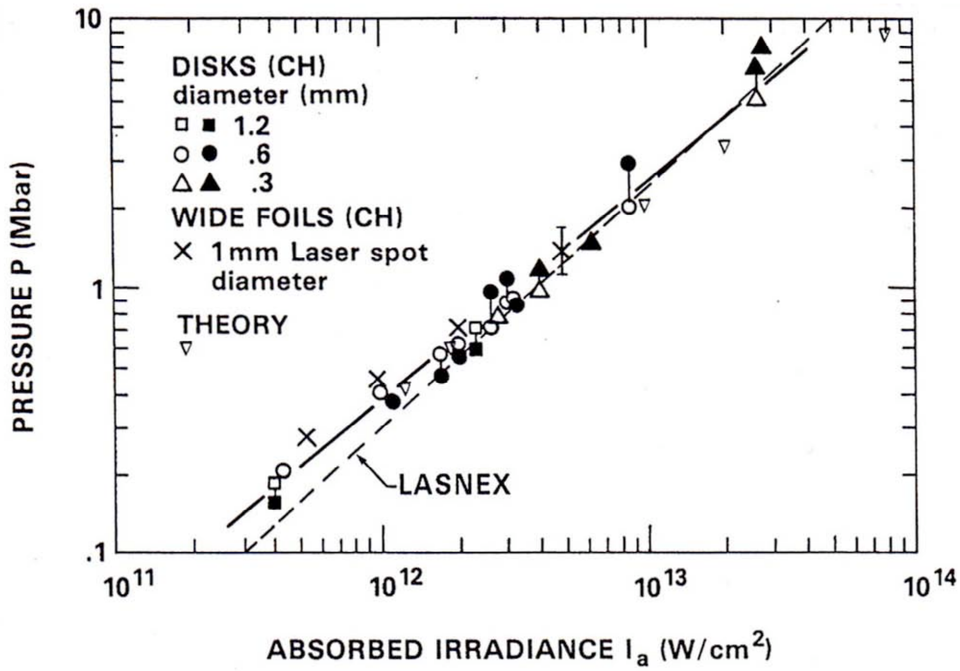


Fig. 10. Pressure generated in planar laser-target interaction at various laser intensities ($I_a \sim 1/3\ I_{tot}$). The experimental data is taken from U.S. Naval Research Lab Report 4212 (B. Ripin, et al.). The dashed calculation is Eq. (14), given first in reference 11.

I'll barely have time to mention the last subject. Of all the things that I've worked on, this turned out to be the most important. It's not my fault that it did; it wasn't even my idea to work on this. The original idea came from Russel Kulsrud, Marvin Goldhaber, and others; the idea is to use <u>spin polarized fuel</u> in fusion reactors. [18] The Princeton group worked out this idea for magnetic fusion, and at Livermore, John Nuckolls suggested we should think about it for laser fusion. It turned out to be a wonderful idea. The big consequence of having spin polarized DT (deuterium-tritium) nuclei is to increase the thermonuclear fusion reaction cross section by about fifty percent. Let's try to imagine fusion targets with spin polarized DT nuclei undergoing fusion reactions. We must ask several questions. Can you polarize DT nuclei in the first place? Can you polarize them in an actual target capsule that you could put in front of a real laser? Would the polarization still exist after you imploded the target? (My calculations seem to say "yes" to this one.[19]). And finally, would you get a significant improvement in target performance? I will just say a word about that last question. Recent LASNEX theoretical calculations show that a target with polarized fuel would perform very much better than a target with unpolarized fuel, and you could cut the laser size by a factor of two or three.[20] Since the laser costs two hundred million dollars, or five hundred million dollars or a billion dollars, you are talking about a very significant financial savings by going to spin polarized thermonuclear fuel. We are trying to encourage solid state physicists to develop techniques for spin-polarizing DT cryogenic solid fuel. We have a project underway at Livermore to do that.

Let me end with an overview of the high density plasma seen from different points of view. When people with these different points of view see a plasma, they may see different things (Fig. 12). For example, the X-ray laser people see dense plasmas as being a place to exercise rate equations and move atomic populations. And, if you look at the fine print you will see that nowadays there are many researchers working on these subjects.[21]

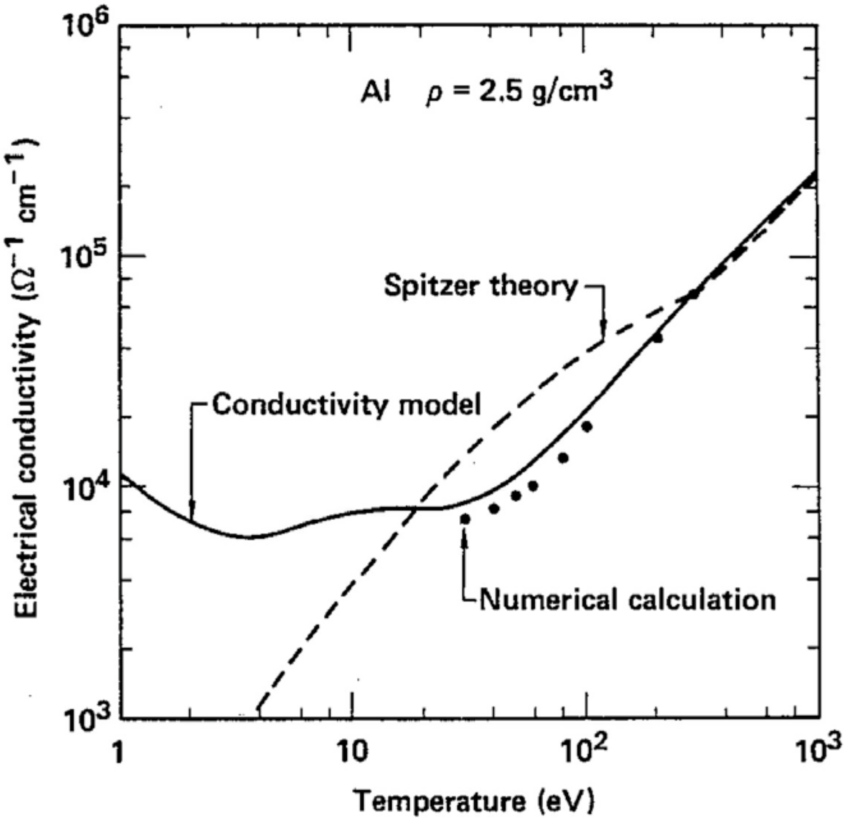


Fig. 11. Theoretical electrical conductivity for hot dense aluminum calculated by the model of Lee and More (reference 15). At low temperatures this approaches the correct metallic result, which is very different from the Spitzer plasma theory. The region near 10 eV corresponds to strong electron-ion scattering with a mean free path as short as the atomic diameter. The calculations are partially verified by experiments reported in reference 16.

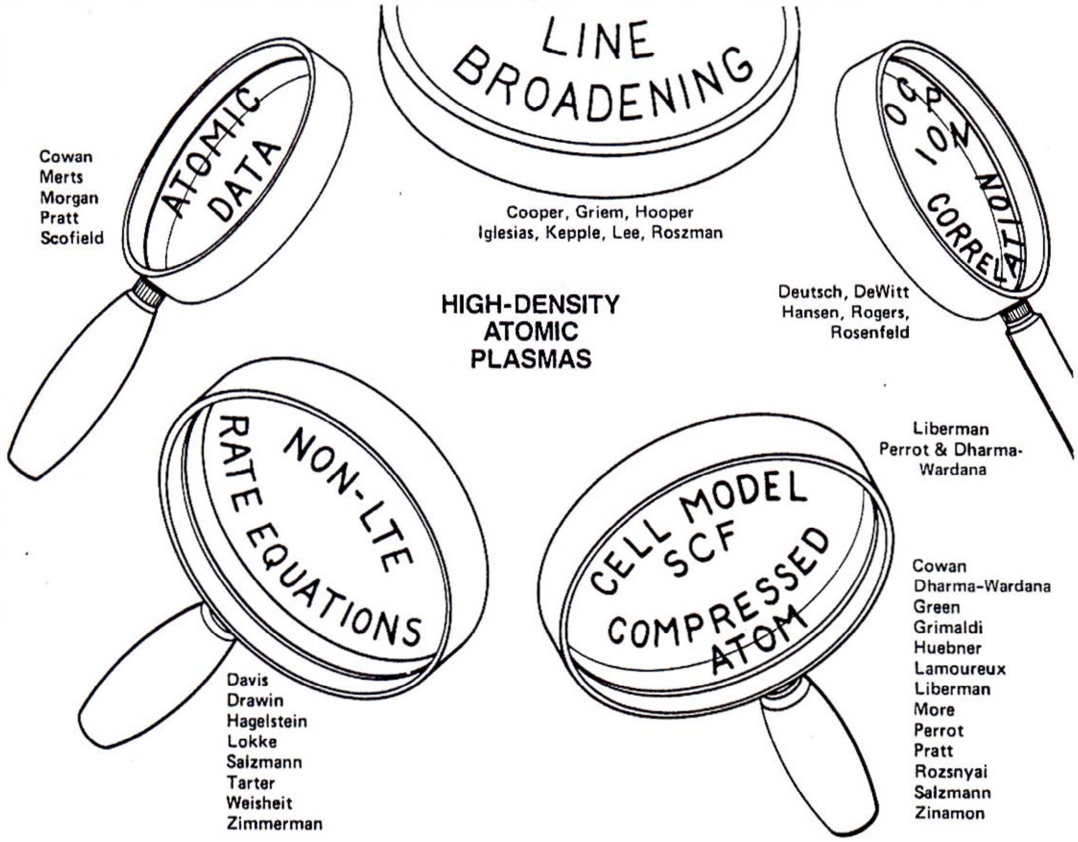


Fig. 12. The high-density atomic plasma can be viewed from several viewpoints in which one important aspect Is brought into clear focus. The names given are merely a sampling of the many active scientists in the field.

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21. Figure 12 does not attempt to list all the many researchers in the dense plasma field, and is several years old in any case. One can get a reasonable overview of the field from the recent conferences on dense plasmas; see JQSRT 27 (1982), or proceedings (to appear) of international conferences in Williamsburg, VA (1985), Cargese, Corsica (1985) and St. Andrews, Scotland (1985).

INTRODUCTION OF M. BRIAN MAPLE

by Professor Walter Kohn

Department of Physics, University of California, Santa Barbara, Santa Barbara, California

I am very pleased indeed to be able to introduce one of my favorite people, Brian Maple. I think you probably all know that there are two people in the program here who appear on two lists, namely, Tom O'Neil and Brian Maple. They belong to that original early class of students and they also belong to the present faculty. And let me just say, first of all, very simply, that the older generation of faculty are enormously proud of these two people.

Brian ‘s very much a local boy. Yesterday, I asked Nancy, his secretary, to get me his vita. This morning she gave it to me and I couldn't believe it! Here it is. You know, I have never seen such a vita, but I will get to the details in a moment. So, from this vita--with the help of this vita-- I would like to tell you a little bit about him. First of all, I said he was a local boy. He got his degree, physics and mathematics, with distinction, at San Diego State College. And then he came here, and then I have to use some sort of Rosicrucian units to convey to you all his positions. I see about three inches, single spaced, including the ranks of professorship here, and then University of Chile, Centro Atomico, Bariloche, Institute for Theoretical Physics, in Santa Barbara--I'll come back to that-- etc., etc. Then we come to research and that's about two and a half inches. Superconductivity is number one and that, of course, will be the subject of his talk, magnetism, valence fluctuations, heavy fermion phenomena, surface physics and others. I will come back to that in another context. I want first to get rid of this long piece of paper.

Committees. Now, that's incredible. I mean that's about five inches, single spaced! And that shows that he is really a very good citizen. If you were there, Brian at the dinner on Friday night when I told my little parable of good citizenship and rewards, you can be confident that you will get your just reward.

Actually to speak a little personally and unprofessionally, Brian is just building a house in Del Mar, which is about two minutes walk from where I used to live, and maybe five minutes walk from where Tom O'Neil has just bought a house. So, I feel very close to that whole situation. Of course, it also has something to do with my difficulty of not being here any more. And some things do make it easier. The fact that Lu Sham is now occupying my office makes it easier.

Well, at this point, let me say that I remember very well when Brian and Dieter Wohlleben, both of whom had come out of the school of Bernd Matthias (and I will come back to that in a moment), struck out very much on their own and, really as very young men, created an incredibly important new field of physics, the field of fluctuating valence systems. I heard quite a bit about it, but I was too stupid to really realize how important this was. I did eventually realize it. I remember when somewhere in the middle or late seventies Brian and another Brian, Brian Sales, who worked with him, gave some wonderful lectures in which they just presented this field in such a clear way even I couldn't overlook how important it was.

Well, when I went up to Santa Barbara to this Institute, then, of course, one of the big jobs I had was to see to it that we had some good programs. And there were really only two programs that I personally sort of put in-- the other programs I had facilitated-- and one was precisely in this area of fluctuating valence compounds. Now that has, in the meantime, not only become more and more important, but also really lead to what is currently probably the most exciting field in solid state physics, namely, the heavy fermion problem. I think even the non-solid state people will know that for the last two years there have been discovered a number of materials that look more or less like Sommerfeld metals--in other words, they have a linearly temperature dependent specific heat at low temperature--except that the linear specific heat may be a hundred or a thousand times as big as in an ordinary metal. Obviously these are extremely interesting and important systems. Much of the forefront research in solid state physics is being done in that area. And that goes straight back to the fluctuating valence materials that Dieter and Brian pioneered.

I mentioned Bernd Matthias a moment ago and, of course, you realize I am standing here in place of Bernd who was Brian's professor. And, some of you may remember the kind of thing that was going on between Bernd and me. Bernd was an obviously inspired experimentalist and I can't imagine that anybody would disagree with this statement. Now, what was his inspiration? Well, I claim that I was some of the inspiration! I think one of his big sources of inspiration was to prove that theorists were slow, wrong and stupid! And , furthermore, yes, another thing--there was the word predict, right, which was very important in Bernd's vocabulary. So, the fourth thing was that the theorists never predicted anything!

Well, somehow, I seemed to be just the right person to be, this symbolic theorist. And, so, there would be all these talks that Bernd would give here, and then he would look me in the eye and say, "How about that, Walter"? So, I feel I can take a little credit, and I feel very glad that--I don't know who was responsible for it--that, in fact, I was asked to take Bernd's place in this particular context.

Let me just say again before I announce the title of his talk, that Brian came up to

Santa Barbara and was one of the coordinators of this program on fluctuating valence materials. He was the only experimentalist in the Institute who was a coordinator. There have been quite a few experimentalists that have participated in these programs, but he was the only one who was actually, formally, a leader, and not only formally, but he was, in fact, the main stay of this program that was enormously effective. And it gave us a chance to become even better friends.

OK, I mustn't talk too long. I come now to the title, "25 Years of Superconductivity at UCSD."

25 YEARS OF SUPERCONDUCTIVITY AT UCSD

by M. Brian Maple

Department of Physics and Institute for Pure and Applied Physical Sciences, University of California, San Diego, La Jolla, California

Thank you, Walter, for those very kind remarks.

Well, twenty five years ago, with an uncertainty of a year or so, Bernd Matthias came to UCLJ and started an experimental research program in the field of superconductivity. I have listed in Table 1, to the best of my recollection, the people that were in the Matthias research group within the first five years of its existence. There was Matthias himself, Marshal Merriam, at that time a young Assistant Professor at UCSD, and Gustaf Arrhenius, a Professor at Scripps Institution of Oceanography who was associated with the group and has an asterisk after his name to indicate that he had a research group of his own. In fact, we are presently collaborating with Gustaf on several research projects. And, of course, Nancy McLaughlin, who was the secretary of the original group and is our secretary now. Nancy has been one of the most important members of the two groups, and she is almost singly responsible for this gathering that we are having here this weekend. The technical staff included Gustaf's brother Olaf Arrhenius, Chuck Fiore, Ray Fitzgerald and Ben Ricks. Ray and Ben are here at this Reunion. There were a number of research physicists--Suso Gygax, Huey-Lin Luo, Chris Raub, Fred Smith, and Edmund Vielhaber. Suso and Edmund have made long journeys to come to this Reunion.

There was a rather large number of graduate students within the first five years. I have listed all of the students that eventually received Ph.D. degrees and the years that they entered. There were probably five or six other students who left to join other research groups or went on to other universities. We also had many visiting faculty and research physicists from all over the world, some of whom are listed in Table 1. At the time, there seemed to be few formal boundaries between all of these people who intermingled in work as well as play, and I think that this was one of the reasons why the group was so successful from the outset.

I guess I should say a little bit about my own experience working with Bernd Matthias. If I were to try to summarize that in one sentence, I would say that it was an experience that alternated between being extremely exciting and utterly frustrating -- but it was always very interesting. And those of you who have worked with him know exactly what I mean! Because of his intuitive approach to his work, I don't think we students were really able to learn to do physics the way Matthias did, at which he was extremely successful. And, in that sense, I believe he was a truly unique scientist. Nonetheless, I think I learned quite a lot from him. In particular, I gained an appreciation for how useful new materials are as vehicles for addressing important problems in solid state physics and, also, for generating new physics, as well as a sense of the excitement that is involved in scientific research and discovery. So, in this talk what I would like to do--obviously I can't really tell you about twenty five years of superconductivity at UCSD in detail--is discuss a few examples of important achievements in superconductivity research that involved some of the people in the original group.

Let me begin by talking a little bit about high temperature, high field superconductivity. This is a subject that was worked on throughout the group for many years and was one of its major research objectives, stimulated, in part, by various technological applications. Some of these applications involve making superconducting wires with zero resistance to the flow of an electrical current with which one could make highly efficient electrical power transmission lines (if this were to become economically attractive), and electrically lossless coils or solenoids that could be used in superconducting machinery such as motors and generators. I might mention that superconducting motors and generators have been of great interest to the Navy in connection with the propulsion of ships. Other applications include superconducting magnets for producing large magnetic fields for research, confinement of high temperature plasmas for nuclear fusion, mineral separation, energy storage, and particle accelerators, such as the proposed SSC (superconducting supercollider).

This whole enterprise really depends upon optimizing a number of superconducting materials parameters which I will now briefly review. The most important of these parameters is the superconducting transition temperature T_c which determines the operating temperature of these devices. I will come to this in a moment, but the highest T_c is ~23 K (room temperature is about 300 K), so we have a long way to go to make superconducting materials generally useful without cryogenics.

Other superconducting parameters are the upper critical magnetic field H_{c2}, the maximum magnetic field in which the material can remain superconducting at a given temperature, and the critical current density J_c, which is the maximum amount of current per unit cross-sectional area that can be passed through the material in a given magnetic field and at a given temperature. A plot of H_{c2} versus temperature T for a typical type II superconductor is shown in Fig. 1.

The most important superconducting materials are the A15 compounds which have been investigated in this group for many years. I thought I would trace some of the major research achievements concerning the superconductivity of these materials (see Table 2) that were made by various people in the group over the years. The compound V_3Si is a 17 K superconductor discovered by Hardy and Hulm in 1953, two associates of Matthias. Later, based on work he carried out at Bell Laboratories, Matthias reported superconductivity in Nb_3Sn at 18 K. Shortly thereafter, Kunzler and coworkers at Bell Laboratories reported that this same material remained superconducting in high magnetic fields in the neighborhood of 100 kgauss. At the time it was widely believed that a magnetic field of the order of several hundred gauss, and certainly less than 1 kgauss, would quench the superconductivity of any substance. Thus, the observation of high field superconductivity was a very exciting result which had important technological implications and generated a great amount of theoretical effort to explain the origin of the phenomenon.

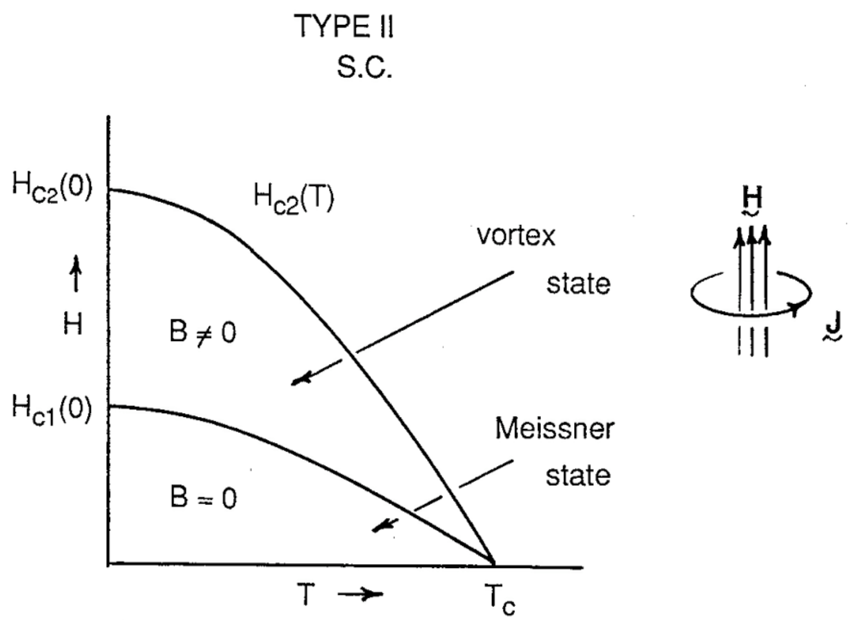

Fig. 1. Lower H_{c1} and upper H_{c2} critical fields vs temperature T for a typical type II superconductor.

Matthias, working with several researchers at Bell Laboratories and his UCSD colleagues Arrhenius, Fitzgerald, Luo and Zachariasen (who was visiting at the time) obtained another important result when they succeeded in raising T_c to 20.5 K in the Nb_3 (Al,Ge) system. Later on, George Webb, who had left La Jolla after receiving his Ph.D. in about l968 to join RCA Laboratories, found along with his coworkers at RCA that Nb_3Ga, another A15 binary compound, could be made and was superconducting with a T_c of 20.3 K. The last real breakthrough in achieving high T_c's was made some time ago in l973 by John Gavaler of Westinghouse who showed that sputtered films of the A15 Nb_3Ge could become superconducting at ~23 K, which is the highest T_c presently known for any material. Efforts to raise T_c to yet higher values are presently being made on these important A15 compounds as well as ternary compounds and even organic compounds. Also indicated in Table 2 is a paper by Al Sweedler and George Webb describing one of the pioneering experiments on the effect of neutron irradiation on the superconductivity of A15 compounds. Although he was looking forward to this Reunion, Al Sweedler was unable to attend because of another commitment. Al is currently spending a year in Washington, D. C., where he is serving as an American Physical Society Congressional Fellow.

I would say that the most important class of ternary compounds are the so-called Chevrel phases that have the chemical formula MMo_6X_8 where M can be nearly any metal and X can be S, Se and Te. Superconductivity in these materials at temperatures as high as 15 K was discovered by Matthias and his co-workers in 1972. Many other materials, too numerous to go into, were investigated in connection with high temperature superconductivity, but I believe that the ones I have mentioned are really the most important.

I thought I would show you a couple of crystal structures because one has to deal with real materials in this type of research. Shown in Fig. 2 is the crystal structure of the A15 compounds. This is actually a relatively simple crystal structure compared to what I will show you later on. The A15's have the chemical formula A_3B. The B

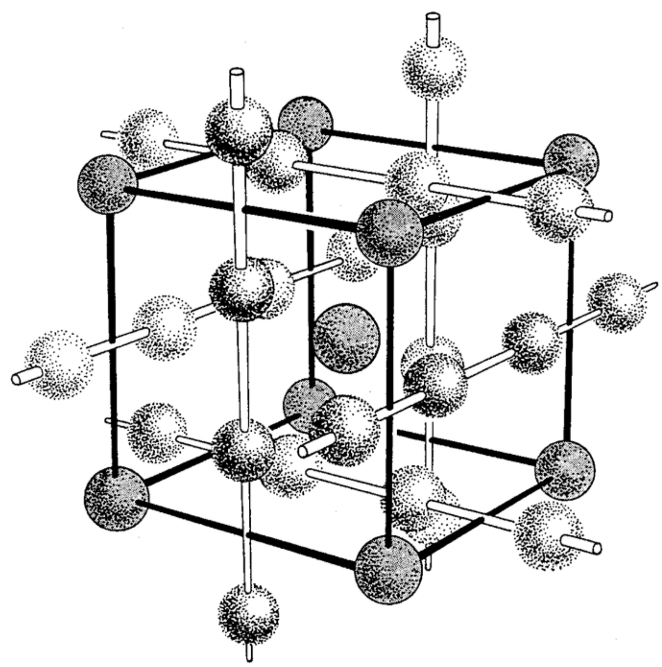

Fig. 2. Crystal structure of the A15 compounds which have the chemical formula A_3B The darker spheres represent the B atoms, and the lighter spheres are the A atoms.

atoms form a body centered cubic lattice, and the A atoms form mutually orthogonal chains that run across the cube faces. It is this chain-like structure that is believed to be responsible for the remarkable superconductive properties of these materials.

Shown in Fig. 3 are $H_{c2}(T)$ data for the two most important A15 compounds, Nb_3Ge and Nb_3Sn, as well as the Chevrel phase compound $PbMo_6S_8$ and an alloy in the Nb-Ti binary system. In addition to their high values of T_c, these materials have high values of $H_{c2}(0)$ which for Nb_3Ge is ~380 kgauss. The compound $PbMo_6S_8$ belongs to the Chevrel phases, the other class of materials I mentioned a moment ago. This compound has a somewhat lower T_c of ~15 K, but an extremely large value of $H_{c2}(0)$ of ~600 kgauss, the highest value of $H_{c2}(0)$ known for any material. Several research groups are attempting to fabricate wires of $PbMo_6S_8$ and, eventually, superconducting magnets, if the critical current density in this material can be raised to a high enough value.

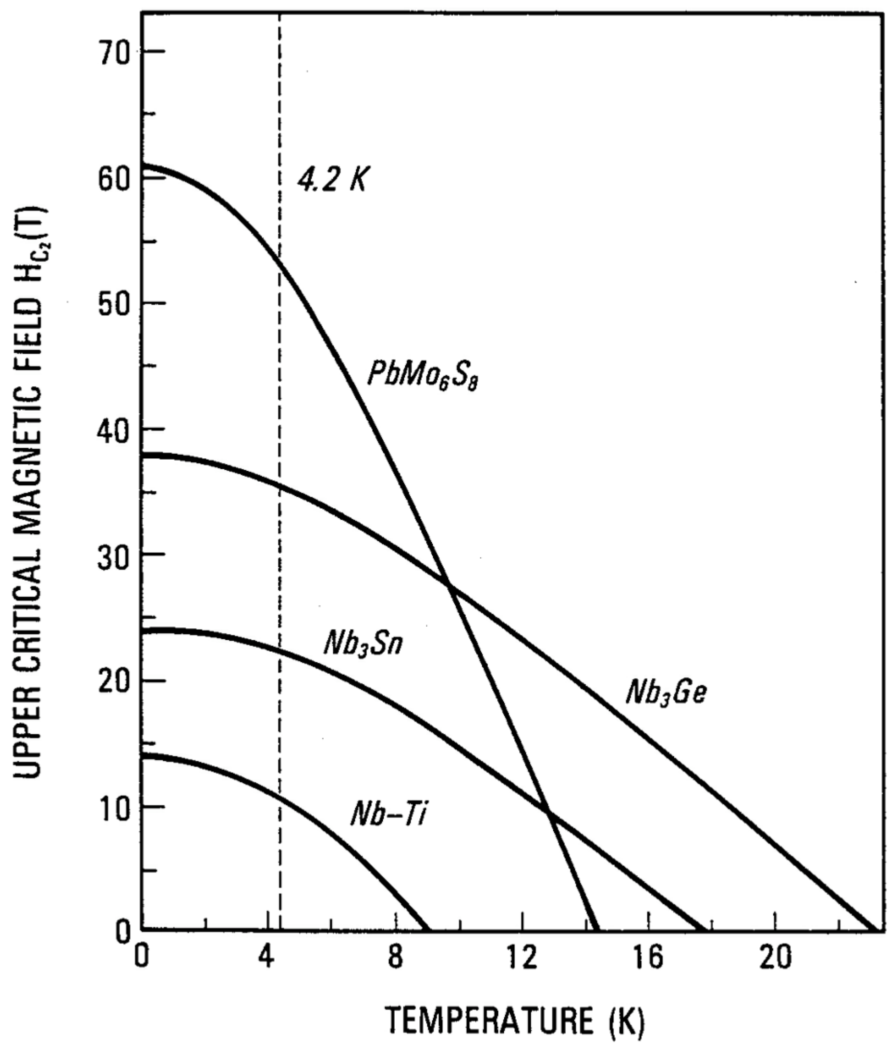

Fig. 3. Upper critical magnetic field H_{c2} vs temperature for $PbMo_6S_8$, Nb_3Ge, Nb_3Sn and Nb-Ti. After Ø. Fischer, Appl. Phys. ***16****, 1 (1978).*

So these are some examples of applied problems. I would now like to turn to some more esoteric questions involving superconductivity and its interaction with magnetism. This is a very broad subject, so I am going to be very selective here.

One of the first experiments in this field, which was concerned with the effect of paramagnetic impurities on superconductivity, was carried out by Bernd Matthias, Harry Suhl and Ernie Corenzwit in 1958 while Bernd and Harry were still at Bell Labs. They dissolved small amounts of rare earth elements, in which the rare earth had a partially-filled 4f electron shell and thereby carried a magnetic moment, into the superconducting element lanthanum which has a T_c of ~6 K. They found that rare earth impurities depress T_c very rapidly and nearly linearly with impurity concentration at a rate that correlates with the spin, rather than the effective moment, of each rare earth impurity. This lead Conyers Herring and, independently, Suhl and Matthias to suggest that the exchange interaction between the spin **s** of a conduction electron and the spin **S** of the rare earth impurity, $\mathcal{H}\mathcal{J}_{ex} = -2\mathcal{J}\,\mathbf{S}\cdot\mathbf{s}$ where $\mathcal{H}_{ex}$ is the exchange interaction Hamiltonian and $\mathcal{J}$ characterizes the strength and sign of the interaction, is responsible for this effect. The conduction electrons, of course, are the electrons that are involved in the superconductivity. This, in turn, lead to the development of a successful theory of the effect of paramagnetic impurities on superconductivity, based on the exchange interaction, by Abrikosov and Gor'kov in 1960. Abrikosov and Gor'kov also predicted the phenomenon of gapless superconductivity which was subsequently verified experimentally. Actually, I should mention that my own introduction to the general subject of superconductivity and magnetism came from some early experiments I participated in with Suso Gygax in trying to characterize gapless superconductivity. This occurred in the early '60's when Kazumi Maki was here, who was at the time generally regarded as one (perhaps <u>the</u> one) of the world's leading authorities on this subject, and he was kind enough to agree to serve on my Ph. D. examination committee in 1968-69 during one of the years that he spent in La Jolla.

Another interesting subject we worked on in the late 60's and early 70's is the Kondo effect in superconductors. When the exchange interaction parameter $\mathcal{J}$ is negative, a many body singlet state between the spins of the conduction electrons and the spins of the transition metal or rare earth impurities gradually forms as the temperature is lowered through a characteristic temperature called the Kondo temperature. The Kondo temperature T_K is approximately given by $T_K \approx T_F \exp(-1/N(E_F)|\mathcal{J}|)$ where T_F is the Fermi temperature and $N(E_F)$ is the density of states at the Fermi level. Associated with this phenomenon is the famous log T anomaly in the electrical resistivity, whose origin was originally explained by Kondo in 1960, and the formation of a nonmagnetic ground state. In fact, in the late 60's, Harry Suhl and his coworkers here at UCSD made some very important theoretical contributions to this subject. Dick More, who gave the previous talk, worked with Harry on this subject and carried out one of the first calculations of the magnetoresistance of a Kondo system for his Ph. D. thesis work. Also, Gordon Knapp, who was in our group, did some important experimental research in the late 60's that excited many theoreticians who were working on the Kondo problem at that time.

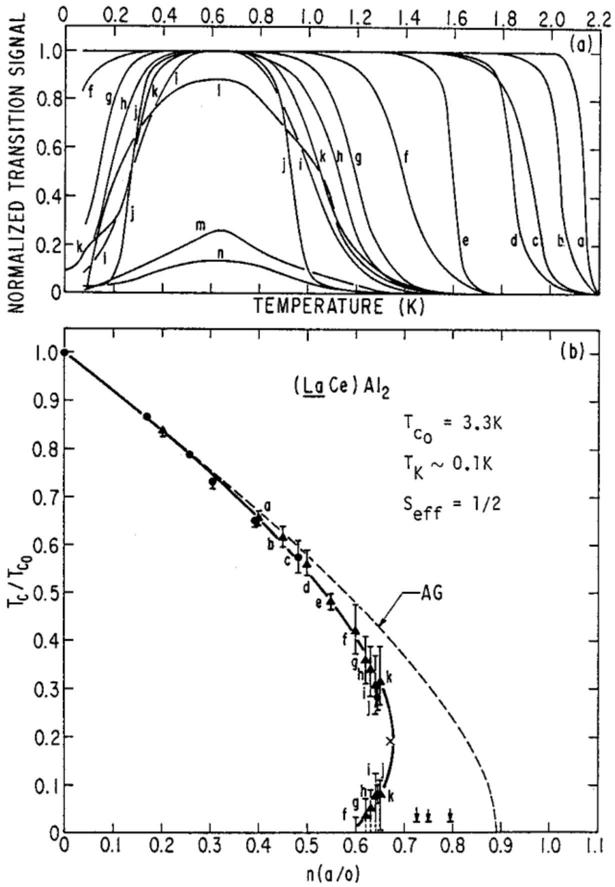

Fig. 4. (a) Superconducting transition curves for (<u>La</u>Ce)Al_2 alloys from ac magnetic susceptibility vs temperature measurements, (b) reduced superconducting transition temperature T_c/T_{c0} vs Ce impurity concentration n (at. % substitution for La) for the (<u>La</u>Ce)Al_2 system. After M.B. Maple, W.A. Fertig, A.C. Mota, L.E. Delong, D. Wohleben and R. Fitzgerald, Solid State Commun. ***11,*** *829 (1972).*

In a superconductor, this many body singlet state involving the spins of the conduction electrons and the paramagnetic impurities competes with another type of singlet state, namely, that formed between two electrons that comprise a Cooper pair. In a Cooper pair, one electron with momentum **k** and spin up is paired with another electron which has momentum -**k** and spin down. In the simplest version of the microscopic theory of superconductivity (the BCS theory), the superconducting transition temperature T_c is given by $T_c \approx \Theta_D \exp(-1/N(E_F)V)$ where Θ_D is the Debye temperature and V is the electron-phonon interaction parameter. The competition between the superconducting state and the Kondo spin-compensated state leads to a very interesting phenomenon, called reentrant superconductivity, which occurs when T_K is much lower than the T_c of the material into which the paramagnetic impurities have been introduced. This phenomenon was first investigated by researchers at the University of Cologne and in our laboratory here at UCSD on the superconducting compound $LaAl_2$ containing paramagnetic Ce impurities. The Kondo effect in the $(La_{1-x}Ce_x)Al_2$ system was discovered in our laboratory at UCSD in 1968

(M.B. Maple and Z. Fisk, Procedings of the 13th International Conference on Low Temperature Physics, eds. J.F. Allen, D.M. Finlayson, and D.M. McCall, St. Andrews, Scotland, 1968, p.1288). One of the most successful of the earlier theories of the Kondo effect in superconductors was worked out by Erwin Müller-Hartmann and Hans Zittartz in 1971, both of whom are permanently located at the University of Cologne and were associated with UCSD in the past.

Now, I would like to show you some of our T_c versus x data for the $(La_{1-x}Ce_x)Al_2$ system in Fig. 4 which we reported in about 1971. Within the concentration range ~0.6 at.% < x < ~0.7 at.%, the superconducting-normal (T_c versus x) phase boundary is reentrant, so that when samples with x-values within this concentration range are cooled to low temperature they first become superconducting at a critical temperature T_{c1} and then lose their superconductivity at a lower critical temperature T_{c2}, in agreement with the calculations by Müller-Hartmann and Zittartz. In fact, the theories of Müller-Hartmann and Zittartz predicted that there should be a third critical temperature T_{c3} below T_{c2} at which the sample should again become superconducting. We never found any evidence for the existence of T_{c3} down to 6 mK in some experiments carried out with Ana Celia Mota. However, in the meantime, several other theories have been advanced that indicate that there is no T_{c3}, while recent experiments have yielded some evidence for the existence of T_{c3}. Thus, to my knowledge, the question of whether or not T_{c3} exists is still unresolved. In my opinion, some interesting experimental and theoretical work remains to be done on this problem.

Another subject we have worked on extensively that also fascinated Matthias as early as 1958 is concerned with magnetically ordered superconductors. In fact, the whole question of whether or not superconductivity and magnetic order could coexist was first addressed theoretically by V. L. Ginzburg back in 1957, and the first experiments were carried out by Matthias, Suhl and Corenzwit in 1959. The systems that were investigated in this early work were similar to ones that I have been discussing where you embed magnetic impurities into a superconductor, and hope that these impurities will exhibit long range magnetic order so that you can examine how superconductivity and magnetic order affect one another. These early experiments were fraught with a number of experimental difficulties that I won't go into, and it wasn't until the mid 70's that a class of ternary rare earth cluster compounds were found, namely, the rare earth molybdenum chalcogenides RMo_6S_8 and RMo_6Se_8 and rare earth rhodium borides RRh_4B_4, which really allowed this whole question to be explored. These materials are characterized by two weakly interacting systems of electrons, mobile electrons that belong to the Mo_6S_8, Mo_6Se_8 or Rh_4B_4 molecular units or "clusters" and are involved in the superconductivity, and localized 4f electrons associated with the rare earth sublattice that carry the magnetic moments and are responsible for the magnetic order. These materials have been investigated extensively and some very striking phenomena have been observed. For example, it has been found that superconductivity and antiferromagnetic order can coexist with one another, although antiferromagnetic order modifies certain superconducting properties such as the $H_{c2}(T)$ curve in the vicinity of the Néel temperature. Another fascinating result is that superconductivity is destroyed by the onset of ferromagnetic order at a second critical temperature T_{c2} that is in the neighborhood of the Curie temperature, and smaller than the critical temperature T_{c1} at which the system first becomes superconducting (another example of reentrant superconductive behavior). In these ferromagnetic superconductors, the screening of the exchange interaction between the rare earth magnetic moments at long wavelengths by the supercurrents produces a new sinusoidally modulated magnetic state with a wavelength ~100 Å that coexists with superconductivity within a narrow temperature interval above T_{c2} which has been observed in neutron scattering experiments. This phenomenon is reminiscent of the so-called cryptoferromagnetic state that was proposed back in 1959 by Phil Anderson and Harry Suhl--before there were any known materials in which it could actually be observed.

Let me show you one more crystal structure (Fig. 5), the one for the rare earth rhodium boride compounds with the formula RRh_4B_4 that we have investigated extensively during the last eight years or so. The circles represent the R ions which carry the magnetic moments, while the open and shaded cubes represent the Rh_4B_4 molecular units or "clusters" that have the two orientations indicated at the bottom of the figure. As I mentioned before, the magnetic moments of the R ions interact only weakly with the spins of the superconducting electrons that are more or less confined within the clusters.

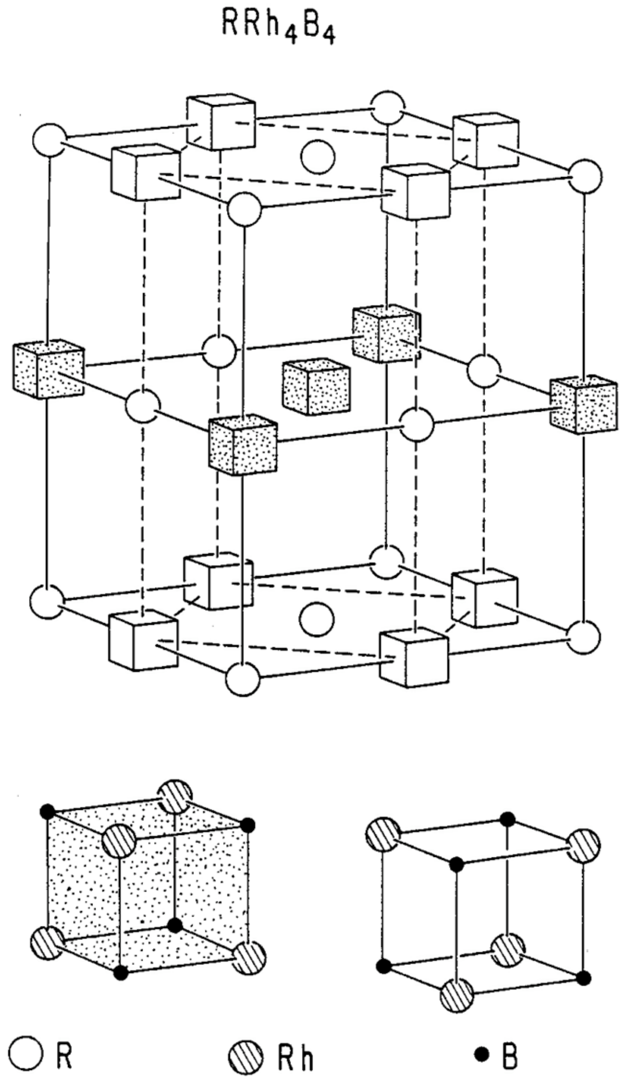

Fig. 5. Crystal structure of RRh_4B_4 compounds.

One interesting manifestation of the weak exchange interaction in the RRh_4B_4 system is illustrated in Fig. 6 which shows the ac magnetic susceptibility χ_{ac} and the electrical resistance versus temperature for the compound $ErRh_4B_4$. The lower part of the figure shows that as you cool the sample down, the resistance vanishes when the compound becomes superconducting at a critical temperature T_{c1} = 8.7 K. However, at lower temperature, the resistance again becomes finite as the sample reenters the normal state at a second critical temperature $T_{c2} \approx$ 0.9 K. The thermal hysteresis reveals that a first order superconducting-normal transition occurs at T_{c2}. Now, this transition also takes place in the vicinity of the Curie temperature of $ErRh_4B_4$, indicating that the destruction of superconductivity at T_{c2} is caused by the onset of ferromagnetism. Between T_{c2} and about 1.5 K, the new sinusoidally modulated magnetic state with a wavelength of ~100 Å that coexists with the superconductivity forms due to the superconducting-ferromagnetic interactions.

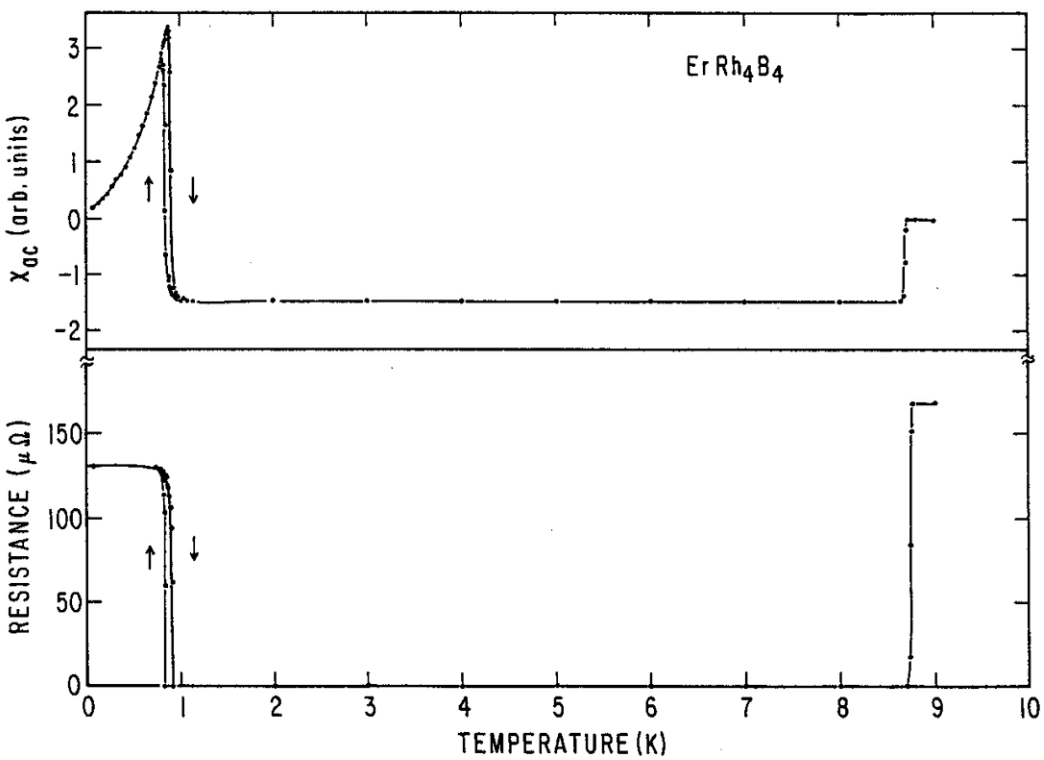

Fig. 6. ac magnetic susceptibility χ_{ac} and electrical resistance vs temperature for $ErRh_4B_4$. After M. B. Maple, H.C. Hamaker, L.D. Woolf, H. B. MacKay, Z. Fisk, W. Odoni and H. R. Ott, in Crystalline Electric Field and Structural Effects in f-Electron Systems, eds. J. E. Crow, R. P. Guertin and T. W. Mihalisin (Plenum, New York, 1980), pp. 533-543.

Shown in Fig. 7 is another example of the interesting types of experiments you can carry out on these superconducting and magnetically ordered materials. A temperature versus Ir concentration x plot for the pseudoternary system

$Ho(Rh_{1-x}Ir_x)_4B_4$ reveals a rich and complex low temperature phase diagram containing normal, superconducting, ferromagnetic, antiferromagnetic, and coexisting superconducting and antiferromagnetic phases. In the left hand part of the phase diagram, there is evidence for the reduction of the Curie temperature when the ferromagnetic state is entered from the superconducting state, in agreement with theoretical calculations. The superconductivity also changes the order of the ferromagnetic transition from second to first. Over in the right hand part of the phase diagram, the line of Néel temperatures actually splits into two, corresponding to two different antiferromagnetic structures. We have not yet determined what happens at low temperatures in the region where the ferromagnetic and antiferromagnetic phases come together. Many other complex and interesting phase diagrams and physical effects due to competing superconducting and magnetic interactions can be generated in these and other types of pseudoternary rare earth systems. Another interesting phenomenon which I do not have time to talk about is magnetic field induced superconductivity, which has recently been observed by researchers at the University of Geneva in certain Chevrel phase compounds containing the rare earth ion Eu.

Finally, let me turn to another subject during the short amount of time that I have left, a subject which Walter mentioned earlier, heavy fermion superconductors. In fact, this is where the subject of the interaction between superconductivity and magnetism is at the moment. In the ternary rare earth systems I have been talking about, there are two sets of electrons, a localized system of electrons that carry the magnetic moments and are responsible for the magnetic order, and a mobile set of electrons that give rise to the superconductivity, which interact only weakly with one another and produce the interesting new phenomena that I talked about. However, in the so-called heavy fermion superconductors there is apparently one set of electrons with enormous effective masses of two to three hundred times the free electron mass that can become superconducting, magnetic, both (over different parts of the Fermi surface), or, neither.

Most of the heavy fermion materials are compounds of Ce or U such as $CeAl_3$, $CeCu_2Si_2$, UBe_{13}, UPt_3, U_2Zn_{17} and URu_2Si_2. At low temperatures, the first compound remains normal and nonmagnetic, the next three become superconducting below 1 K, the fourth becomes antiferromagnetic, and the last exhibits the coexistence of superconductivity and charge or spin density waves (probably the latter). In addition to their low values of T_c, the heavy fermion superconductors also have very large values of H_{c2} and slope of H_{c2} near T_c, as well as unusual power law dependences of the physical properties below T_c such as the nuclear magnetic resonance relaxation rate, ultrasonic attenuation, and thermal conductivity. These unusual superconducting properties have lead to the speculation that these materials may exhibit an unconventional type superconductivity such as triplet superconductivity, in analogy with the triplet superfluidity displayed by liquid ^3He. At the moment, this subject is being vigorously investigated by many groups throughout the world, including our own. Shown in Fig. 8 are H_{c2} (T) data we recently obtained for the heavy fermion superconductor UBe_{13}. The shape of the H_{c2} (T) curve is very unusual and cannot

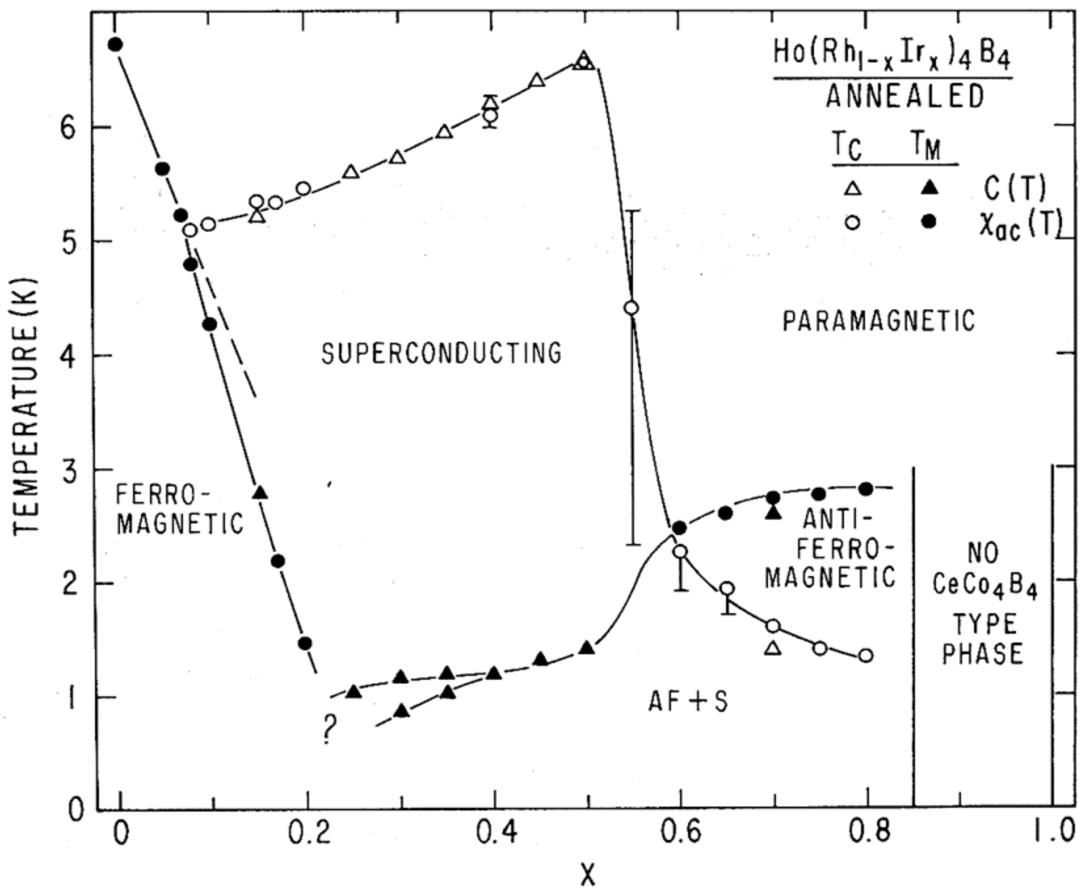

Fig. 7. Low temperature phase diagram for the $Ho(Rh_{1-x}Ir_x)_4B_4$ system. After K. N. Yang, S. E. Lambert, H. C. Hamaker, M. B. Maple, H. A. Mook and H. C. Ku, in Superconductivity in d- and f-Band Metals, eds. W. Buckel and W. Weber (KFK, Karlsruhe, 1981), pp. 217-221.

be accounted for by the ordinary theories of type II superconductivity, at least in their present forms. The initial slope $(-dH_{c2}/dT)_{Tc} \approx 420$ kgauss/K is enormous, greater than that of any other known superconductor, including the high temperature-high field A15 and Chevrel phase superconductors. This work was done in collaboration with Zachary Fisk and Jim Smith of Los Alamos National Laboratory. I should mention that Zach Fisk, who was associated with the Matthias group, both as a graduate student and, later, as a research physicist, has been one of the major contributors to the field of heavy fermion materials. In fact, at this moment, Zach is speeding through the desert on his way from New Mexico to La Jolla and will hopefully be able to join us at the farewell dinner this evening. The heavy fermion state is found in rare earth and actinide materials that exhibit intermediate valence or Kondo lattice effects which Dieter Wohlleben and I worked on back in the early 70's, as Walter mentioned. Dieter has continued to work on the intermediate valence problem at the University of Cologne and is one of the leading researchers in this field. Well, I could talk about these remarkable heavy fermion materials for hours, so I'll stop here. Let me close by showing you a list of the people that have been associated with our present research group during the last three years (Table 3) and by making one final remark about the direction of our future research. Actually, I can't predict the course of our future research precisely. However, what I do know is that we will continue to work on subjects that strike us to be interesting and important, and try to uphold the tradition of excellence that Bernd Matthias brought to UCSD back in the early '60's. Thank you.

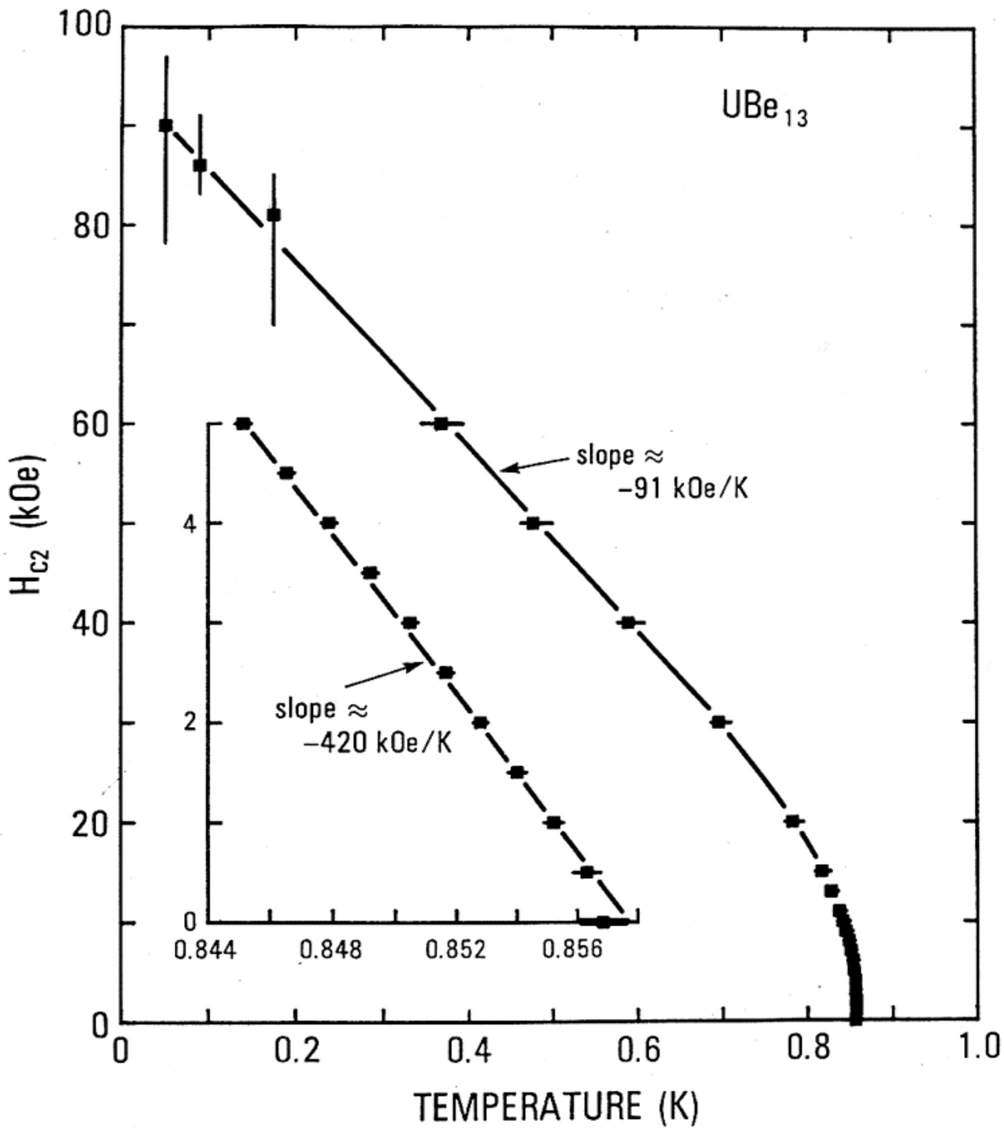

Fig. 8. Upper critical magnetic field H_{c2} vs temperature for UBe_{13}. After M. B. Maple, J. W. Chen, S. E. Lambert, Z. Fisk, J. L. Smith, H. R. Ott, J. S. Brooks and M. J. Naughton, Phys. Rev. Lett. ***54****, 477 (1985).*

TABLE 1.

UCLJ SUPERCONDUCTIVITY GROUP 1960-65

FACULTY

Bernd T. Matthias
Marshal F. Merriam
Gustaf Arrhenius*

SECRETARY

Nancy J. McLaughlin

TECHNICAL STAFF

Olaf Arrhenius
Charles Fiore
Ray W. Fitzgerald
Benjamin M. Ricks

RESEARCH PHYSICISTS

Suso Gygax
Huey-Lin Luo
Christoph J. Raub
T. Fred Smith
Edmund Vielhaber

GRADUATE STUDENTS

David C. Hamilton	'60
John J. Englehardt	'61
M. Anthony Jensen	'61
George W. Webb	'61
Gordon S. Knapp	'62
Dieter K. Wohlleben	'62
M. Brian Maple	'63
Alan R. Sweedler	'63
Tord Claeson	'63
Zachary Fisk	'64

VISITING FACULTY & RESEARCH PHYSICISTS

John Bardeen
Bryan R. Coles
Herbert Frohlich
William E. Gardner
Theodore H. Geballe
John Hulm
William H. Zachariasen
Jon Olsen

TABLE 2.

A15 COMPOUNDS

V_3Si	$T_c = 17$ K	G. Hardy & J. K. Hulm	(1953)
Nb_3Sn	$T_c = 18$ K	B. T. Matthias, T. H. Geballe, S. Geller & E. Corenzwit	(1954)
Nb_3Sn	High Field SC	J. E. Kunzler, E. Buehler, F. S. L. Hsu & J. H. Wernick	(1961)
Nb_3 (Al,Ge)	$T_c > 20.5$ K	B. T. Matthias, T. H. Geballe, L. D. Longinotti & E. Corenzwit	(1967)
		G. Arrhenius, E. Corenzwit, R. Fitzgerald, G. W. Hull, Jr., H. L. Luo, B. T. Matthias & W. H. Zachariasen	(1968)
Nb_3Ga	$T_c = 20.3$ K	G. W. Webb, L. J. Vieland, R. E. Miller & A. Wicklund	(1971)
Nb_3Ge	$T_c = 22.3$ K	J. R. Gavaler	(1973)
	$T_c = 23.2$ K	L. R. Testardi	(1973)
Neutron Irradiation Experiments		R. Sweedler, D. G. Schweitzer & G. W. Webb	(1974)

CHEVREL PHASES MMo_6X_8 - M = nearly any metal, X = S, Se,Te

$PbMo_6S_8$	$T_c = 15$ K	B. T. Matthias, M. Marezio, E. Corenzwit, A. S. Cooper , & H. E. Barz	(1972)

TABLE 3.

UCSD SUPERCONDUCTIVITY, MAGNETISM & SURFACE PHYSICS GROUP 1982-85

FACULTY
- M. Brian Maple

SECRETARY
- Nancy J. McLaughlin

RESEARCH PHYSICISTS
- Steven E. Lambert
- Milton S. Torikachvili
- Kuo-Nan Yang

GRADUATE STUDENTS
- Jenq-Wei Chen
- Yacine Dalichaouch
- Thomas W. Dyer
- Jeongsoo Kang
- Michael W. McElfresh
- John J. Neumeier
- Christopher L. Seaman
- Patricia K. Tsai
- Ming Wu
- Hu Zhou

VISITING FACULTY & RESEARCH PHYSICISTS
- Jose Beille
- Luc Brossard
- Michel Decroux
- Robert P. Guertin
- Richard R. Hake
- Takao Kohara
- Christophe Rossel
- Masashi Tachiki

EPILOGUE

Following this conference, superconductivity was discovered at ~ 30 K in the $La_{2-x}BaCuO_4$ system by George Bednorz and K. Alex Müller in the latter part of 1986. This touched off an explosion of research on high temperature superconductors that still persists after nearly two decades, although at a somewhat lower intensity. One of the most important early developments was made by a former graduate student of Matthias, C. W. (Paul) Chu, who received his Ph.D. degree from UCSD in 1968. Chu and his collaborators (M. K. Wu, J. R. Ashburn, C. J. Torng, P. H. Hor; R. L. Meng, L. Gao, Z. J. Huang, Y. Q. Wang, and C. W. Chu, Phys. Rev. Lett. **58**, 908 (1987)) discovered superconductivity at 92 K, well above the boiling point of liquid nitrogen (77 K) in the compound $YBa_2Cu_3O_{7-\delta}$. Within a few months, the maximum superconducting critical temperature T_c rose to 110 K ($Bi_2Sr_2Ca_2Cu_3O_{10}$) and then 122 K ($TlBa_2Ca_3Cu_4O_{11}$). After several more years, T_c was increased to 133 K in $HgBa_2Ca_2Cu_3O_8$. The T_c of $HgBa_2Ca_2Cu_3O_8$ can be increased even further, to ~160 K, through the application of a high pressure of several hundred kbar (C. W. Chu, L. Gao, F. Chen, Z. J. Huang, R. L. Meng, and Y. Y. Xue, Nature **365**, 323 (1993); M. Nuñez-Regueiro, J.-L. Tholence, E. V. Antipov, J.-J. Capponi, and M. Marezio, Science **262**, 97 (1993)).

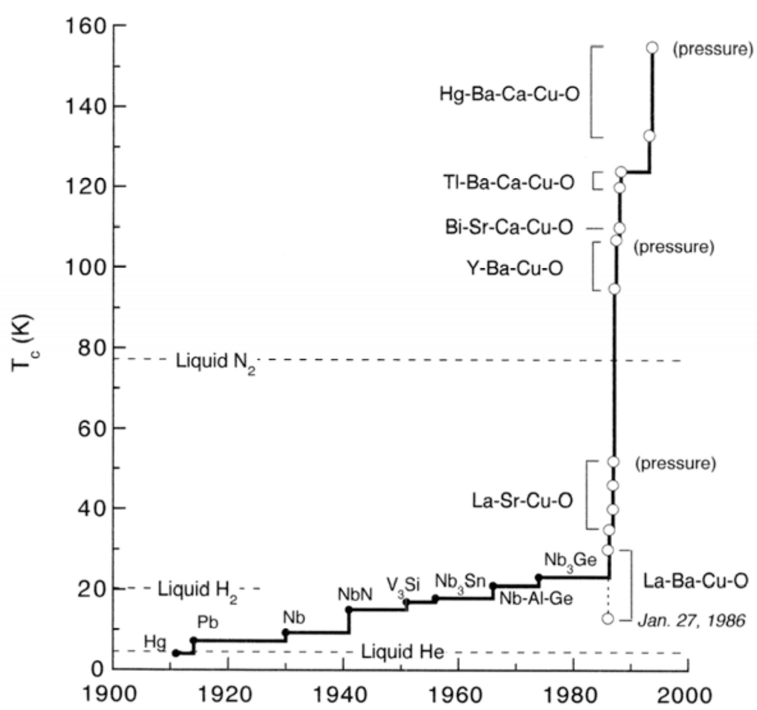

Fig. 9 Summary of increases of T_c.

These dramatic increases of T_c are summarized in Fig. 9. This extraordinary episode in the history of science has been driven by two challenging objectives: the development of a fundamental understanding of high temperature superconductivity and the realization of the widespread use of high temperature superconductors in technological applications. Although an enormous amount of progress has been much in both fronts since 1986, many obstacles remain to be overcome before these two objectives can be achieved.

INTRODUCTION OF GREGORY BENFORD

by James Benford

Beam and Plasma Research, Physics International, San Leandro, California

Well, I've certainly known Greg longer than anyone else, since I was born ten minutes before him. He has been late to most things since. I thought I would mention to start with that Norman told me outside a minute ago that he was a tough student to get through, but worth it in the end.

Now, I thought I would quickly try to tell you some things about what he has done since he was here, and perhaps some of the things he was doing clandestinely while he was here. Greg is a fellow who has run two careers of roughly equal magnitude--same order--and so I'll try to give this talk in terms of those useful things in physics, the dimensionless ratios, considering his two careers. Now, those careers are theoretical plasma physics and the writing of science fiction and some science fact. He began writing when he was about twelve years old. He wrote short stories. I am probably the only person who has ever read them. They were really terrible! And he wrote a lot of amateur things through high school, etc. He sold his first short story in 1966, when he was here. He won a prize in a contest. Then he began to write other very short, short stories. His natural medium in that time was the post card, I think. (laughter) You know being taciturn has its advantages when you are trying to get something across. It is helpful in physics papers. I wish more people would follow that. When he graduated from here, he went to Livermore and on the physics side he was there for a few years and turned out a lot of work on different subjects, and then escaped from there to UC, Irvine where he has been for thirteen years and is full professor now. And he has a bunch of graduate students and runs experiments as well as theory. His specialty in physics is radiation processes, primarily of astrophysical origin, galactic jet phenomena and collective interactions, beam plasma interactions for radiation processes.

Now, while he was doing all this, of course, he was doing his writing career as well. When he moved up north, he produced his first novel, and I want to show you some of his work. Here is the cover of his first potboiler novel, "Deeper Than the Darkness," which is a novelization of a novelette which was really rather good, but I must say the novel really wasn't very good. But, what the hell, it was a start! In those days, you got $1500 for a novel. The standard advance for novels has gone up since that time somewhere between one and two orders of magnitude--at least for Greg, which is very helpful to him living on only a UC professors salary. Now, he continued and wrote a series of books that I'll try to describe very quickly.

He wrote a short series of juveniles, and this is the first one, which, for those of you who know science fiction, integrated with Heinlein's future history series and dovetailed with some of those characters and situations. These books are mainly texts that educate people about technology and the process of growing up. He is very interested in that, and some day he'll make it!

He continued to write novels. He wrote a very successful novelette with Gordon Eckland which they expanded into a novel, "If the Stars Are Gods,"-- the story of a visit to the solar system of a race of beings that consider stars to be gods and that something lives inside stars. And it contains some very interesting depictions of alien psychology. Greg is very interested in alien psychology, and he has a book of his short stories coming out next year called, "In Alien Flesh," which sounds a little risqué. He has also been very interested in his own past. Some of you may know that we were born on the coast of Alabama and our origins are there and a lot of our conditioning, I suppose. We were only six years old when we left, but we have gone there often and he has written a novel which springs from those southern origins. It is actually set on Ganymede called "Against Infinity." This is his Faulkner novel and I think you can read a lot of Faulknerian things in that novel: concern about community and the evolution of the person in a southern type community.

Now I said I was going to introduce some dimensionless variables, to get some idea of what the ratio is between his accomplishments in both fields. If you take, for a rough order of magnitude estimate, the ratio of scientific papers to short stories, for example, the ratio is about one. There are about 50 of each, which is a lot of writing. Another ratio would be the number of novels divided by the number of thesis students. That again is about one, each number being about ten. So you can see that these fields are in some kind of rough ratio to one another.

He has written a great deal: ten novels with another one on the way. The most recent one, that very few people here have read yet, it has only been out a few months but selling very well in hardback, is "Artifact" which is not a science fiction novel, but in fact a novel about archaeology. It's about the discovery of a very interesting artifact in a Mycenaean tomb and it has a very, very interesting piece of physics in it about what that artifact really is and harkens back and eventually is elaborated in terms of the most ancient legends in western civilization. And, I think it is a very good book for physicists to read because it has a very interesting physics question in it, and there is a lot of political commentary about Greece.

Finally, I want to mention the book that everybody knows about, "Timescape". I'll show you the cover in the English edition, which, if you can get it, is the best one to read because it is much better printed. "Timescape" has been a very successful novel. Its origin was a paper in *Physical Review* (*Phys. Rev. D* **2**, 263 (1970)), "The Tachyonic Antitelephone," which concerns the causality violation which follows from the assumed existence of tachyons. They had a tough time getting it published because it basically said that all the theory being done on tachyons was worthless. They had to get Edward Teller to intervene with the *Physical Review* editor to get it published. Following that, the novel had its conceptual origins in a short story "Cambridge, 1:58 AM," which actually quoted a substantial portion of the Phys. Rev. paper. The idea kept working around in Greg's head until he finally approached my wife, Hilary Foister Benford, and suggested they write a novel together considering time travel as an attempt to modify the past from an ecologically destructive future. She wrote the English parts of it, and he wrote the UCSD parts of it, and after a year or so and a lot of work, they came out with "Timescape." It has since been published in six languages, four editions in English, one English, one American, one Canadian, one Australian -- six languages and about somewhere between 250 and 300 thousand copies in print and continues to sell very well. I think that it is, as this cover says, "the most convincing portrayal of working scientists" that I have ever seen. Perhaps it convinces me because it is at UCSD, but also because it contains all those sociological intrigues that are never referred to in the journals, but which determine the trajectory of many scientific careers. I found it a wonderful book, although both Greg and I are portrayed negatively in the book. I thought that was funny. And, a lot of people here in this room are mentioned there. So, Greg is going to talk today about, "Why Does a Scientist Write Science Fiction?" and I suspect the answer is basically, why not?

WHY DOES A SCIENTIST WRITE SCIENCE FICTION?

by Gregory Benford

Physics Department, University of California, Irvine, Irvine, California

I won't take up too much of your time talking about a book you have already read--well, some of you have probably already read. I remember very well the days when I was first here as a graduate student. What struck me at the time was how much communication there was among the graduate students and, even better, between the faculty and the graduate students. Somebody remarked at that time that the principal job hazard of being a student at San Diego was getting sun burned tongue-- because everyone talked so much.

I found it such a fascinating place that it stuck in my mind, and I eventually wrote a whole damm novel about it. It had struck me, in fact, that La Jolla was a unique place. It seemed to be invested heavily in the future--using, of course, as we all do, the taxpayer's dollars. Also, La Jolla, was about the future more than any place I had ever been,-- particularly if you come from Alabama, which is fundamentally about the past. Another place basically about the past is Cambridge, England. The reason I chose the strategy in the book, basing half in UCSD in 1962 and 1963, and the other half set in Cambridge in the late 1990's, was to talk about the difference between the two societies. The novel is based on the experience I had in Cambridge when I was there in 1976, on sabbatical leave. One evening I went to dinner at King's College--the whole high table ritual, you know, with the cracked walnuts and the port wine and the obsequious behavior. And someone told me a story I have never forgotten. They had gotten a large bequest to the College and were trying to decide how to invest it. The bursar said, "Certainly we ought to invest it in property, real property. That has stood the college very well for the last thousand years." But the oldest senior fellow in the room shook his head and said, "Well, that is true enough. But the last thousand years have been atypical." (laughter)

Well, I feel the same way. It has been atypical, and one thing I am sure of is that the next thousand years are as sure as hell going to be atypical too. Fundamentally, that is the message science fiction has to say in literature. To my mind, most literature is focused very much upon the immediate past and personal experience, without realizing what is going on in society as a whole. So I was drawn to write science fiction (although I don't write only science fiction) because it tries to talk about the impact of everything on society, not just the individual experience.

But, of course, fiction has to be about individuals. The trick, you see, is that science fiction talks about science. You might even guess that from the name, although you wouldn't guess that necessarily from reading a lot of it. You know these statements at the beginning of books where they say, these characters bear no resemblance to any person living or dead? Well, that is the problem with them, usually. [laughter] There is no semblance of real human beings, and that is the trouble with science fiction novels frequently. One of the things that I have tried to do is counter that all-too-frequent fact.

Science is the big main spring in this century. Historians will call this the century of science, more than any other century, because this is the place where it became obvious that the big driving term in the equation of society is science. In the past, for example, it may have been whatever crank religion was on the scene, or something like that. Science has really started to drive human society right into the nonlinear phase--a phase I spend a lot of time with in plasma physics and have some association with. But, now society is clearly in that regime, also.

As a scientist the first thing you have to counter is, you might say, the cult view of the scientist; this lab smock image. You see, we have become the emblem of truth. If you don't doubt it, just look at commercials. If they really want to say something is undeniable, they say it is <u>scientifically proved</u>--which means they took a poll or they asked six people. And the opposite of that, of course, is lies. Another synonym for lies is, as we know, fiction. So, how can you construct a thing that is called science fiction? What does it mean? Fiction is nice, it is pretty, it is poetic, exciting, informing, maybe even enduring, but fundamentally it is lies. So what is science fiction? Is it lies about the truth, or is it the truth about lies? Either way you choose, it looks like a mug's game.

The right answer to this is <u>none</u> of the above. Science fiction is supposed to be literature that tells us what the hell science is doing in society. One of the things that has bothered me about SF is that it doesn't seem to be able to talk concretely about scientists themselves very frequently. Instead, it is about people like star ship captains and other riffraff who will land on alien planets or world dictators and other figures close to our hearts.

I should answer the question in my title. When Brian Maple called me, he said, we want you to talk. We are having people discuss history and research and so on. Oh, I said great, Brian--you want me to talk about relativistic jets from galaxies? He said, "No." And I asked, you want me to talk about plasma physics? He said, "No. " I said, you want me to talk about surfing, don't you, Brian? He said, "No no," it's going to be the old SF talk again. So, to give the same old answer: The basic reason I write SF is that it is <u>fun</u>. I don't think you should write anything unless it is fun. So why do people have so much trouble writing scientific papers? [laughter] The <u>doing</u> of science is fun. Writing it up, though--particularly this Germanic way we have evolved--then the scientific paper is not a whole lot of fun.

I did a parody once called, " How to Write a Scientific Paper. " It was published in Omni. It was supposed to be a paper written the way scientists actually read them. So it opened with the references. [laughter] Yes, you see you all understand! [laughter] Then there came the acknowledgment. [laughter] Then the title, then some figures. That is where the paper ended! [laughter] You see, if you put that in a book, no nonscientist would understand it unless you explained-and then you would kill it. That is one purpose of art. Art is often the embalming of what was once lively.

That is largely what I tried to do in "Timescape"--particularly, in the UCSD portions--to write about our experience. What it is like to come to this blissfully beautiful place, full of gigantic minds, and similarly sized egos, and the atmosphere as it was then. Because, as you have probably noticed, it ain't that way now. It is a big, calcified University with an apparatus in place, and reputations to protect. It is not the same experience we had. I thought that was such a wonderful time, I decided to write about it.

I also wrote a fair amount of the stuff set in England. My sister-in-law, Hilary, wrote the point of view of the English housewife, which I felt unqualified to talk about. I wrote about physicists in the English environment and tried to talk of what I think is going to happen to England--and some other places, like the United States, too, if we don't change.

A lot of things happen when you are trying to write about science. Of course, I know you find our profession absolutely fascinating. Seen from the outside, stylistically, watching scientists work is essentially on the same par with watching paint dry [Laughter] But, at not quite the same pace. And in the SF media, of course, we have things like "Star Wars" and high camp SF. That doesn't have anything to do with science. What does sometimes have to do with science are films like "200l" on a higher plane. "Timescape" was the first major novel in which I really tried to just talk about scientists. This new one, "Artifact" is another such novel, although written with a much faster pace about other kinds of scientists. I got bored with physicists after a while and did a lot of work on archeology. "Artifact" is mostly about how archeologists work, and the fact that it intersects politics a great deal.

As many of you have noticed, in "Timescape" I went around, and, as every novelist does, copied a lot from real life. There is a person in this room, I stole this gesture from. [laughter] [Leans back, puts a foot flat against the wall.] Laurie Littenberg! I noticed that when I ran into Laurie here the other night, within three minutes he repeated this gesture. It was heart warming. I stole a whole lot of things from a whole lot of people. There is the character that everybody always asks me about, Gordon Bernstein, who is not a copy of Herb Bernstein, although I took some stuff from Herb Bernstein. And, you know, until I finished the novel I did not realize at all--at least did not realize consciously--that the life profile of Gordon Bernstein is exactly that of--is anybody recording this?--Shelly Schultz. Shelly is not here is he? Oh, he sent you. Yes, well, in the story, Gordon Bernstein is having an affair with a woman of another faith--there is a word for that [laughter] and there are a lot of things in there that are not true of Shelly, I think. [laughter] But many things are. He went to Columbia University, he is Jewish, he was an Assistant Professor, he was coming here to try to get some papers out so he would get tenure. We remember that, don't we? Things of that sort, but the rest of the thing, of course, was invented entirely. I stole bits of stuff from diverse people like Bud Bridges. Roger Isaacson appears in somewhat transmuted form in the book, and for a small sum I can tell you who that is. [laughter] Maybe not a small sum!

And there are a lot of real walk-on people. People like Herb York, for example, is mentioned in passing. In the Department often that was what he was doing, passing through on his way to the Test Ban Treaty or Camelot or someplace. And lots of people are in there who were on the faculty. It was not their fault, they just happened to be on the faculty and so I used their names. Gordon Bernstein, the character falls asleep in a Colloquium given by, yes, by Norman Rostoker. That's right. Can't figure why I said that.

But I take from a paper on Norman's wall a list of the evolution of the laser fusion program. I used that as a parody of what happens to scientific programs. You can find it in the book; I can't repeat it off hand. I took lots of things from graduate students--most of them unsuspecting. There was only one person whom I really felt that I had to take material from, and use as a foreground character and assign lines of dialog. I had a very clear memory of what this person had said, but nonetheless I wanted to OK it. Freeman Dyson read it, thought it was great and said fine, go ahead and use it. I Xeroxed that and sent it to the publisher because he was worried about people taking the wrong idea about themselves being presented in novels. I wasn't worried about it--scientists don't have the time to sue anybody. The person whom I did decide to disguise quite well was a person who is called in the book Saul Schriffer. Now the real life stand-in for Saul Schriffer was not on the faculty here. He occasionally passed through, like many of the self-luminous objects in our universe, giving off a lot of radiation. I was giving an invited talk at the AAAS about three years ago. I sat in the preparation room with this guy and he said, "You know, I just read this book *Timescape* because I have just sold a contract for a 2 million dollar novel with Simon and Schuster, and I was trying to figure out how to write it. We had a discussion, over an hour long, about how you present scientists, what you do about covering up your tracks, how you decide to portray people in just the right way so you get the essence of them without all the messy details. We had a technical discussion about writing. He had not thought about many of these things before and he was trying to discover them. He discussed the book in detail--this aspect and that, how you cut scenes, all this kind of detailed stuff. No where in this conversation did he give the slightest hint that he thought he might be portrayed in the book. And I am convinced that he is not at all aware that he was in the book. It was to me a revelation. Someone had said to me long ago--I think Arthur Clarke--that if you change the appearance of a character from that of the actual person, he will almost never recognize him or herself. I think it is actually true, because Saul Schriffer has no physical resemblance to Carl--what's his name? (laughter) I learned a great deal from that. So if you are ever thinking about writing a book about UCSD you can go ahead and just do anything--simply change the color of the eyes or something, and you're on safe ground.

One thing that I particularly liked about writing the book is the fact that it put me back in touch with a lot of people who were here at that time. That is why I was so happy to hear that this was going to occur. So many of the people who figured--sometimes only indirectly--in the book are here this weekend. It was a great chance to see them all over again. It is quite possible that somebody else could write another novel about UCSD in that era, but it ain't going to be easy--because there is a limited market for these things you know. (laughter) And, what the hell, you don't want to repeat.

I don't think I'll ever write anything about UCSD again. I think I've largely exhausted it. And things have changed. It was a unique era. And, of course, the point of the book is to talk about time and the fact that physical theories are not complete. There are lots of things about physics you don't understand. There are many hidden assumptions in Physics which only later, as Einstein showed, have to be reinvestigated. He used to talk about subjects like that, too.

And that is one of the reasons I think that scientists should try to write fiction--to convey to people the fact that science is not what most of those people out there think--that is, a set of received opinions, a set of frozen data. In fact, it is a giant slug fest of ideas, and always provisional ideas. What they don't get out there, is the fact that science is provisional. Often they want to turn it into a faith. That is why there is the Jonas Salk lab smock image. We have become the emblem of certainty to them. "**Scientifically true! Scientifically proven!"** And they don't get it when we say, "Hey, but that can be changed at any moment." One new fact can destroy the oldest theory on earth. They don't understand that. And yet that is the most simple thing about science.

I feel that we have an obligation as scientists to continually remind people that these are provisional truths and that we are not the mandarins of some Byzantine, complex, nonunderstandable, great set of facts. Instead, we are explorers trying to find out what is going on. Often in the physical sciences, as you know, it is like trying to take a drink out of a fire hose. It is often trying to find the fact which will reveal something, against the blizzard of facts that roars through your life all the time. Just like the blizzard of view graphs that blew through here this morning. (laughter)

There is so much complexity in science. They can't understand the difference between that complexity and the simplicity that underlies it. That is why I was happy to see Herb Bernstein talking about trying to find simplicity in science, for once. Simplicity is the only way we are going to convey to people what science is about. Complexity just makes them think it is another faith. See?

They just think it is a big, complicated machine with no hope of them understanding it. How many people do you know who are afraid to get on airplanes and have no understanding of how they work? Well, everybody that I know who is afraid of getting on airplanes doesn't know anything about Bernoulli's Law, or anything like that. They can't understand what holds it up, which leads to the fact that they are afraid it will fall down. That is a simple example, but it expresses a great deal of what people feel about science in our society.

Anything we can do to contradict that, to undermine that, to falsify that as their view of science--is a very good idea. So I would urge all of you to communicate--in what ever ways you want. Writing articles for high paying magazines like Omni or Science Digest, or appearing at the Rotarians and preaching a "faith." Anything you can do to tell people about science as she really is, is a good idea. Because, basically, you know all those tax dollars I alluded to do have to come from someone. If they are not getting the spirit of science, ultimately we are not communicating and they are not going to fund us. That is the bottom line of things.

So I would say to you all, you ought to be communicators of science, because the alternative to popular science is unpopular science!--and we don't want that, do we? Thank you.

PANEL DISCUSSION: FUTURE OF THE UNIVERSITY, THE CAMPUS AND THE DEPARTMENT

William R. Frazer

Senior Vice President--Academic Affairs
University of California

Harold K. Ticho

Vice Chancellor, Academic Affairs
University of California, San Diego

Norman M. Kroll

Chairman, Physics Department
University of California, San Diego

Moderator: **Herbert F. York**
Physics Department, University of California, San Diego

Introduction by Richard L. Morse

We are going to have a panel discussion on the future of the University, the Campus, and the Department. The panelists are Bill Frazer, now senior Vice President for Academic Affairs of the University of California, Harold Ticho, Vice Chancellor of Academic Affairs, UCSD, and Norman Kroll, now Chairman of the UCSD Physics Department. The Moderator will be Herb York who was Chancellor in our time. Gentlemen, would you come up and make yourselves comfortable.

Herb York: Some acts are harder to follow than others, but there are four of us and so maybe somehow we can cope. I'm going to follow a format that was suggested by Tom O'Neil. We will have a ten minute discussion by Bill Frazer on the future of the University, followed by a ten minute discussion by everyone, and then we will do the same for the future of the Campus and the same again for the future of the Department. In order to introduce a little discipline into this, I recommend that everybody take good notes so that at our 50th anniversary, we can compare what they said with what we are actually going to see. Bill Frazer on the "Future of the University."

Bill Frazer: I feel that the coffee break was at the wrong time. The last talk totally undermines mine. Not just by being at a level of humor and articulateness of expression that I can't possibly match, but by saying a number of things that totally contradict some of the things I am going to say. (laughter) Furthermore, I was just thinking about the statement that one should never write anything that isn't fun, and wondering what sort of talk I would give if I had to give a talk on, "Why a Physicist Decides to Do Administration." One thing I certainly couldn't claim is that any of the turgid prose that I write is the least bit fun. I had a gratifying experience at this meeting where I met one of my former students from freshmen physics, Charlotte Van Andel. She is married now. She said she regretted so much that I am not teaching anymore. That was very nice of her. I said, "No, I am teaching. I just came back from a meeting of the Board of Regents. It is just that I have a different group of students nowadays." I also have no transparencies. I haven't got to hold one of these things [chalk] in almost two years. I miss that. (from the audience--You have the wrong end-- laughter). I won't try to comment.

I am going to follow on Keith Brueckner's talk. Keith talked very eloquently about a very exciting period in the formation of this Department and the formation of the Campus, and a period of great expansion in the University of California. Keith plotted a graph, or at least he referred to a graph that was plotted in those days. I'll have only two graphs for the whole talk. I'll put them on these two black boards. This was the number of students in the University of California projected as a function of time--I forget what--I have a displaced zero here--it was something rather small, around say 1955. (Figure 1) Something that was projected to grow very, very rapidly. In fact, it was projected to grow at some ridiculous rate. What actually happened was a plateau at some period here, which we are pretty much still in, and, actually, some projections of decline that never occurred. And then the true projection of another period of quite remarkable growth. Actually, Keith said the projections that were made in the days of the master plan were really quite wrong. Actually, they were right for about 15 years, and that was quite an accomplishment. It is just that people got a little careless and kept extrapolating the same projections. All right. Now, I won't put exact dates on here. This was like the midseventies and this rapid rise occurs, say in about 1995. Now there is another curve, which is roughly speaking the derivative of this one. (Figure 2) Very large, almost zero and again a period in which there will be a large derivative. Needless to say this is what Herb Bernstein calls a qualitative calculation. But, now what is the axis here? Well, there are a number of figures it could be, and there is somewhat of a correlation. It could be the magnitude of the capital budget, a period of rapid growth, a period of zero capital budget, a period now in which the capital budget is again l50 - 200 hundred million a year. It could also be the esteem for the University on the part of the citizens of California, and there are a number of other closely correlated variables there.

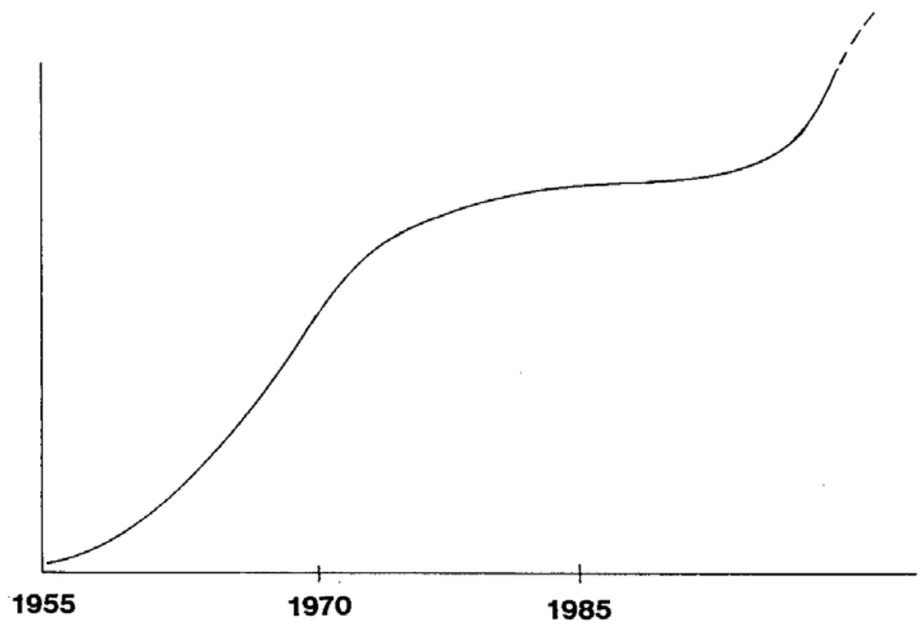

Fig. 1. Number of students at U.C.

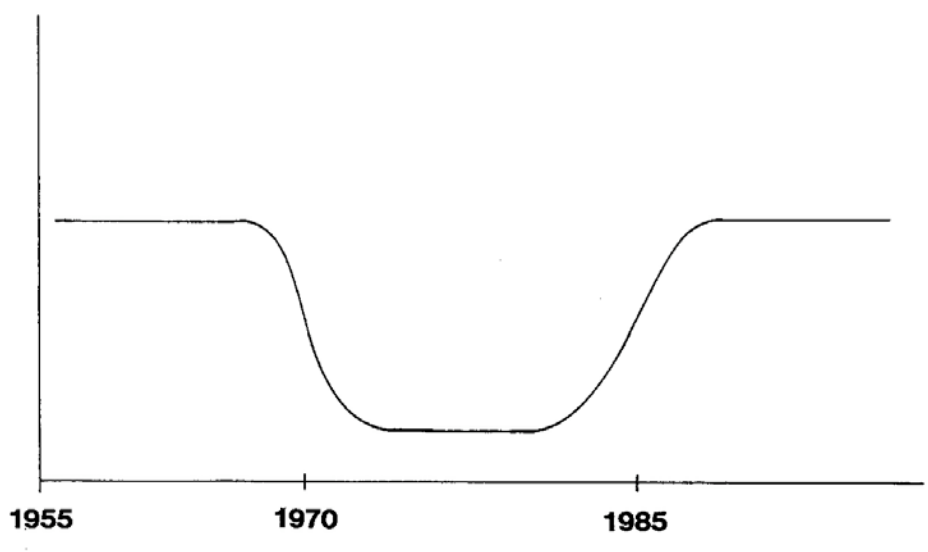

Fig. 2. Rate of increase in number of students.

We went through a period, after this wonderful period that Keith described, in which the University was almost in a survival mode for nearly two decades--about 15 years-- in which the University was held in relatively low esteem by the people of California and was beleaguered by two successive governors who held the University in even lower esteem, for totally different reasons, strangely enough.

Now, my little thesis then is that we are about to enter and have already entered another period of rapid growth, another period in which the University of California is held in tremendous esteem by the public at large in which there is a friendly governor and so forth and so on. And I think we are about to see an exciting period. You can never recreate something. You can't recreate the atmosphere of those early days. As I say, my talk has been undermined because the previous speaker pointed out, we now have a calcified bureaucracy with a great large administration. You see here some elements of that calcifying structure (laughter) but, I think really a period of great excitement is just beginning again.

I am reminded of a story that--this is of course anecdote telling time, we have all been enjoying hearing the anecdotes. I think the people telling them have been enjoying it more. It gives me a chance to tell practically every story I have ever told and retold and many of you have heard before, but this always gives an excuse to tell it again. I heard a story from Carl Eckart which was touching, though I don't think it's completely applicable. When I first met Carl Eckart at a cocktail party, and I knew, of course, of his famous contributions to the history of physics and knew of his work in the beginning of the founding of quantum mechanics, and I said, "Professor Eckart, that must have been an incredibly exciting period in which to be working." He said, "No, no, I was a graduate student, I was a young physicist and I thought that that was simply the norm. It is just that things have been rather dull ever since." (laughter) Now, I actually don't think things have been dull. And I think that in that period in which the University of California was falling on rough times, there were a lot of things going on. There were administrators struggling very hard to minimize the adversity to the system. There were faculty members who were continuing to build and to keep excitement going. The University of California didn't decline as one can see by all kinds of objective measures. But, now we are entering into a period which is just a lot more fun. Let me personalize it a little bit. I have been in the administration of the University for four years. Two of them were spent in a period of adversity and two of them were spent in this just incredibly rapid turnaround in a period of expansion. When I arrived, we were predicting a decline and planning for it. We now see that is wrong. We have a new round of planning. We are planning for expansion. When I arrived the faculty salaries were measured and projected to be about l8% behind the institutions we compare with. In a perfectly chosen period of one year and one day, the faculty salaries at the University of California went up 24%, and that is not counting merits and promotions. The capital budget went from zero to this level of 150 to 200 million. The first year I was there, I feel I really did accomplish something. I was charged by David Saxon as he went out the door and said," I am going to spend a few months in Cambridge. Figure out how to subtract 20 million from the budget.

We have subtracted all we can from maintenance and such things. This has to come out of academic programs." So, we got it down to 7 rather than 20, but I had the very challenging intellectual experience of deciding how to do this; helping to decide how to do this without damaging the highest quality programs in the University.

I was delighted that scarcely a dollar of it came out of UCSD, but that happened on the grounds of objective measures and -- it really did (laughter.) I really wasn't criticized much for that at all because I actually had some numbers I could quote. The quality of this campus, not just the Physics Department, is really incredible measured by some objective, really objective, standards. Well, anyway, the first two years were exciting. Trying to minimize the damage to the University. The problems were exciting. But you really are greeted with a lot more enthusiasm from your colleagues in the University when you are in a position of talking about expansion and handing out money than you are when you are figuring out where to take it from.

Now, just a few more facts on which to base these rosy projections. The people of the state of California now hold research in very high esteem. Now, this is very remarkable, too. And, a subject which the previous speaker talked about in a different way. When I first began testifying before legislative committees, I was grilled about such things as why did the faculty spend so much time in research and why don't they teach more. I don't hear that any more. People are now convinced, perhaps even overly convinced, that the health of the economy of the State of California is dependent upon research done in the Universities. We could conceivably have a bit of a backlash when they realize how long it takes for the work done at the SSC [Superconducting Super Collider] to be applied to the economic health of California, but nevertheless there is this atmosphere in which research is appreciated. We don't have to apologize for it anymore. We have a Governor who values the University, who values education, who values quality. There is the possibility of an economic decline which of course very much effects the quality of life in all things that are funded by the State. But, all the indications are upward and we have a very, very exciting period in which to plan.

Herb York: OK. A brief discussion first of Bill's paper by the other panelists or questions or remarks from anybody.

Herb York: Yes.

Peter Feibelman: Where is this exciting expansion going to occur? Is this campus going to double in size or something?

Bill Frazer: In a sense that's for the next speaker, but a lot of the expansion will be here. I think one remarkable thing is that it will occur everywhere, except at the campuses which are already the size they consider to be saturated, namely

Berkeley and UCLA. The expansion will occur at least as dramatically at Irvine, the expansion will also occur at Santa Cruz and Riverside.

Tom O'Neil: You have 1995 as the onset of the expansions. Is it happening earlier or is that much lead time required?

Bill Frazer: Well, the figure is inexact. Let me correct it just slightly here. It is already occurring and it will become very rapid around that time. Now we are in a period of demographic decline. The number of high school graduates is declining, but the number of applicants to the University is going up despite that. Around l995, the number of high school graduates takes off again. So, we have got to be ready.

Walter Kohn: What about relative demographic changes that are taking place in the State of California, that is to say, it is rather explosive, but there still are minority groups.

Bill Frazer: Very important issue. I thought a moment about whether to talk about that, but I decided not to try to make too many points. From one point of view, that is very worrisome because the minorities, the under-represented minorities, that is Blacks and Hispanics, participate in the University to a much lower degree. Less than a third of the participation rate of other groups. And the Hispanic population is where some of the most rapid growth is taking place. This is a major problem for the University and I think I could best express it by quoting from a talk by a prominent Hispanic legislator. He gave a talk in Sacramento at a panel discussion on demographics and looked at those University representatives who were there and said, "Think about how many Hispanics there are going to be in the University in the year 2000 because I don't know whether there will be very many or not, but I can tell you where they will be and that is in the legislature." So, that is a very important issue, you raise. On the positive side, this year for the very first time the entering class at UCLA reflects in all of the under-represented groups what's called parity with the high school graduating class. It is a remarkable accomplishment. l6% Hispanics in the entering class at UCLA. Perhaps there are some signs of hope in that area.

Greg Benford: Why are we getting a higher percentage of high school students now?

Bill Frazer: We have done some inconclusive studies and so I think I'll just give my own gut answers. There are a number of contributors, actually. You are at Irvine. One thing that is happening there is a large influx of Vietnamese refugees. That is one factor. Another is a somewhat higher participation by Hispanics. Another factor is a recognition on the part of students who might have gone to private universities that this place is just one hell of a good buy--high quality, and low fees.

Margaret Maple: I think I can make a comment about that. I'm a high school teacher and concerned with this point. Not only are more students applying to the University of California, but the quality of students is definitely improving. I teach students that rank in the upper 2% of high school graduates, National Merit students and so on, and they apply to the University of California. They came here for economics and because the University of California very, very wisely decided to give students who took a very rigorous high school program extra points. For example instead of getting a 4.0 for an A, they give them a 5, instead of 3.0 for a B, they give them a 4, and so they have really gone after the best students. And, they are coming.

Bill Frazer: Maybe one more question and then we will go on to the next panelist.

Question from audience: I was going to ask about the loss of a computer research center to Texas and the reason they cited is the lack of support by the California educational system.

Bill Frazer: Yes, that was a wonderful event. That was an absolutely wonderful event. When Admiral Inman quoted the reasons for going to Texas, he wrote them in a letter to me which I read before legislative committees every year. One of the main factors was the lack of evidence at that time of commitment on the part of the state to education, the University of California, and, particularly, graduate education. That letter has been very useful! Incidentally, there were two things I wanted to mention, because they tie in with efforts here, and simply neglected to. I just want to mention them as evidence of the vitality of the effort, the excitement. One is the accomplishment of building the Keck telescope, the world's largest telescope on the top of Mauna Kea that Harold Ticho spent a large fraction of his recent life making possible. And, now I am taking over on that and enjoying it immensely. Secondly, this is not a fait accompli by any means, but Dick Lander, is Project Manager of our project to bring the SSC to California. Whether we get that or not, I think we have an excellent chance. We are in collusion with OPEC though to make sure that the Texas economy is depressed. I think we have an excellent chance, as good a chance as anyone of getting the SSC, and the leaders in the state, the Congressional Delegation of the Legislators, and the Governor, are tremendously enthusiastic about that effort. That again is somewhat in answer to your question.

Herb York: OK. Thanks, Bill. Next, Harold Ticho on the future of the Campus.

Harold Ticho: During the coffee break, one of the distinguished members of the Physics Department has urged me to, for God's sake, talk like a physicist and not like an administrator! Given the fact that Norman Kroll is going to be talking about the future of the Physics Department, this leaves me a rather narrow area to talk about. So, you shall have to bear with me. I will try to be brief.

As Bill Frazer mentioned, the last four years have seen a very marked change in the fortunes of the University of California. There were two years of rather serious problems and a downturn and then two years of very rapid growth. I cleverly left UCLA two years ago and became Vice Chancellor here just as things started taking off on the positive side. So, it has been a very, very exciting period and a very stimulating one.

It would be tempting to talk about what UCSD will look like 25 years from now, but I will not do that for all the obvious reasons. I would like to talk about what could happen during the next five or ten years. First of all, the size of enrollment that Bill Frazer alluded to. Enrollments here on this campus have been growing very rapidly during the last four or five years--in fact, about 7% per year. That is twice as fast a rate as was anticipated when the enrollment plan was first worked out most recently in the early eighties, 1981 and 1982. And, this is clearly a rate which we cannot sustain indefinitely. Nevertheless, it has been necessary to start looking at a new enrollment plan and we have submitted a new plan to the Chancellor which ultimately will wend its way to the President. This plan is based on the kind of a curve that Bill drew, but tuned to the San Diego area. What we expect is something of the order of a 25% growth by the year 2000. Now, as I said, in the light of this we have to revise the enrollment plan that was established in the early eighties and what we are projecting now is that by the year 2000 our undergraduate enrollment will be somewhere of the order of 17,500 in contrast to 15,000 which was the number which was bantered about in the early 80's. I might mention that when UCSD was first founded a total number of students 27,500 was envisaged in the steady state. Later this number was dismissed as impossible. But, actually of that 27,500, 17,000 were supposed to be undergraduate and 10,000 graduate so that the number that we are getting to is just about the number that was initially foreseen. There is one fly in the ointment and that, of course, is that as a consequence of the rapid growth of undergraduates, the ratio of graduate students to undergraduates has been declining.

Now, of course, with the growth of the enrollment, also comes the growth of support and particularly, of faculty positions. In the last four years, the number of new faculty positions on the campus has grown by about 150. And if the enrollment projections until the year 2000 hold and are accepted and actually materialize, we expect to get another 350 positions during that period. So, one of the very important things that we face is a very energetic recruitment program of new faculty. When you also fold in the number of people who will be lost by attrition, we will have to be hiring about 40 new faculty members each year.

One of the advantages of growth is that new capital projects become possible. We have during the last two years been engaged in some very extensive general campus studies. You may not think that it is very important, but there has been a very extensive campus traffic study to find out how traffic will move with a very much larger faculty and a very much larger student body on campus. There has been a student housing study. We have also initiated plans for the creation of a new college. And, of course, there are detailed development plans now for various college regions. There is the Miramar region which will now become the formal home of Warren College, and a Mathews region which will become presumably the formal home of the next College.

If you walk around the campus you can see that there are all kinds of new research buildings going up. It turns out that most of those are not State funded, but are funded from private gifts or from funds of the office of the President, or from internal campus funds. I just mention the Supercomputer Building which is going up north of here, the Center of Magnetic Recording Research Building, the Structures Lab Building, the Molecular Genetics Building that is going up right next to you here. There are two other buildings which are in various stages of planning. As you probably have heard, we have gotten quite a large gift from the Hughes Medical Research Institute, which will add a second part of a Molecular Genetics Building. We are also looking into the possibility of a center in education and research and applied mathematics. As to State funded projects, there is a huge engineering building which will start construction this year and will be finished in l987. The Physics Department ought to be particularly interested in the next project which will be a teaching and research building for social science departments, but will also house the Physics Undergraduate Labs. We have just submitted a proposal for a substantial enlargement of the central library and if our enrollment plan is accepted, we will have to start worrying about a second Instruction and Research building which hopefully will be finished around 1990 to 1992.

So, to be brief you can see what we can foresee for the next five to ten years is a continuing growth of enrollment, growth of the physical plant and certainly not the kind of calcification that has been referred to a little bit earlier.

Now, of course, all of this is just sort of the superstructure and one really worries about the quality of the University and one worries about new programs, new organized research units, because those really are at the core of things. As I said a moment ago, we have a very substantial recruitment program ahead of us. The long range programs for the next 25 years will, in fact, be developed by the faculty that we bring here. It would be inappropriate for me to describe all the programs that people that we hope to bring here will start. The main thing is to be sure that the, new faculty brought here is of the quality which at least equals the very high quality that this University has achieved.

Let me tell you about a few of the academic programs which are sufficiently far along right now so that we can hope that we will see them in operation during the next five years. One very important project is the School of International Relations and Pacific Studies. That will be a new graduate school, a professional school leading mostly to a Masters Degree. It is intended to be a school to bring together quite a few of the efforts that we already have in place. We have a Chinese studies program, we are developing a Japanese studies program, a Malaysian studies program and we have a Latin American development program. I might say that this particular project has received a great deal of support from the Office of the President and also, we had some very positive signals from the State.

In engineering, of course, the Magnetic Recording building is being completed. It is a nice model of cooperation with industry. The Supercomputer Center, we hope will energize our Computer Science Department as well as Applied Mathematics on this campus. Of course, it will have an impact on all of the disciplines that require large scale computing. The Structures Lab is a national facility to study the seismic response of structures. There is one in Japan and ours will be the national facility for the United States.

In the natural sciences, we have submitted a proposal for a Center in Education and Research and Applied Mathematics. We hope that will be successful and should provide a strong boost to applied mathematics on this campus. I think that most or many of you know that we have made recently some outstanding appointments in pure mathematics, and this is an attempt to bring applied mathematics to the same level of excellence. This would be of interest to Physics because of its association with nonlinear sciences.

We are also looking for means to strengthen our research effort in the molecular biology of plants. It is time to apply to the flora what we have learned about the molecular biology of the fauna. We are pushing programs in molecular neurobiology and are beginning discussion of human molecular genetics with the School of Medicine.

Herb York: OK, there is time for some questions and (inaudible).

Bill Thompson: I noticed a correlation between these two talks. You mentioned the fraction of graduate students is unfortunate. Bill told us there was a new enthusiasm for research. Is there any mechanism by which we can tap one of these to stop the other.

Harold Ticho: All we can do here is to plead for more graduate students, but Bill has an ear to the Legislature and probably he should answer.

Bill Frazer: Oh, just a word. Yes, this is a very important planning issue. And the knighted planners in Sacramento that have been trying to restrain the growth of the number of graduate students realized in this new climate, they have got to reconsider. So, we are negotiating.

Bill Thompson: Will this correct the situation?

Bill Frazer: Yes, it will be OK. You will have the opportunity to accept all of the good graduate students you can get and place.

George Webb: I have a comment about the traffic study. I was looking at an old master plan for this University in l964, long before you came here. One of the things that it showed was an aerial tramway from the upper campus down to Scripps so you may ask Vice Chancellor Kennedy when we can expect that. (laughter)

Harold Ticho: I shall make a point of bringing up that subject.

Unidentified: Did you say that the high bay research area is still progressing along with the new buildings.

Harold Ticho: Yes. When I mentioned the I&R building, I mentioned the undergraduate labs for Physics and one other item for physics in that building in a high bay lab for research presumably primarily for elementary particles, and also space physics.

Herb York: OK. The last speaker is Norman Kroll who will talk about the future of the Department.

Norman Kroll: Well, everyone has, at least my two predecessors have, referred to this sudden change in the climate, and I actually took on the Department Chairmanship while we were still in this very flat portion and with great reluctance, actually, because I was sure it was going to continue that way, indefinitely. Fortunately, it changed. I didn't have to wait even as long as Bill had to wait for it to change. It changed, I would say in perhaps half a year. So, after a very long period in which the Department could make no appointments at all, I thought I would just say a few words about how I view the different situation now.

We currently have about thirty-two faculty members. There are a number of ways I might put that but I think that is probably accurate enough. We have actually two unfilled positions that we have been recruiting for. Within the next nine years there will be eight retirements, and I believe that we can expect to make and certainly need to make over the next ten years at least twenty new appointments. I mention those two numbers because twenty is a big fraction of thirty-two, and it means that the entire face of the Department will be reformed in this ten year period. And the future of the Department depends very much upon how much wisdom with which we proceed to carry this out. I would say if I were just to talk about areas which the Department covers in one way or the other, we have a very substantial condensed matter physics program, we have a particle physics program, we have an astrophysics and space physics program, a plasma physics program, which in fact, you saw well summarized by Tom O'Neil in his talk, and we also have a biophysics program. And then there are individual interests of various faculty members which go outside those groupings, but those can be mentioned as the principle groupings. And the question that one could ask is, "Well, is that how it is going to continue, or will there be new fields we will want to develop, what new trends can one see." Well, I am not very much of a futurist, so in terms of looking at new trends, I can only look at what is sort of right in front of me. There is the project for nonlinear science which undoubtedly will have an influence on the nature of appointments in some way because I think the existence of that project simply represents the fact that all of the fields that I mentioned have important interactions with that general area, and it seems to me likely that appointments made in those areas will be of such a nature that some of them will interact rather strongly with the project for nonlinear science.

The Department doesn't really have people in it at the present time that I would call mathematical physicists. It may well be that that should change because first of all, it is quite clear that particle physics is evolving in a way in which mathematical physics plays a much more central role than perhaps that sort of mathematics did in the past. Also the general area of nonlinear dynamics reflects the same thing and it may well be that is an area where there should be some motion.

Then it seems clear to me that there will be a lot of interest in applied physics. Many people in the Department already have shifted their interests in that direction. I think the Center for Magnetic Recording Research is an example of a very applied field with which the Department will be interacting. Free electron lasers and particle accelerators got mentioned earlier today. Those are just examples of applied areas. I think much of solid state physics is characterized by its close connection with application. So, those are a few trends that I could see becoming relevant in the relatively near future.

When one looks forward to how these twenty appointments are going to be made, of course, one has to worry about priorities. I think we have already spent a lot of departmental time talking about priorities, and I am afraid it is not easy to get agreement in the Department as to what the priorities should be. (laughter) I think there is a general subagenda, a subconscious agenda, which people probably have, but there certainly is no formal agenda. I think with as many positions as we hope will be available, and expect will be available, I think the following view has sort of emerged. One is that really all of the areas I mentioned are to some extent undermanned. That really came about as a result of the fact of when the Department was planned and started, it expected to be much larger than it is today and that simply resulted in these areas not being fully manned. If one looks at the number of positions which are likely to be available, there is the feeling that resources will be available to support all of the areas and the actual priorities and the way that they are developed really depends upon the excellence of candidates who can be recruited. Now, when Keith Brueckner opened this session, he explained about how easy it was to recruit in the days that this campus started. I would say on the basis of our current experience, it is not quite as easy now as he found it then. Maybe he was better at it, but we do intend to pursue the same level of quality that was pursued at the start. And it would be nice to do even better. So, that does make recruiting a complex problem, but I think it is the major challenge which faces the current faculty to provide the kind of reforming that will make the anniversary 25 years from now as satisfying as this one has been.

There is one other aspect of it which seems to me to be also very important which I would like to mention, and that has to do with the recruitment of high quality graduate students. Again, I would hope that in 25 years from now when the graduate students who are more likely to be from this period would come back present talks, I hope again we can look at that period with the same satisfaction that we can look upon the talks which have been given here. And that depends, as I said, very much upon the quality of students that we can attract. Those of you who were our students and who have commented about the quality of the education that you got here, I think you can help us a great deal in the outside world in letting students know that this is a good place to come and steering the best students you know of--the best prospects you know of--in this direction.

Herb York: We have ten more minutes for questions and still be on schedule.

Unidentified: With the large number of potential openings you have, one thing you might consider is a technique that was used at San Diego State many years ago in their English Department where they recruited a number of actually fairly senior people from all over the country. They really had a fair drawing card by suggesting wouldn't you love to come live in San Diego in that case and La Jolla in your case.

Norman Kroll: Well, I should say that we don't have twenty positions now. I'm projecting something over the next ten years. That of course is the method that was used to some extent when this Department was founded and I think that it is probably true that we would do well to make some relatively senior appointments. And by relatively senior, I think of full professors in the early forties or late thirties which is the sort of scheme that was used 25 years ago.

Peter Feibelman: Out of curiosity, since you want us to recommend graduate students, what does a TA make these days?

Norman Kroll: Well, it is not bad. I think we have a pretty good package now.

Peter Feibelman: How close do they live, for example, within twenty miles? ten miles? Comment from audience (inaudible)

(Graduate Student) I'm an entering graduate student and after you paid out -of-state tuition, I was making less money than the students of the University of Colorado and the rents here are just about 50% more than the University of Colorado. So I must spend everything I make, a little bit more, to live in Del Mar in the lowest housing I could find. The house I got has all the amenities, two car garage, everything. I felt I'd arrived. I don't need that, but I can't live anywhere near campus without getting it.

Bill Frazer: Remember you only pay out of state tuition for one year. It will be a tough year but-----.

Harold Ticho: Let me just comment since this question was raised. Among the studies that were just completed last year was the student housing study and there is a plan to significantly augment graduate student housing in the very near future.

Norman Kroll: Walter.

Walter Kohn: You mentioned briefly physics graduate students. How about undergraduate students? What has been the trend in the last few years. What has been the impact of the school of engineering and how do you see that for the next two years. I mean Physics majors, undergraduate.

Norman Kroll: Well, there has not been a growth in the number of undergraduate physics majors. It has been fairly constant. I don't want to predict what is going to happen there. In terms of the general undergraduate teaching involvement in the Physics Department it has, of course, been increasing and fairly rapidly, and I think very much in response to the engineering programs.

Walter Kohn: How about the quality of the undergraduate physics majors? I am asking this partly because I see some problems in Santa Barbara.

Norman Kroll: I really don't think I can comment on that.

Tom O'Neil: Well, I have heard one comment from David Wong who is teaching the undergraduate course and that is that -- (its frightening) -- now you find students flunking out of engineering and going into physics. And that doesn't seem like such a good idea.

Tom Delmer: I would like to comment on that too because I am teaching over at the EECS Department. And they are grand, are just grand students. And they certainly would fit in very well with the class of graduate students I was with here--these undergraduates. So the engineering is strong, really strong.

Unidentified: I think there is a correlation. If we just increase our entrance requirements for the Physics majors we'd get the students. I get the feeling that a lot of good students are going into engineering because it is challenging. (side comment from audience) We should make it tougher.

Tom Delmer: No, I think they go into it because they realize it pays off.

Unidentified: It doesn't pay off though, not compared to Physics.

Tom Delmer: Perhaps it doesn't, but they believe it does.

Unidentified: I get the impression that they feel that it is better for them, a better curriculum for them because it is hard to get into.

Tom Delmer: They figure they make the costs they pay to go here and they want to get out and make that back.

Skid Masterson: Another feature is that a baccalaureate in the engineering field is very marketable, very marketable. You really, and it is perceived, have to spend the time to get a Ph.D. if you go into Physics, and that seems a lot longer way to go than in engineering. And in hiring college graduates you pay an engineer more.

Unidentified: What is tuition for an undergraduate going to be, as a parent of a 13 year old, going to UC in 1989?

Bill Frazer: I don't say this to the Legislature, but tuition is scandalously low. Of course, we don't call it tuition, but fees. But the fact is that it has been dropping over the past few years because the politicians have been unwilling to increase it to the level the traffic could certainly bear that I think would certainly help the state. But, in fact --it has been going down because it hasn't increased in absolute dollars and inflation is really lowering it. I have a daughter who is going to Hastings and I feel embarrassed paying so much. So I think from your point of view it is good. From the State's point of view, I don't think they are doing the right thing. But being politicians, they will continue doing it.

Greg Benford: From the point of view of the Professoriate, which is the smart thing, to raise the tuition or not?

Bill Frazer: Well, I think you could raise it quite a bit. It should at least keep up to the cost of living.

Greg Benford: How much would accrue to us?

Bill Frazer: There would be a little more--it is just a source of funds.

Greg Benford: But the legislature won't just soak it up?

Bill Frazer: Yeah, I don't want to get into that, but you are right. We are not going to make the move to raise the tuition except for cost of living adjustments. But if we raised it dramatically, as we thought of doing during the period of tight budgets, we raised it as much as the traffic would bear in the sense that the traffic there being the legislature. Because beyond some point they will just soak it up and say, thank you for helping us fund your budget.

Herb York: You know this question of whether it is going up or down depends on where you start from. Because those of us who went to school here 40 years ago remember that it was $27.50 times 2 for a year. And inflation is not the answer to that difference.

Bill Frazer: It is a lot less now than it was in 1971. That was the peak year.

Unidentified: The principle originally was there should not be any tuition at all.

Unidentified: I might point out, if I understand the fees correctly here, that in the State of Michigan, it is about a factor of two higher and it seems acceptable. There is a lot of room to go.

Gordon Knapp: As a parent of a student, particularly an out of state student, let me comment on the housing. The cost of living here as a student, because there is almost no student housing beyond the freshman year, is very high. And of course the out of state tuition is very high, so my son is paying at least as much as at most good, private universities.

Bill Frazer: Yes, my comment didn't apply to out of state tuition. That attempts to be calculated in such a way as to pay full cost. And in some sense it is. I meant the in-state fees.

Gordon Knapp: But, the cost of living is very high. Tuition may be very low even for the in-state, but the cost of living is very high and that also includes the undergraduates.

Jim Benford: It follows from the low turn over rate in the faculty that the average age must have been going up a year per year for the last decade or two and therefore the new people who come in you really want to emphasize your relative youth (middle age anyway). Therefore, it is my own experience in watching this kind of process at some other institutions, you want to go way over toward keeping the new faculty in the administration or the department; not only give them some responsibilities like teaching and things like that but some real authority. Otherwise you will develop a stratified age structure and a class structure will go along with it, and the older faculty will be very reluctant to turn the reins over to people for a long time, and that would be very frustrating and you won't get out of them what you want. You really want to watch that.

Norman Kroll: I am sure we will have lots of problems in finding out how to do these things, of which that's probably one, yes.

Herb York: Any more? If not, we will thank all our panelists and turn this (microphone) off.

Norman Kroll: Before I give this microphone up, I do want to thank the organizers of this conference for the wonderful job they have done, and thank all of you for coming. It really made it the success that it has been.

List of Participants

Maris A. Abolins
Gustaf Arrhenius
James Ball
Hans-Ulrich Bauer
Gregory Benford
James Benford
Herbert Bernstein
Dilip K. Bhadra
Frank (Bud) Bridges
Paul Brissenden
Keith A. Brueckner
E. Margaret Burbidge
Geoffrey R. Burbidge
Thompson Burnett
Laurence J. Campbell
Angelo F. Carlucci
Jeng-Wei Chen
Sidney A. Coon
Daniel Cox
Deborah J. Delmer
Thomas N. Delmer
W. James Durant Easton
George Feher
Peter J. Feibelman
R. Walker Fillius
Zachary Fisk
William R. Frazer
Kevin Fine
Donald R. Fredkin
Jobst Frohberg
Jose R. Fulco
John M. Goodkind
Michael P. Greene
Donald Griesinger
Suso Gygax
Poul Hjorth
Wendell Horton
Roger Isaacson
Allan S. (Bud) Jacobson
Gordon S. Knapp
Walter Kohn
Norman M. Kroll
Steven E. Lambert
Richard L. Lander
Roland Leadon
Laurence Littenberg
Douglas H. Lowndes, Jr.
Kazumi Maki
Michael M. Malley
M. Brian Maple
Kleber S. Masterson, Jr.
Kyoto Matsuda
James L. Matteson
Carl E. McIlwain
Nancy J. McLaughlin
Gregory Meisner
Shigeyuki Miyashita
Richard M. More
Richard L. Morse
Susarla S. Murty
David L. McKenzie
Roy Neynaber
Thomas M. O'Neil
Michael R. Pelling
Laurence E. Peterson
Oreste Piccioni
Philip M. Platzman
William A. Prothero, Jr.
Stephen D. Rearwin
Max Luming Ren
Benjamin M. Ricks
Amiram Ron
Marshall N. Rosenbluth
Christophe Rossel
Norman Rostoker
Andrei Ruckenstein
David B. Sailors
Sheldon Schultz
Joyce Sessa
Mickey R. Shanabarger
Gordon Shaw
T. P. Janice Shen
Arnold I. Sherwood
Dennis Shields
Victor Lee Stephen
William B. Thompson
Harold K. Ticho
Wayne Vernon
Edmund A. Vielhaber
George W. Webb
Dieter Wohlleben
Ming Wu
Philip M. Yager
Herbert York
Carl R. Zeisse

PHYSICS ON THE BEACH --SCAPE

Theo: This is an early work by a celebrated author who was one of the first physics students at La Jolla, and whose novel "Timescape" deals with those days. In fact, it's such an early work he may not remember it. But I ought to say one thing more about the author.

This is the first musical theater piece written by an extraterrestrial. It's obvious. The author appeared from nowhere, bringing his own clone with him, claiming to be from a fictitious place which he picked up from a musical comedy by Rodgers and Hammerstein, and writing books about time and space travel. This is a fantastic story, with resemblance only to persons living or not yet dead. Such things as described here simply do not happen in real life.

Since this is a story of time travel, it requires for our Virgil a narrator who is himself outside of time. Therefore our narrator will be [*taking off clothes to reveal only bathing suit and running around the stage*] the timeless sage, Theodorus Foster.

The story starts in the mists of time when the first creature of a new species slithered out of the slime-scape onto the beach-scape. There they were greeted warmly, with open arms, by the local fauna [*oceanographers appear in mask and flippers. A female embraces Theo*] and, er, flora. These were the first physicists in La Jolla, and they were soon invited to participate in the bizarre mating customs of the local residents.

Oceanographers: [*singing the tune from Pirates of Penzance*]

Hail men who plow the sea
Studying little fishes
Tasting the good dishes
We sit on th' beach all day
It's work for us--it looks like play.

Oceano: Actually we do have our mathematical models in oceanography. You see, if you sit on your surfboard and count the waves until nine-squared ....

Theo: Soon the first brilliant, never to be duplicated, class of physics students arrived. At first they were so few that no one noticed them. But finally notice had to be taken because--they had to be given a qualifying exam!

Listen and you will eavesdrop on the first faculty meeting.

Prof 1: What do you mean we have to give them a qualifying exam. Can't we just toss a few of them out. You know most of us come from Bell Labs or the Fermi Institute; we have no idea what a qualifying exam is.

Prof 2: I've never given a qualifying exam. Do we get to physically brutalize the students

Prof 3: No, we have to be more subtle. Do you remember last week there was a speaker, and Carl Eckart questioned him so intensely that he fell off the stage and was carried to the hospital? That's the way it should be done.

Prof 4: What the hell. Why don't we just pass them all and be done with it?

Prof 3: No, we can't do that. There's a proud tradition to uphold. You remember that Jack Steinberger failed his exams at Chicago.

Prof. 1: But none of the students is as good as Steinberger.

Prof 3: That's all right. We'll just have to pick the one who has the most promise. Abolins, maybe.

Prof 4: Yess. He will have to do.

Theo: Meanwhile the students were hysterically preparing for the unknown exam. Listen...

Stud 1: Can you explain how the prof in E&M derived the second Maxwell equation from the first one? It's in my notes.

Stud 2: What the helll is a k-meson. Piccioni thought it up and he's on my committee, so I'd better memorize the mass. But there are two figures given ....

Stud 3: Just take any one. He won't know the difference.

Stud 4: Why is everyone in this place talking about magic numbers. If I wanted to be an astrologer, I wouldn't have gone into physics.

Stud 2: Forget about that. We heard today that dispersion relations holds the answer to elementary particle theory. Why in five years this field will be solved, and there will nothing else to do.

Stud 3: No. Better to go into superconductivity. In Dr. Matthias's lab, they have a device: you put the sample in, if the needle goes left it's a superconductor, if it goes right its a ferromagnet, and if the sample slips away, it's a superfluid. They can produce a lot of PhD's with that.

Theo: As the department got bigger, it attracted other visitors. There's the chairman leading a distinguished visitor around.

Visit: Nice setup here. But what is that banging noise? It sounds like construction.

Chair: Oh, that's just Professor Brueckner banging Dave Rosenwald's head against the wall. It's good for both of them.

Visit: And those strange languages I hear from the next floor?

Chair: Oh, that's the high energy group. They communicate in only Italian and Yugoslav. But the students wouldn't understand them anyway.

Visit: And what are those figures I see in the misty distance? It looks like a giant duck followed in a straight line by giant ducklings.

Chair: Nothing too strange. That's Walter Kohn taking his students for a walk after lunch.

Visit: And where do your students come from?

Chair: Oh, from all over. New York, Berkeley, Minnesota. And some from India, gentle, soft-spoken Ghandian scholars.

Visit: But what was that! It sounded like a motorcycle gang.

Chair: That was one our Ghandian students. Perhaps he was referring to Indira.

Theo: But all was not just work. Listen to the *Cancion del Zorro Azul*, the Ballad of the Blue Fox.

Students [*singing to the tune of Las Mananitas*]:

> This is where we'd rather gather
> When we wanna
> go to Tijuana.
> It's a lunch club with a difference
> Viva la difference. Ole
> {etc.}

Stud: But that was not the only thing. We did some very interesting low temperature experiments on cooling by blackbody radiation into the night sky of the Anza-Borrego desert. We almost reached the theoretical limit, but we had to come back for class. And there were experiments on angular momentum in biokinetics; called by some the Twist. We did the Twist to the new Beatles music, and even to the news.

Bernstein [*twisting*]: Ekis E Te Ere A

Theo: And there was also politics. Ted Foster may have been the one to invent jogging before anyone knew the human body could be used that way, but Dick Morse actually invented an implausible political candidate.

Goldwater: Moderation in defense of extremism is a comsymp agitprop antichrist plot.

Student [*earnestly*]: Well, I'm for Johnson, actually, because I don't believe that the United States should send US troops to get involved in wars in Asia.

Theo: But finally the pride of La Jolla is the students, and the great advances in physics they have created. Let's look at the roster.

Chair [*rustling papers*]: Let's see. We have a computer system, a super spy, a congressional aide, a science fiction writer, a master potter, an admiral in the Navy, a management professor, a karate champ, an air pollution expert. Here's a professional kibitzer, who tells the third world how to do research so he doesn't have to do any himself. Doesn't any of them do research? Ah, here's one. But what the hell is transcendental reconstructed knowledge? Oh, here's another. "The application of the Schroedinger equation to human emotion?" Did we only get any physics out of the ones we flunked? I think there's a lesson for the future.