fermilab report

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THE COVER: *On May 2 and 3, 1986, Fermilab Users Executive Committee (UEC) Chairman Neville W. Reay convened the Fermilab Annual Users Meeting in Fermilab's Ramsey Auditorium. The meeting's agenda included a message to the users from Universities Research Association President Edward Knapp; reports on the Lab's accelerators, computers, experimental programs and schedules; talks on astrophysics, collider experiments, jet spectroscopy; and an update on the status of the SSC project. In addition, John McTague, then Acting Presidential Science Advisor, addressed the users in person, and Senator Pete V. Domenici (R.- N.M.) spoke via video tape.*

The centerpiece of the 1986 Users Meeting was a panel discussion on the topic, "How Do We Sustain Good Science?" The distinguished panelists were chosen to represent a broad spectrum of the sciences. This issue of Fermilab Report, *produced in conjunction with the UEC and the Fermilab Users Office, is devoted to the transcript of that discussion.*

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1986 Fermilab Annual Users Meeting Panel Discussion:
"How Do We Sustain Good Science?" 1

Notes and Announcements
Reorganizations... 27

Manuscripts, Notes, Lectures, and Colloquia
Prepared or Presented from May 19 to June 27, 1986 28

Dates to Remember inside back cover

1986 Fermilab Annual Users Meeting
Panel Discussion

"How Do We Sustain Good Science?"

Participants:

R. Stephen Berry
(James Franck Institute, University of Chicago)

James W. Cronin
(University of Chicago)

Riccardo Giacconi
(Director, Space Telescope Science Institute)

Bertrand I. Halperin
(Harvard University)

Daniel Kleppner
(Massachusetts Institute of Technology

Moderator: Edward Knapp
(President, Universities Research Association, Inc.)

Ramsey Auditorium
Fermi National Accelerator Laboratory
May 2, 1986

Preface

Everyone knows that we are in a phase of extreme fiscal constraints bordering on the irrational, if we judge the measures taken by the Congress (i.e., Gramm-Rudman-Hollings). In these circumstances of austerity, there is a tendency among scientists, as among all other groups containing human beings, to begin to fragment and to contest one against the other. There was a famous cartoon whose caption read: "There is no meat, we are fighting over the bones." Thus "small science" is pitted against "Big Science," chemists vs. physicists, basic research vs. not-so-basic, etc.

Before we decide which Beirut militia we want to join, we ought to think about the welfare of the country which, among some other things, also means the welfare of our Science Enterprise. Can one science, say HEP, advance with SSC while the rest of physics and colleagues in chemistry and geology starve? Or is there a fundamental unity in science *requiring* broad advance along a common front, physics dependent on mathematics and engineering, solid state physics needing electron accelerators, particle theorists leaning on condensed-matter techniques, etc? Much more, is the shared intellectual tradition, shared scientific heritage, and shared environment of the university, broadly defined, a strong unifying element? Finally, what is the best political strategy for preserving science in the forthcoming period of budgetary brutality?

The panel discussion which the Fermilab Users Executive Committee organized, is a step in trying to generate a conversation between the diverse scientific disciplines; I hope we do better than Beirut!

– *Leon M. Lederman*
Director, Fermilab

Edward Knapp (*President, Universities Research Association and panel moderator*): I'm going to take the opportunity and prerogative of the moderator to make a short statement about the problem myself, and set the stage for this discussion.

Today's budget crisis brings into sharp focus problems which have plagued science and science funding over the last 20 years. Today's panel discussion will bring five eminent scientists together to discuss their perspectives on how to obtain and sustain good science in times of budgetary stringency, and I hope that a consensus will emerge on a plan of action to help us carry through in the years ahead. The panelists are:

R. Stephen Berry conducts research in the areas of atomic and molecular processes, molecular structure, spectra, dynamics, thermodynamics, and the analysis of allocation of natural resources. He currently holds a professorship in the Department of Chemistry and the James Franck Institute at the University of Chicago. The most recent in a long list of awards and honors is the MacArthur Prize Fellowship for 1983.

James W. Cronin we all know. University Professor of Physics at the University of Chicago, he conducts research in the field of high-energy physics. He has carried out several experiments at Fermilab. In 1980 he shared the Nobel Prize for physics with Val Fitch for studies showing CP violation. He was chairman of the Division of Particles and Fields of the American Physical Society in 1985.

Daniel Kleppner is Lester Wolf Professor of Physics at the Massachusetts Institute of Technology. He specializes in atomic physics research with interests in high precision measurements and fundamental constants, highly excited atoms, and spin-polarized hydrogen. He is co-inventor of the hydrogen maser atomic clock. Among other awards and positions of note, he was chairman of the Division of Electron and Atomic Physics of the APS.

Riccardo Giacconi is the Director of the Space Telescope Science Institute and Professor of Physics and Astronomy at the Johns Hopkins University. He led the group of scientists who first made astronomical observations using the x-ray part of the spectrum. He has been directly involved in the conception and development of the UHURU and Einstein satellites. His most recent awards include the 1982 Gold Medal of the Royal Astronomical Society and the 1982 A. Cressey Morrison Award in Natural Sciences from the New York Academy of Sciences.

And finally, *Bertrand I. Halperin*, Professor of Physics at Harvard University, pursues his research activities in the field of condensed-matter physics. A member of the National Academy of Sciences and a Fellow of the American Physical Society, he is a past associate editor of *Reviews of Modern Physics*, and was the 1982 recipient of the APS Oliver E. Buckley Condensed Matter in Physics Prize.

Now, to set the stage for our discussion I would like to make a few comments myself. At a time when unity amongst the various sciences in presenting their views would seem to be essential for progress in making our case for public dollars, we see signs of disunity and [illegible]sing within the scientific community, seeming[illegible] self-interest or by mis-

Edward Knapp

understandings of the roles and missions of the various performers. A perennial set of misunderstandings between big science and little science (classifications which I don't believe even make sense) have surfaced again after being submerged for several years. The relative merit of basic versus applied science studies have also been discussed with the usual pressures to shift support to applied programs which have immediate application to industrial or defense needs. Another point of view pits the university system against government laboratories with implications that laboratory-based science might not be able to withstand a thorough peer examination.

In reality, there is a basic unity of science, and most of these conflicts are imaginary. This country needs big facilities *and* individual investigators. It needs great national laboratories *and* productive university departments. It needs basic science *and* engineering research. The battle should be to obtain a funding level to allow our creativity to blossom. We need the SSC, mass spectrometers for chemistry, new telescopes, synchrotron light sources, and sufficient funds to allow individual investigators to support the graduate students and post docs necessary for creative research.

How did we get in this mess? I believe we can demonstrate that the decline of support for university-based science started in 1966-68 with the budgetary pressures surrounding the Vietnam War and the concurrent launching of the Great Society. An examination of the NSF budget as a percentage of gross national product, which is truly representative of the support for basic university science, shows an almost 50 per cent decline between 1966-68 and 1979-1980 with a slight but significant rise in the period 1980-85, years of renewed interest in scientific research.

Well, you can say this decline has been absorbed and our science is still healthy - the best in the world. I would say our response to this decline has been almost the worst possible, almost totally irresponsible. In our rush to include the maximum number of performers in the system, we first eliminated federal support for building construction. Then we tried to reduce support for equipment within the grants awarded, and finally went to a system which spreads the funds broadly with substantive reductions from requested funding levels in many fields and with substantial uncovered costs to the host university system. We are left with a system which has university administrators vying with one another for federal pork-barrel projects to fund facilities without adequate peer review, a system with almost all university equipment at least antique, and principal investigators busy writing multiple grant applications for enough funds, now from multiple sources, to actually carry out the research they want to do and not having any time left to do the research itself. It is time to make this system healthy again.

I can think of no expenditure of public funds which has returned to the people anywhere near what the investment in science has returned. Now's the time not to be shy, to hide for fear of further cuts. Now's the time to insist that increasing the research budget would be a superb investment for this country. We could get back on the track we left in the mid-60s, get all the highly trained scientists working again with the ability to perform investigator-initiated research which has been the key to our productivity. As Leon has written, we should not be feeling guilty about the federal support we have received, we only wish we could contribute more to the health and vitality of our society. This is a time of opportunity, as John [McTague] said. Let's take it.

And now I think we should hear short statements from each of our panelists and then we can have a discussion on how we can get ourselves back on track.

R. Stephen Berry: I'm Steve Berry from the University of Chicago. I am a chemist, as Ed said.

I'd like to raise four points to try to address the question. I think it's a very challenging question. Did you think of it? Was it you who posed the question?

Edward Knapp: "How to Sustain..."? No, it was Leon.

Stephen Berry: Thank you, Leon. The four points that I'm going to raise are, I believe, necessary but not sufficient for the sustenance of good science. I wish I knew what was sufficient. These four are, first, one that we've all worried about for many years; that is, we must find mechanisms to maintain the continuity and damp the wild oscillations that are caused by fads and short-term market fluctuations in the support and continuance of

R. Stephen Berry

research and development. Second, I think one of the deepest problems we have to address is to return to a pattern of delegation of responsibility and of trust in the operation of the science and technology enterprise to replace the excess of accountability - the micro-management and suspicion that I think pervades the science/government relationship Ed Knapp was just talking about. Third, I think it's important for us as scientists to try to address the problem of the scientific illiteracy outside our own community. And this, I think, is something for which the initiative must come from us because the people outside won't do it. And, finally, there is one which I think sounds self-serving and maybe it would be, but that really isn't the reason I raise it: I think it's important for us to develop a means of support of research that looks more and more at the performance of individual scientists, rather than formal proposals and projects, as they establish track records. Let me just say a few words to flesh out each of these.

The first point: finding mechanisms to maintain continuity and damp out the oscillations. This is a problem which is serious in universities because of the problems that are associated with fads among scientists and in support agencies. It's a more serious problem, a significantly more serious problem, in government laboratories because of the short institutional memories that your supporting agencies have. But the most serious arena where this problem, I think, is devastating and is doing terrible harm to the United States, is in industry. Despite the

increased support over the past five years that John McTague discussed, what we're seeing now are very, very severe reductions, just in the last few months, in a number of major industrial laboratories, in industries that had five and ten years ago claimed to be establishing the new Bell Laboratories. What's happening is that these industries have done nothing to buffer the oscillations of their own markets, have done nothing to maintain and support the long-term investments that they have made in their R&D programs and, of course, have not taken into account the costs of re-establishing those research programs, or the costs of the lost momentum and the lost information that comes from disestablishing those laboratories. And I think of such places as Allied, Occidental Petroleum, and most recently, Exxon. And we all, of course, wonder what's going to happen to AT&T Bell.

The second point, the matter of delegation of authority, is not a new point. It was contained in a very explicit way in the Packard Report. The problem is that the relation we have is what it was at the beginnings of NSF, or before that between ONR and the basic research community. Support is now at a level at which, the research community agrees, many fine researchers are not being supported because they are below the cut-off levels of agencies such as NSF. Yet we are faced with a forthcoming challenge of a recycle of 10 per cent of the NSF research budget each year for the sake of recycling that budget because there is no longer the trust between the administrators of the support money and the research community. The administrators seem to believe that the fraction of the research community still being supported is not doing a good job. It's a very frightening thing and I hope that some of the other panelists are also willing to address this point.

I'm afraid that supporting projects as we're doing it now (going on to my last point) is inhibiting innovation. I'm watching some of the most creative senior researchers I know losing support because the traditionally basic-research-oriented DOD agencies have narrowed the definition of their missions. Consequently, the pool of fine scientists trying to get support is forced to a narrower and narrower circle of supporting agencies. Furthermore, even if support is increasing, it comes more and more frequently with tags that imperil the core programs of basic research.

I think I'll just briefly reiterate the third point that I made, namely that I believe, in agreement with the viewpoints that John McTague and Ed Knapp expressed, that we as a community have to face the problem of trying to educate our non-scientist, laymen friends, to be able to explain to them not only our own problems but the technological problems that we all have to face.

Bertrand I. Halperin: Well, I should say right away that I'm not an expert on public relations, I'm not an expert on politics or the functioning of Washington, and I'm not even really an expert

on the transmission of physics into technology or the selling of physics. And, I should say, when Neville Reay invited me to participate in this panel I tried to get out of it by explaining all of this. What he said was that he wanted me to present in a few minutes my own personal view of what the research opportunities and the funding priorities are for condensed-matter physics and just try to give you a little overview of that. In any case, I should say that the way to sell all of this is not at all clear to me, but I think we all face the same problems.

Bertrand I. Halperin

My view of condensed-matter physics would be best represented by visualizing the great number of issues of *Physical Review B* that come out each year and the huge quantity of published material they represent. It think that does emphasize at least one aspect of the field, namely that condensed-matter physics is really a tremendously broad subject, and that we as condensed-matter physicists study maybe hundreds of different phenomena and thousands of different materials. It's a field in which progress proceeds on many fronts simultaneously, hopefully with progress in many of them. We have to somehow convince the rest of the scientific community that it is not only important for technology, but is also intellectually very exciting. This despite the fact that it may be difficult to conceive of a single development that might revolutionize the entire field the way one might imagine such a thing happening in elementary particle physics, either a single new experimental discovery or a single new theory that might perhaps change 80 per cent of the field. It's very hard to imagine something like that happening in condensed-matter physics. Although there certainly have been, I must admit, a number of very dramatic recent developments which have certainly revolutionized *parts* of the field, for example the recent experimental discovery of icosahedral quasi-crystals which have long range orientational order with a tenfold symmetry axis and, perhaps, infinite range translational correlations. That was in 1984. Other examples in the recent past include the quantized Hall effect in 1980 and fractional quantized Hall effect in 1982, which certainly changed our way of thinking about portions

of the field. And one can also cite examples of instruments that have been invented. For example, the scanning/tunneling microscope, which was perfected about three or four years ago, which gives us a whole new way of looking at what's going on on surfaces, looking at individual atoms, and certainly presents tremendous opportunities for a new research. On the other hand, sometimes it's hard to convince students coming into the field, or to convince people who are supporting us, that this widespread attack really is the way to make progress.

Now, of course, you know that most research in condensed-matter physics is done in small groups. I would say a typical small group requires equipment that costs $300,000 to a few million to set up a laboratory, and operating costs of a couple of hundred thousand a year. I think most condensed-matter scientists would agree that our most urgent need is the support of small groups and that this support has fallen considerably - both the number of groups that have been supported and the adequacy of the support has fallen significantly over the past ten years. There is, however, even in condensed-matter physics a need for major facilities, at least what *we* call major facilities. For example, synchrotron light sources which cost on the order of $150 million, and just to give you an example of how one of our large facilities stacks up, the DORIS storage ring at DESY Lab, which we use as a light source for condensed-matter physics, is 24 meters in diameter. The outer ring, which is PETRA, is 300 meters in diameter. And I suppose if the SSC were superimposed on this scale you would not even see the curvature of the track. But, nonetheless, $150 million is a lot of money. We have to justify that, too. So, those are the problems as I see them as a condensed-matter physicist.

Riccardo Giacconi: I am Riccardo Giacconi. I am the Director of the Space Telescope Science Institute. My interest is in astronomy at all wavelengths, but particularly from space. Somebody said it's a great time to be a physicist; it's also a great time to be an astronomer. I think that it's fair to say that almost everything we know about the Universe we have learned in the last century. How big is the Universe? When did it start? The fact that it expands. How do stars work? How do galaxies form? How do stars form? How do planets form? How does life evolve? Many of these questions could not even have been asked as short a time as 50 years ago.

Today the joining of astrophysics, cosmology in particular, with elementary particle physics, is producing what I would consider one of the great achievements of the human mind. By utilizing laboratory physics we can interpret what happened billions of years ago, and on enormous scales. We are seeing Galileo's ideal realized - namely, that we can measure something in a laboratory here on Earth and find that the same laws rule the Universe.

Of course, we don't know that all the laws of physics hold at the very large scales of the cosmos. It could be that at these distances new laws would be necessary or at extremely high temperature, very high density, or very strong gravitational fields, some new physics may arise. Clearly it is a tremendous opportunity, a wonderful intellectual adventure. The potential for observational work that will impact and give progress in this

Riccardo Giacconi

area is very great. The technology and the people required for this task are at hand. There are plans, both in the space astronomy component, mainly supported by NASA, and the NSF component, which mainly supports ground base observations, to carry out this program. The program is characterized by an all wavelength approach, which you must realize by going into orbit, so that NASA, for instance, is planning observatories from the gamma ray range of wavelength through x-rays, optical and UV, to the infrared and possibly radio. On the ground there are studies or actual plans for construction of very long base radio interferometers and also for new technology optical telescopes of much greater size than what we currently have.

There are problems - and they're very serious problems - which now face us as the cumulative result of very serious errors in the management approach. Within the space program we're now grounded, as you know. The costs are ballooning out of sight and, therefore, no matter how much money is spent, we are really limited in the kind of programs that we can do. As a direct result of that, we have had no new start in Space Astronomy since 1981. The delay between conception and execution of a mission is becoming more of the order of 25 to 30 years, rather than the usual 5 to 10 years that it used to be. The effect is very serious because bright young people will not go into a field where they can't obtain data for a long, long time.

With respect to the ground base component of astronomy - the public investment in ground base astronomy is going down at the very time when much greater investment would be needed to keep

pace and respond to the opportunity offered by discoveries in space, and when, in fact, private investment is somewhat on the rise.

Now, what's the solution to all of these problems? With respect to the large problem, I have no particular wisdom. I hear that there is a problem with the fact that we don't want to have deficit spending and therefore we have to manage ourselves in a certain way. I don't know why that's necessary. I'm told that that's because we want to maintain a certain ratio between private and public funding. I wonder whether we understand economics enough to really make those judgements. I observe that although for many years past, we have been in a total Keynesian economy with the largest deficit ever accumulated, we seem to have reduced inflation. This seems to be contrary to economic models, so I don't understand any of it, nor do I pretend to. I also don't know how to influence politicians and increase the size of the budget. All that I believe I can contribute has to do with making the best possible use of the money that we are given. I think there are many things we can do to improve science productivity within constrained budgets.

One of the aspects of the problem has to do with the structures, the management approaches, which is something normally distasteful for scientists to worry about. But I think things have gone far enough that we have to worry about it, otherwise we must accept very severe limitations in what we can do. I believe the answer lies in giving more responsibility to scientists so that through their own work they can achieve some tangible results. There are questions of efficiency and questions of education and training, and there is a particular way in which efficiency and education are tied together. If you give responsibility to scientific groups as it used to be in the good old times, then you would tend to use graduate students to keep the cost down, which is a wonderful way of training graduate students - at least in plumbing, as Beppo Occhialini used to recommend to me. He suggested that in the study of cosmic rays I should go into cloud chambers rather than nuclear emulsions because, although there was more physics in emulsion work, there was more plumbing in cloud chambers, and that was useful in itself. I am concerned about the trend of putting together large observatories in which a few specialists put together complex instruments, and then there are a large number of users who never get involved in operating the facility. I think that's a terrible pedagogical approach. Maybe if we could be given the opportunity, with a wrench in our hand, to go back into the laboratory and build something and get our students involved in building something - that might be very good.

Finally, no matter how we improve the efficiency, I think we can't escape something which is very, very difficult - and which all advisory committees avoid like the plague - and that is, even within a subject like astrophysics, at a certain point we might well have to set up scientific priorities across discipline

boundaries. I think we can avoid doing that if we can maintain a reasonable health in the disciplines across the board. But when the budget dwindles below a certain point, then, I think, we would really threaten the health of the field if we were unable to make those judgements and set up those priorities. I'll stop here.

James W. Cronin: Jim Cronin, from the University of Chicago. I'd like to speak in a somewhat different way, maybe more naively. I think with respect to physics, I don't recall a time when I could see, in all the fields that I'm aware of, better opportunities. One just feels an imperative to do these things, and I think we have to really believe in the premise that the gain of new knowledge, whether it has long-term or short-term effects, is really a benefit to society. I think it has been amply proved if we go back in history. The work of Faraday clearly turned out to be rather practical, although it was not recognized so at the time.

James W. Cronin

And when I think we have to approach science with a better sense of unity, I think most of us have the intellectual capability to appreciate the important achievements of other fields of science, and I just don't really see any gain in trying to suggest that one field is worse than the other when we clearly can recognize the intellectual quality of other fields of science. For example, we had colloquia at the University of Chicago just this last year; I'm thinking about it. We heard a lovely discussion of using lasers to bring sodium atoms in an atomic beam completely to rest and put in a trap. What marvelous things! Quasi-crystals with a fivefold or tenfold symmetry, fractal growths, or if we look in space physics, the magnificent knowledge and pictures we gain of the planet Uranus. These are things that just give shivers to one. We are just so fortunate, the capabilities are so great, that we can't let these opportunities go. We have to have a unified approach. I feel very strongly that I could defend to my colleagues at this table the intellectual reason for building the SSC. I think the reasons

cannot be denied. I am perfectly convinced I could persuade them of this fact. I would be willing to let them make a judgment about the intellectual viability of it.

The question of funding the SSC is really a very serious problem. It is very natural to feel that a huge thing like the SSC is going to drive everything else out of business. I think Dr. McTague pointed it out, and I believe it myself, that there's no evidence that past large high-energy physics projects have had such an effect. I think that to maintain the scientific vitality in our particular field that most of us in this room have worked on, we simply have to build this machine. But I think we also, as high-energy physicists, have to spend a little bit of our time looking more broadly at all the other things that are going on. We must recognize, also, all of the links and cross currents from which we have benefited and those which other people have benefited from us.

Professor Giacconi has mentioned laboratory experiments related to astrophysics, but indeed we have learned some things from astrophysics which we feel are relevant to understanding particle physics. If these great strings are ever correct, it may be that some astrophysical observation will choose between the hundred million solutions that are apparently possible in that area. There are other things, like the notion of dynamical symmetry breaking. I think there is a big issue about whether that began in condensed matter, or began in particle physics. Clearly there is a great unity and a commonality of symmetry-breaking phenomena that span a broad range of disciplines. Just think what microelectronics has done to enable us to do experiments. If we go out and look in the Meson Lab and other places, we would be lost without such things. Metallurgy has made it possible to build very fine superconducting cable and improve it by nearly a factor of two in current-carrying capacity since the TEVATRON was built. Our contribution to condensed matter is the building of electron machines which were recognized finally to be a source of synchrotron light. Development of superconducting cable in turn has enabled proper magnets to be built for tomography.

So we are really participating in a great unity, and I'm sure that I could go and talk to our colleagues who are fearful of the SSC, first convince them of its intellectual vitality, and then discuss all of these matters with our funding agencies in a unified front. We who want to build the SSC are very concerned that the major developments that are going on in small physics and atomic physics - exciting, intellectually terrific - should all go together. If we don't do that we certainly are going to suffer, and that would be very sad.

Thank you.

Daniel Kleppner: I'm Dan Kleppner. I'm a survivor of the Brinkman Committee and an atomic physicist. I don't propose to

tell you about atomic physics today, because that's all documented in one of the *Physics Survey* volumes. I do urge you to read that volume. Give it to your friends and students, and don't forget that Mother's Day is coming.

What I would like to talk about is talking about physics. The day before yesterday, I heard Bob Wilson give the Presidential Address to the Physical Society. He talked about the general issues of physics in a most interesting and moving fashion. He discussed the unity of physics, and questioned whether it really existed. When one looks at the barrage of physical review volumes, the huge number of physics subfields, the enormous number of specialized scientific meetings, the gigantic APS divisional meetings (and the diminutive national meetings), it is not at all obvious that the idea of the unity of

Daniel Kleppner

physics is anything more than a token. In spite of all of these factors, however, Wilson did make the case that there is a unity to physics, that fundamentally physics is one world.

I was pleased by his analysis and his conclusion. They caused me to think about this one world of physics. I believe that it really is one world, but it seems to be a world of many different nations. The subfields are starting to view each other with the same sort of national chauvinism that can poison relations between nations. I think that it is essential for us to overcome this. We must try to understand each other's points of view, to understand each other's language and each other's problems.

A problem that Wilson points out is that with the impending success in grand unification, physics will be over. No one will have anything to do. I hadn't been concerned about that, but I can see that it really is a serious problem. I have a solution to offer, though: Particle physics may be over, but atomic

physics isn't. Among the nations of physics, I think of atomic physics as Switzerland - very small but with many attractions. You are always welcome.

However, to press on. I'm concerned about the nay-saying that John McTague talked about. There are vocal critics, people like Rustrum Roy, who seem so opposed to particle physics and all of what Roy calls basic physics, that it's impossible for me even to follow their reasoning. However, their arguments receive attention - they make good press - and I am concerned about this. It seems to me these people argue at the pitch of the true believer, at a level where debate isn't going to really help. However, there are thoughtful physicists whose opinion should be respected, who share many of these concerns. Arno Penzias has expressed his misgivings about the scientific resources that are being put into large physics. The chemistry survey, the Pimentel Report, which is a very interesting report, makes much the same case for science in the universities that's made in the Brinkman Report. However, it proceeds to justify increased funding in chemistry by the fact that the level of funding in physics is so much larger.

The actual level of support for physics in universities and chemistry in universities is roughly the same. Pointing to the large expenditures for the major machines of physics to justify increased support for chemistry is, in my opinion, a poor argument. Fortunately, there are lots of other good arguments in the Pimentel Report. But I think this illustrates the misgivings about the costs of large physics. As an atomic physicist viewing particle physics, it appears to me that to be a leader in particle physics you need extraordinary qualities; you must be something of a scientific general. Not only must you be a superb scientist, but you must be a master in tactics, strategy, and logistics. You must also be a politician. It seems incredible to me that one can do all these things. This is an heroic arena for physics. In contrast, the qualities it takes for success in atomic physics today in the United States are the qualities of a small shopkeeper. One must raise funds continually. One is perpetually trying to make the income match the outgo simply to keep the enterprise solvent. This begging position is a difficult situation to sustain, but even the best people in the field are often in it. Physics in the university is deteriorating as a result of this situation.

I belive that those of us who are in little physics should be supportive of big physics. When one visits a laboratory like Fermilab, it is difficult not to be overwhelmed by the grandeur of it. When one learns about the achievements of particle physics, feels its excitement, or simply looks at it as a model for international cooperation, it is just impossible - at least for me - not to be turned on.

Jim Cronin just gave the best arguments for the support of science that I have heard in a long time. When particle physicists discuss the needs for the SSC or other major facilities, you are often rather defensive about it. I think that this defensiveness sometimes makes you insensitive to the rest of us and science as a whole. Your case is much stronger if you talk collectively about science; the whole enterprise fundamentally works together. In my view, if particle physics does not move forward in this country, if the U.S. turns its back on particle physics, it will be the death of physics. We'll just withdraw from science. But similarly, if science in the universities deteriorates, that will equally be the death of science.

Particle physics is faced with a dramatic decision: at some point one must decide whether or not to go ahead with the SSC. I can see the importance of making that decision before too long. This is an urgent problem.

We, too, have an urgent problem, but it is much more insidious. In the areas of small science, we must worry about sustaining its quality. We are finding that it is increasingly difficult to fill university positions in these areas. That the universities cannot hire good scientists is unprecedented. The underlying reason is that the atmosphere for doing the research has become so poor that good people will no longer pursue university careers if there are any reasonable alternatives. I'm most concerned about this because the universities must fill these positions before too long. If the universities can't fill them with first-rate people, they will start to fill them with second-rate people. We're fighting mediocrity, and mediocrity is a dangerous enemy. That's our fight, but I think we've all got to fight our fight together.

So, the bottom line of what I'm trying to say is that we must support each other.

Edward Knapp: I suppose the best thing to do at this point is to have a discussion between the participants in this panel. Are there any questions that any of you would like to ask any of the others, or general questions, or general points of discussion, or questions that you'd like to raise?

Stephen Berry: Let me ask: Do you think that the mechanism of support is influencing the divisiveness that you're so concerned about? Do you think that mechanism is a problem, or do you think it's irrelevant? Do you think that the divisiveness is simply a defensive reaction of the scientists themselves or do you think that it has anything to do with the interaction between the scientists and the support community?

Daniel Kleppner: I think the divisiveness is basically a symptom of malnutrition. It's inevitable when you're in a difficult position to look around and try to see who may be better off, and to resent it when they are, or when you think they are.

Underlying this are problems in the structure of the support of science in this country. In a sense, particle physics is well off in that it has one agency which is responsible, and with which there is good communication. The DOE has a mandated responsibility for particle physics. In other areas like condensed-matter physics and atomic physics the support is spread through many different agencies. Formerly, the support for these fields came chiefly from the DOD. Science as we know it in this country, in the universities, was created by the DOD which abruptly withdrew and left us at the mercy of other agencies. Today the National Science Foundation has primary responsibility in these areas, but the NSF still assumes that the responsibility is shared by many different agencies. These mission agencies no longer accept their responsibilities. You particle physicists have the advantage of talking to essentially one agency, and it's an agency that listens to you. In a sense, that's enviable.

In another sense, though, we are at an advantage because the variety of agencies gives us great resources; we are not victims of unilateral decisions. The problem - and I don't think it's adequately recognized - is that the system for the support of basic science in this country, perhaps outside of DOE, is simply not working.

Edward Knapp: Professor Giacconi.

Riccardo Giacconi: Maybe I haven't been reading my *Physics Today* lately, but I am amazed that you all seem to be extremely concerned with the fact that there must have been something going wrong and you have been attacking each other. Our problem in astronomy is not that we attack each other; we're fairly in agreement. We have set up priorities. It seems to me that the most recent *Physics Survey*, which up to now I have only seen in abstracts, was an effort to present a unified approach and avoid interdiscipline troubles. But the one thing I was hoping was that while we attempt to work more harmoniously together, we do not prevent ourselves from looking directly at the possibility that there are, in fact, structural conditions making it difficult to support science properly and to do science efficiently.

Bertrand Halperin: I think there are a number of things that have struck me, sitting up here and listening to the various speakers. I think that as a university condensed-matter physicist, at least, I am very aware of the interrelations among the various fields of physics and I certainly, as I think some of the other speakers have remarked, have benefited enormously from hearing colloquia on high-energy physics, and on other topics ranging from chemistry to astrophysics. I think that emphasizing the interfertilization of ideas, as has been done here, is a very positive step.

Now, addressing myself to perhaps the more specific interests of this audience, how do the high-energy physicists sell the

SSC? I think the approach that has been taken here, of an ecumenical love-in, if you wish, is very fine. And I think it's correct that in selling it, it has to be a two-way street. I think one has to emphasize that, indeed, knowledge flows both ways, because obviously if you don't emphasize that, you put other people on the defensive, and obviously that's not what you want to do.

I, myself, am very sympathetic to this line of argument, but I think there are some real questions that obviously also have to be addressed, and I don't quite know what the answers to them are. One obviously is the enormous cost of the SSC. It really is a lot bigger than the numbers we're generally used to dealing with in the pure sciences. The issues about draining of scientific resources that Penzias, for example, has presented are real issues that must be addressed. Another concern is that, even if we are working in a state of constant chemical potential rather than constant numbers of dollars, and if the SSC is funded, and not at the expense of other science, which I think is a plausible possibility, one does worry about what happens three years down the line or five years down the line when there's a new administration and all of a sudden the chemical potential is lowered. What pressures develop then? I think that the worry that the government will respond in an irrational way to such pressures is a very real fear, and it is not completely without justification. Then having a very large consumer of dollars, that is, a big construction project that's already been begun, may cause particular problems. In other words, once you begin something like the SSC, it may be a sink of money that's a sink of a certain number of dollars not operating at a certain chemical potential and so, therefore, if you lower the chemical potential and have this sink, then the results could be disastrous for other branches of science.

These are the kinds of worries one hears in the halls of condensed-matter physics institutions.

Stephen Berry: I'd like to turn to this point of scientists standing together, seeing a unity to their subjects. It seems to me that during the past ten years there has been a kind of convergence of perspectives, a kind of growing ecumenism, if you like, in the degree to which scientists working in different fields, approaching problems with seemingly very different tools, are beginning to realize that indeed these tools are very, very similar. I think of things like the new topical group on fewbody problems in the American Physical Society, composed of a mix of atomic and molecular physicists and nuclear physicists who have deliberately decided to get together because they see common problems. It has been a bother, an annoyance, to many of us to have been aware of how frequently physicists from various fields and some of the physical chemists like myself who work on the border, have, in fact, been using very, very similar approaches and very similar concepts without talking effectively with each other. After all, there aren't many solvable problems in

science. For a long time we were not very good at capitalizing on these similarities or at trying to translate among the languages of particle physics, nuclear physics, and atomic physics. But phase transitions are phase transitions. Roughening transitions are roughening transitions, regardless of whether they're in condensed matter or strings. And I think we are beginning to learn to talk to each other in a way that we had not been 20 years ago.

James Cronin: I would like to just respond a little bit to Bert Halperin. Certainly, when you commence on such a big project as the SSC, there are these fears. But I would say two things: If we are so shy as not to begin it because of such fears, we certainly lose. The need to build this machine is just absolutely essential. I feel that one has to take the risks and try to cover them as best as possible by trying to establish a reasonable scientific policy.

Secondly, I am quite sure, either I or my colleagues could talk with your friends in the hall and at the very least convince them of the necessity, the intellectual necessity of this machine.

Edward Knapp: I think it would be a good time at this point to perhaps take questions from the floor for people on the panel.

Audience: What do you think are the proper criteria for the level of effort in science?

Edward Knapp: Would any of you like to comment on that?

Daniel Kleppner: Yes, for I've been puzzled about this question: Just how much should nations spend on science? I can give you a normalizing figure, or at least the name of a normalizing figure. Look at our total national R&D effort, at all the federal and industrial programs, too. This includes the DOD, energy, the environment, everything. I believe the figure for that is something over $110 billion. Look at the size of that enterprise, then ask: What's a reasonable level of support for basic science? If we hadn't been doing basic science 20 or 30 years ago, we wouldn't be engaging in these programs today. If we want to sustain such programs in the future, we've got to have an adequate level of basic science today. In this context, one can make a strong case that the overall level for support of basic science in the United States is too small to meet the needs of the future.

University-based research needs to be increased by roughly a factor of two. The federal government is spending about $400 million a year in physics departments in universities. That's the total federal investment in university-based physics today. Everyone who has looked at this problem has come to about the same conclusion: the support needs to be doubled. The condensed-matter panel came to this conclusion, the AMP panel did, the

Pimentel Report came to that same conclusion, too. Doubling of this support, within the overall picture of the national R&D figure, is a reasonable thing to do.

I think from the point of view of the overall R&D effort, the SSC can also be justified. The cost is not out of line. The argument for more support for basic science should be made in the context of today's national programs such as the manned space station or the hypersonic plane.

Edward Knapp: Professor Giacconi?

Riccardo Giacconi: I don't want to discuss figures, but I would like to try to put this in some historical context. One could ask what the ratio was of Stonehenge to the gross national product of England at the time it was built. Tycho Brahe got a ton of gold to start building Uranienborg, the great observatory where he first observed that comets were outside the atmosphere, and where he discovered supernovas. Since we are spending more on science fiction movies about space exploration than we are on planetary exploration, I feel safe. I figure we are taking too modest an approach. Without getting into a discussion of philosophy or religion, I believe that we are constructing cathedrals of the mind. They are the expression of a very deep desire of mankind not to live on bread alone, but to express a certain spirituality. We don't have to be that bashful about it.

James Cronin: Yes, absolutely.

Stephen Berry: If we ask not necessarily what criteria we can easily apply as a measure, as if we were trying to behave like a government agency, but turn to criteria like the kind Professor Giacconi was just describing, we might look at it a little more broadly and say that the total that the science and technology enterprise delivers to a society will inevitably contain a significant component of deeper understanding of the world around us, and it will also contain a slightly delayed component of changing our lives with goods and services. It's unthinkable now that our society could be technologically static. We have come to expect a rate of change, and perhaps we and other nations drive each other to accelerate that rate of change, but we could not think of living with a zero rate of change. In effect, our society has decided that the criterion for support of science and technology enterprise should be associated with what rate of change we would like.

Now, one problem that we face is, sometimes, that we have become accustomed to chugging along, working out our own contributions to the thought of man, and we run into bumps along that road coming from things that happen in society outside our scientific world. This brings me back to that point I made earlier about the need to smooth the means of support of science. That is, our society must learn that it is cheaper and healthier to sustain science on a regular, continuing basis,

consistent with the long time horizon that's associated with its sustenance and with its own natural time scale, than to face the constant fluctuations that we've been forced to live with recently. Perhaps a criterion of how reliably the support should be delivered may be more useful than a criterion based on what the absolute level should be.

Edward Knapp: Maybe I could add a comment, even though I'm just the moderator. This factor of two which Dan mentioned a couple of times, keeps coming up, and the question that can be asked is, "Is this the chicken or the egg?" Where does this factor of two come from? As I mentioned in my little introduction, I think one source of this factor of two is the discontinuity that occurred in the late '60s, and the fact that in our universities we have a scientific community which is sized for roughly double the budget that we are now providing it. This, I believe, has two solutions. I think it's obvious that one of them is to double the budget. The other one is to halve the number of scientists.

I think that, really, the latter is a totally impossible solution to the problem. It does everything wrong. I think it really leaves us with only one solution to the problem, and that is to somehow double the budget for the operation of the scientific community in our university system over the next few years. Halving the number of scientists would, among other things, absolutely and completely destroy the possibility of a scientific career for any young person in the United States today. It would give an absolute signal, give a direction to what we're doing, give a flavor to our country and its intellectual vitality which would destroy it. So, I think it's completely not within the realm of possibility.

I was in Geneva a couple of days ago, and a British fellow came up to me and he said, "Oh, but we can't invest in more science in Great Britain; we're a poor country." Well, there's a difference between being a poor country and being a country which is broke, and I think that maybe the United States is a country which is broke, but it's not poor. I think we've got to convince the people who provide the funds to us that we have to make our scientific communities healthy and able to provide to our society what they're capable of providing.

Stephen Berry: Of course we'll lose just exactly the wrong people, if that were to happen.

Edward Knapp: Of course! Yes.

Stephen Berry: Whether they don't go into science or go elsewhere. Those are the ones we need most.

Edward Knapp: Yes. Any comments?

Audience: How can you sustain good science in an atmosphere where, when you look right and left, you find your colleagues

accepting money that's destroying their initiative? For instance, the Strategic Defense Initiative. How can you sustain good science in such an atmosphere?

Edward Knapp: Would someone like to handle that?

Daniel Kleppner: Well, that's a provocative question, and a provocative question deserves a provocative answer. Some people are accusing physicists of being corrupted by accepting SDI money for research. Unfortunately, the accusation isn't correct. We're not being corrupted because the SDI is not putting money into basic research. I would love to have the opportunity to be corrupted just to see how I stand up to it. The fact is, the SDI, as far as I can tell, is actually taking money out of basic research. It is paying for engineering and all sorts of applied things, but as far as physics goes, and basic physics is what I'm concerned with, the net effect seems to be some withdrawal of funds from university-based research. So, I'd love to be able to respond to your question from first-hand experience, but I can't.

There is a great deal of misrepresentation about this point. The DOD claims that it is supporting basic research again. The university initiative is putting some money into basic research, but it is a very small amount. In one of the volumes of the *Physics Survey*, the volume on Applications and Interface, there is a marvelous graph which shows the whole story. It shows citations for support from the NSF and from the DOD in *Physical Review Letters* from around 1958 to the present. Through the '60s the number of DOD citations was much higher than for the NSF. The DOD was the major source of support for research in the universities. In the late '60s the lines on the graph crossed. The DOD has plummeted. Its support for basic research stayed down, and as far as I can see, it is still staying down.

So, there is my response to your question. Much as I would like to have the opportunity to deal with being corrupted, I haven't had that pleasure yet.

Stephen Berry: Let me speak a little to that myself.

First of all, Dan, let me say I quite agree that basic research overall is suffering considerably by the change in pattern of funding within DOD, partly because of the money that falls in the category 6.3 under SDI, and partly because of the redefinition of what is basic research, that is, 6.1. Things that you and I would call very applied research now fall into the 6.1 category and are, of course, much more mission oriented than things that were called basic research. Let me give you one example. I think now the DOD would not fund the basic research on rare gas excimers which it used to fund, and which led, of course, directly to rare gas halide excimers and then to rare gas

halide lasers. But in its recent decisions, DOD has specifically said it will not fund basic research in the general area of photochemistry because it is not sufficiently mission oriented.

With regard specifically to the SDI research, I have no familiarity with the classified part, but I have been on a review panel that's begun reviewing some of the unclassified innovative science and technology work. Some of that that I have seen is research that I personally believe is very good research which the federal government should be supporting under some program, somewhere. And it happens to have been put into SDI. An example is work on optical computing. But some of the work that I have seen under that innovative science and technology program I didn't like at all. And it's considered basic research by the program, and I think it's an embarrassment to the basic research community to have it there. And I hope that review panels like the one that I sit on can help, at least a little bit, to shape the program so that it at least will be supporting work that is intellectually sound and of broad general interest.

Audience: Dr. Kleppner made the point that it was beginning to be difficult to fill university positions. Is the problem that of not wanting to face the difficulties with funding, or is it a problem with supply?

Daniel Kleppner: I don't believe it's a problem of supply. Let me tell you where this fact comes from. I made an informal survey last June of about 30 physics departments, asking a rather neutral question: How is the supply of candidates for the positions that are open? I got about 20 responses. Some said the supply was adequate, though I think only one said the supply was abundant. Out of the 20, however, 12 pointed out severe problems. Often they wrote about their problems in detail. The one overwhelming fact which turned up was that physics departments today cannot fill positions in condensed-matter experiment. The universities can no longer compete with Bell Laboratories. At Bell Laboratories a young scientist can start right into research. In a university it may take several years to get support, and one faces a career of struggling with inadequate support. In the past the best students would be divided between a place like Bell (though there are not many places like Bell) and the best universities. The universities can no longer compete.

The Physical Society, through POPA, is now doing a more thorough study of who has this problem. If you ask anyone in a university who has tried to fill a position in condensed-matter experiment you'll find that it is close to impossible. It is not because there aren't good people there. It is because the conditions for research in the university are so poor, and the opportunities in industry are so good, that the best people will go to industry. This is also true in AMO physics. In other areas where one does not have industrial opportunities, students are still applying to universities because that's the only place

there is. Nevertheless, the basic facts are still the same. One can't do research in the university at a competitive level for lack of support.

Edward Knapp: Professor Giacconi, did you want to comment?

Riccardo Giacconi: I think this effect exists with respect to the phenomenon of availability of people. You can't find x-ray astronomers because they're involved in diagnostic analysis of fission at Livermore, and so forth. I think there is an indirect influence of both SDI and a lot of money being poured into classified research. In my institution we have no contracts which are provided by SDI. On the other hand, it is getting harder and harder to get a contract from NSF, and people who can get research support from DOD under the condition that their work be unclassified do it that way. I personally don't like it, but I can't blame them for doing it. I think that the very large expenditures in the defense area are attracting a big percentage of our pool of scientific and engineering talent, and are pushing costs way up and efficiency way down. So we are finding an indirect ill effect on the high-tech things we're trying to do.

Audience: Dr. McTague told us that we really had to make more of an effort to communicate with the congressmen about the purpose of science. He also gave us the advice that, in our past attempts to do this we've often been our own worst enemies. I guess the concern I have is how we'll have some assurance that we'll avoid doing the same thing in the future. I think scientists are human animals and when we're under enough pressure we sometimes start to lose our high scientific principles. Maybe some of the panelists have some advice on this matter.

Edward Knapp: I think I listened fairly carefully to what [John McTague] said. I don't think he said he wanted us to communicate with our congressmen. I think he said we had to communicate with the public, and there's a difference. I also think he told us that, in many ways, communicating with Congress was not particularly effective in that they could say "no" but they could never say "yes". By that I mean that they don't start things, they only stop them. At that particular point he, unfortunately, did not leave us with a plan of action. I was hoping we might get a plan of action out of this panel. I don't think we've discussed this point, however. Who do we talk to and how do we talk about science and the way that it can help us as a nation? I don't know. Does anyone want to comment on that?

James Cronin: I don't have that experience but, it would seem to me, of course we have to communicate to the public. I think a lot of effort is made, and in some fields, perhaps astronomy, more successfully than others. But I find it hard to believe that going to the public is the most effective way. It seems to me inevitably one has to deal more directly with the body that's going to make the decisions. I don't know whether it's possible to communicate to them the kinds of conclusions that come in

these broad studies like the Brinkman Report and other things like that, which do carry a level of unanimity in them and do recommend SSC's, and they recommend synchrotron light sources, neutron spalation sources, I suppose, and all of these other things. And I think one simply does have to go to the Congress. I just don't see how not to do that.

Edward Knapp: I couldn't suggest that you wouldn't go to the Congress, but I think that the appeal and the program has to be much broader than that.

James Cronin: Yes, but I think there are vehicles. These studies are not throwing stones, one at the other. They are very positive and they emphasize the unique and important features of a number of different fields. And it seems to me that this factor is the kind of message that should get through. I can imagine there are vehicles, maybe through the American Physical Society or perhaps the members of the Brinkman Panel themselves, trying to make an effort along that line.

Stephen Berry: First, I think this point that John McTague made about Congress saying "no" and not "yes" in a way is a recent phenomenon and perhaps a fluctuation. It used to be that it was the authorization committees that carried the true influence and the appropriations committees essentially seconded what the authorization committees did. And that role has been reversed. Now it's really the nay-saying of the appropriations committees that has the power. But I don't think we necessarily have to expect to live with that indefinitely.

Secondly, I think that we should remember that the congressmen we deal with are the elected representatives, the chosen representatives of the public, and therefore are the appropriate people for us to talk to. But just because we talk to them is no reason why we shouldn't continue to try to influence in a broader way the viewpoint of the public toward science. One of the healthy symptoms of recent years that I would like to point out right now is the degree to which physicists and astronomers, particularly, have been writing for the public at a level that laymen can read and in very, very skillful ways. I think this kind of thing is a remarkable phenomenon; that physicists and astronomers of distinction are willing and even eager to write such books is a very positive sign for how we may look to communicate to the public. I wish more chemists would do it.

Audience: People have been discussing the criteria by which we should establish the appropriate level of government funding of science. When I went to college, Keynesian economics and government spending were in their ascendancy, and today's deficit was in its ascendancy, but it seems to me that the general principals really still apply. The reason that government spending was said to be a solution to problems back in the 1940s was because you spent money on things which generated income goods which in their turn generated enough income to the federal government so that

the spending paid for the deficit it generated, assuming deficit spending. I think the problem we have encountered recently is spending money on things that didn't generate that income.

What we have to do in science is to argue that, in fact, as John McTague I think it was, said before, the investments that we make in science are not government spending, they are an investment, and they have created an income for society that far out-balances the investments that are made. It's very hard to make that argument in regards to the future, but it seems to me we just have to keep grinding away at that argument and show that it is an investment and not an expenditure and that more money, more goods, a better standard of living will be forthcoming. We may want to do science because of its intellectual interests for us, but I think society has to be persuaded that it will, in fact, not be at outlandish cost, that in fact the income will be greater than the outlay.

Daniel Kleppner: I agree with the argument. The difficulty in addressing Congress is this question of urgency. Why do it now if it can wait a year? I think there's a real question as to how urgent the need is. In my own mind, the need is very urgent. I don't know how long you will have to get SSC launched before the project just wilts away from lack of enthusiasm. That's a real worry, but the concerns that I have about the university positions are also urgent. The official AIP predictions are that there may be a shortage in the next decade. However, I see shortages starting to develop right now. Things can turn around very, very rapidly. Once the universities start to go down, they will be in a sharply descending spiral. Poor faculty don't attract good students. The number and quality of the students decrease, the faculty get worse...You can see the scenario. The need for moving ahead with science right now is a very urgent one. Somehow we must be able to document and communicate the urgency.

Bertrand Halperin: I'd just like to make two comments. One is that I'm afraid I'm rather pessimistic about the possibility of really reaching the public, any significant fraction of the public, with science articles written by scientists. I think the audience is rather small. I mean, *Scientific American* is failing. I think that the books on *astrology* may have big audiences, but the most that scientists can reach this way is a very few per cent of the public. That would be wonderful, but it's, I think, probably not in itself enough. I think one probably has to sell science on its utility to society. Perhaps sell it on the grounds that the cathedral we're building is essential for the health of society, but not...I don't think you're going to get lots of people out there writing their congressman saying they want us to discover the next particle or what-have-you.

The second point is an idea that might be worth thinking about a little bit that I've actually heard in private discussions here, but which I don't think was mentioned in the

panel discussion. I don't know how persuasive this is as a way of selling science, but at least one reason for doing pure science is that it really does make demands on technology which force technology to keep up, and give people who develop materials, for example, an incentive to develop materials to meet criteria of purity, etc. The large-scale production of superconducting wires for the accelerators was an example that was specifically mentioned here. There are many examples in solid-state physics, such as the incentive to produce semiconductor layers of particular perfection so you could see the quantized Hall effect. These goals may be more accessible, more noble, more inspiring, perhaps, to some of the people who develop new materials than the possibility that *maybe* there will be millions of dollars in it 15 years in the future. If the companies that do the manufacturing are not themselves seeing profits in the 2-year time frame, I would say, they are not interested. And I think that it would be perhaps nice to try to document the extent to which science has forced technology to move ahead. Obviously, for better or for worse, the military has played a very big role in forcing technology to move ahead, but I would like to think that science is another good way of doing that. Of course it works both ways. Science has benefited tremendously from technology, but it is a symbiotic relationship. And when science benefits, it helps technology to march on, too.

Edward Knapp: I think we're basically at the end of our discussion. I think this could go on all night.

Neville Reay: I think that we're ended here. I thank the panel for this wonderful discussion. I've learned a lot today and I've really enjoyed this personally.

NOTES AND ANNOUNCEMENTS

Reorganizations...

Effective May 1, 1986, Roger Dixon, formerly Deputy Head of Fermilab's Research Division, was named Head of Program Planning at Fermilab, replacing Taiji Yamanouchi, who has joined the Collider Detector at Fermilab (CDF).

Ken Stanfield, Head of the Research Division, appointed Peter Garbincius as the new Deputy Head, and Ray Stefanski to the post of Associate Head of the Research Division. Stefanski was also named Head of the new Research Facilities Department, with Stephen Pordes appointed Deputy Head. The Facilities Department contains almost all of the Research Division's fixed-target physicists, and is responsible for coordinating the installation, execution, and removal of experiments and beamlines in the fixed-target experimental areas.

As part of the reorganization, Jeff Spalding was appointed Head of the Site Operations Department, with Greg Bock as Deputy Head. This department is responsible for overall site support and operation of the fixed-target experimental areas at Fermilab. During the next fixed-target running period, the Operations Group of the Site Operations Department will operate 16 beamlines and also provide operations support for the upcoming Collider Detector at Fermilab run later this year.

MANUSCRIPTS, NOTES, LECTURES, AND COLLOQUIA PREPARED OR PRESENTED FROM MAY 19, 1986, TO JUNE 27, 1986

Copies of preprints with Fermilab publication numbers can be obtained from the Fermilab Technical Publications Office or Theoretical Physics Department, 3rd floor east, Wilson Hall. Copies of some articles listed are on the reference shelf in the Fermilab Library.

Experimental Physics

S. Ahn et al. Experiment #557	Valence Quark Effects in Beam Remnants in High E_t Proton-Proton Collisions at $\sqrt{s} = 27.4$ GeV (FERMILAB-Conf-86/74-E; submitted to Physics Letters)
A. Mukherjee Experiment #594	Azimuthal Energy Flow in Deep Inelastic Neutrino Scattering (Ph.D. Thesis, Massachusetts Institute of Technology, March 1986)

Theoretical Physics

J.C. Collins and W.-K. Tung	Calculating Heavy Quark Distributions (FERMILAB-Pub-86/39-T; submitted to Nucl. Phys. B.)
L. D. McLerran and J.P. Blaizot	Jets in Expanding Quark-Gluon Plasmas (FERMILAB-Pub-86/56-T; submitted to Phys. Rev.)

Theoretical Astrophysics

E. W. Kolb et al.	Yet Another Possible Explanation of the Solar-Neutrino Puzzle (FERMILAB-Pub-86/69-A; submitted to Phys. Lett. B.)
B. S. Meyer and D. N. Schramm	General Constraints on the Age and Chemical Evolution of the Galaxy (FERMILAB-Pub-86/71-A; submitted to Astrophys. J.)
F. S. Accetta and M. Gleiser	Thermodynamics of Higher Dimensional Black Holes (FERMILAB-Pub-86/76-A; submitted to Nucl. Phys.)

E. W. Kolb et al.	The Effect of Interacting Particles on Primordial Nucleosynthesis (FERMILAB-Pub-86/77-A; submitted to Phys. Rev. D.)
N. Vittorio and M. S. Turner	The Large-Scale Peculiar Velocity Field in Flat Models of the Universe (FERMILAB-Pub-86/79-A; submitted to Astrophys. J.)
R. Mayle et al.	Neutrinos from Gravitational Collapse (FERMILAB-Pub-86/81-A; submitted to Astrophys. J.)
J. C. Charlton and M. S. Turner	Kinematic Tests of Exotic Flat Cosmological Models (FERMILAB-Pub-86/83-A; submitted to Astrophys. J.)

General

J.A. Carson et al.	A Device for Precision Dimensional Measurement of Superconducting Cable (submitted to the ICFA Workshop on Superconducting Magnets and Cryogenics, Brookhaven National Laboratory, Upton, New York, May 12-16, 1986)
G. Chartrand	Computer Networking at Fermilab (FERMILAB-Conf-86/78; presented at the Data Systems Users Working Group Meeting of the Space Physics Analysis Network [SPAN], Goddard Space Flight Center, Greenbelt, Maryland, May 8-9, 1986)
D. Edwards	Status of the SSC Design (Submitted to the Proceedings of the XXIst Recontre de Moriond; Perspectives in Electroweak Interactions and Unified Theories, Moriond, France, March 9-16, 1986)
P. M. Mantsch	Accelerator Magnetic Measurements (Summary) (Submitted to the ICFA Workshop on Superconducting Magnets and Cryogenics, Brookhaven National Laboratory, Upton, New York, May 12-16, 1986)

R.C. Niemann et al.	SSC Dipole Long Magnet Model Cryostat Design and Initial Production Experience (TM-1404 [SSC-N-190]; presented at the ICFA Workshop on Superconducting Magnets and Cryogenics, Brookhaven National Laboratory, Upton, New York, May 12-16, 1986)
T.H. Nicol et al.	A Suspension System for Superconducting Super Collider Magnets (Presented at the 11th International Cryogenic Engineering Conference, (West) Berlin, Germany, April 22-25, 1986)
P.S. Martin	Design and Operation of the Quench Protection System for the Fermilab Tevatron (TM-1398; submitted to the 1984 U.S. Summer School on High-Energy Accelerators, Fermilab, Batavia, Illinois, August 13-24, 1984)
A.D. McInturff et al.	High Gradient Accelerator Quadrupole Design (Submitted to the ICFA Workshop on Superconducting Magnets and Cryogenics, Brookhaven National Laboratory, Upton, New York, May 12-16, 1986)
T.M. Miller	The Effect of Pulse-Modulated Thermal Radiation on the Time-Intensity Relationship for Dermal Pain (Submitted to American Industrial Hygiene Association Journal)
T.M. Miller	Results of Microwave Oven Radiation Leakage Surveys at Fermilab (Submitted to American Industrial Hygiene Association Journal)
J. Peoples, Jr.	Prospects for $\bar{p}p$ Experiments in the TeV I Accumulator (Submitted to the AIP Proceedings of the Workshop on the Design of a Low Energy Antimatter Facility in the USA, University of Wisconsin-Madison, October 3-5, 1985)

Physics Notes

K.-Y. Ng	Microwave Instability Criterion for Overlapped Bunches (FN-432; presented at the Second Conference on the Intersections Between Particle and Nuclear Physics, Lake Louise, Canada, May 26-31, 1986)

Colloquia, Lectures, and Seminars

M. Gormley et al.	"Status Report on a Proposal to Increase the $\bar{p}$ Source Accumulation Rate to 4×10^{11}/hr" (Fermilab, May 20, 1986)
F. Mills	"TAC Superferric SSC Magnet Re-evaluation" (Fermilab, May 20, 1986)
G. Fisk	"ICFA Magnet Meeting at Brookhaven National Laboratory" (Fermilab, May 20, 1986)
G. Dugan	" γ_T Jump in the Debuncher" (Fermilab, May 22, 1986)
C. Ankenbrandt and W. Merz	" γ_T Jump in the Booster" (Fermilab, May 22, 1986)
J. Grimson	"Discussion of Draft SD-41 Vacuum Vessel Standard" (Fermilab, May 22, 1986)
C. Moore and L. Ketchum	"Report on CERN Accelerator School on Geodesy for Particle Accelerators" (Fermilab, May 27, 1986)
J. Crisp	"Longitudinal Emittance Growth in the Booster" (Fermilab, May 29, 1986)
S. Holmes	"Transverse Emittance Growth in the Booster" (Fermilab, May 29, 1986
R. Carrigan, Jr.	"Fermilab Industrial Affiliates Sixth Annual Meeting" (Fermilab, May 29-30, 1986)

H. Fenker	"Charm Cross-Section and Charged Multiplicities in pp Collisions at $\sqrt{S}$=38.8 GeV (E-743)" (Fermilab, May 30, 1986)
G. Dugan	"Schedule to Complete the Antiproton Source and the Plan to Resume Commissioning (Fermilab, June 12, 1986)
L. Bartoszek and G. Lee	"Report on Trip to Calorstat, CERN.....VAT" (Fermilab, June 12, 1986)
C. Ankenbrandt et al.	"Space Charge in the Booster - Proposals for Improvement - Linac Upgrade" (June 14, 1986)
F. Cole	"Summer Student Lecture Series for College Students - Lecture #2" (Fermilab, June 17, 1986)
R. Orr	"Accelerator Division Information Meeting" (Fermilab, June 17, 1986)
J. Marriner	"Requirement for Dampers in the $\bar{p}$ Source" (Fermilab, June 19, 1986)
J. Petter	"Status of the Dampers and Their Expected Performance" (Fermilab, June 19, 1986
L. Bartoszek	"Twisting Bellows and Rolling Behemoths in the Accumulator: Orbit Control at Your Console" (Fermilab, June 26, 1986)