ROUND TABLE ON INDUSTRIAL PARTICIPATION IN LARGE-SCALE SCIENCE PROJECTS

Chairman, Dr. Richard Lundy

The Participants

Dr. Ray Beuligman is Program Director of Energy Systems, Convair/General Dynamics. Convair has fabricated many magnets for fusion and isotope separation.

Dr. C. H. Dustmann is from Brown-Boveri. He is now working on superconducting magnets for the HERA colliding-beam system in Germany.

Dr. John Hulm is Acting General Manager, Research and Development Center, Westinghouse. He is one of the developers of modern superconducting wire and a member of the Board of Overseers for the SSC.

Mr. Dick Rhodenizer is Manager of Systems and Products Engineering, Medical Systems, General Electric.

Mr. Carl Rosner is Chairman and Chief Executive Officer, Intermagnetics General. Intermagnetics General was the primary supplier for the Doubler superconducting wire.

Mr. Ryusei Saito is Chief Engineer, Nuclear Fusion, Hitachi. He has been involved in construction of magnets ranging from the KEK accelerator to levitated trains.

Dr. Ed Temple is Head of the U. S. Department of Energy Working Group on the SSC.

ROUND TABLE ON INDUSTRIAL PARTICIPATION IN LARGE-SCALE SCIENCE PROJECTS

Dick Lundy (Fermilab):

Today I hope to elicit from each of the panel members and some of the audience a response to a hard hypothetical question. Let's define an index that runs from one through ten in industrial participation with one being minimal and ten being maximal. The question for each member is: "What do you think the optimum number is on philosophical grounds and what do you think the realistic number is?" Realism has to be taken into account because we have real laboratories, real industries, and real times. I'll also be very interested in examples from the past of cooperation, of participation, and how it worked--a hindsight view of the good and the bad of industrial cooperation.

Let me set the extremes of this participation scale with an imaginary example. On a scale of one, a laboratory would decide to build an SSC and would hardly let anyone know. They would make numerous trips to the hardware store, buy lots of nuts, bolts, bar stock, and steel plate, work furiously night and day on the site and assemble the SSC themselves. They would install the accelerator and pray that it would work. That is the "one" end of the scale, the low end with total laboratory commitment and no real industry involvement except as a basic supplier of materials. The Energy Doubler, probably rates at the two or three level in part due to the high risk nature of the endeavor when it started five years ago. We bought basic commodities, such as steel plate, and we bought the next level up, fabricated subassemblies. We did a lot of drilling and burning and welding and praying here on the site. The other extreme, ten, can be illustrated facetiously with one side of an imaginary conversation: "Yes, this is Big Corporation, incorporated. Glad to be talking to you. My name is Newhart; I'm a sales engineer here. You want 40 TeV in the center of mass, with a luminosity of $10^{33}/cm^2$ sec. Well, we've been selling a lot of those this spring. I'll have to check stock... Yes, you're lucky. We've got two in stock. We've got one with experiments and one without.... Yes, they both have twenty-year warranties--no problem with that. Now, most of our customers take the one with experiments and they get about a Nobel Prize per year with four experimental areas.... You'll take that one? That's fine. Yes, we'll deliver it and set it up next Friday. Only one question, now, will that be cash or on your credit card?" That would be cooperation.

Now I should ask Ed Temple to make some opening remarks. Maybe Ed will tell us what the right numerical index is and whether it's going to be cash or credit. He's in a unique position--he represents the sponsor. Assuming there is an SSC, the participation will have to be played under the ground rules that the sponsor, the Department of Energy, operates under. These are the federal procurement regulations. There are good and bad features of these regulations. We've got to maximize the good and minimize the bad. That may be one of the major problems for the SSC builders.

Ed Temple (Department of Energy):

The first thing I want to discuss is a little bit about the SSC organization. Figure 1 shows the SSC reference designs study organization. Here I want to give full credit to the lab directors and the Reference Design Study Group who have produced the foundation upon which the Department of Energy (DOE) can go forward and upon which this kind of meeting can be held with some real serious paper studies for reference. These paper studies will be available to the world at large sometime in June.

Within the Department of Energy, the Secretary is Paul Hodel and the Director of the Office of Energy Research is Alvin Trivelpiece, so those are two principals in these discussions. The Chicago Operations Office of DOE will be an important contract administration arm for the Department for this effort

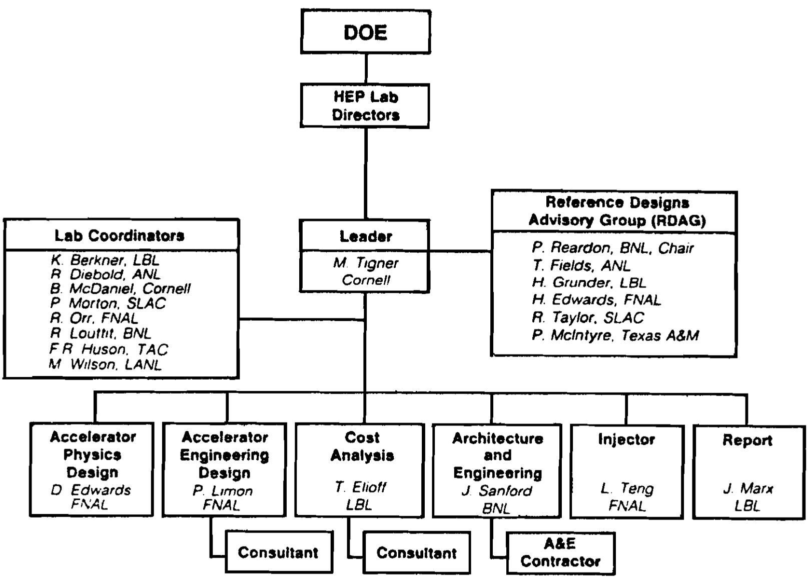

Fig. 1. SSC reference design organization.

for the interim period. In the Office of Energy Research (ER) under Trivelpiece, there is the Office of High Energy Nuclear Physics headed by Jim Kane and the Division of High Energy Physics headed by Bill Wallenmeyer. I head the Division of Construction Management Support in the Office of Energy Research. In that role, I provide office-wide oversight of all projects in ER for the Director or the Deputy Director and then provide construction management support to the various program divisions, high energy and nuclear physics, fusion, and basic energy sciences.

Since this is a round table on industrial participation in large science projects, it may be propitious that I have the opportunity to be here. In the Office of Energy Research, we actually have some semi-large scale science projects right now. In high-energy physics, for example, the Saver has just been completed, TeV I and TeV II are under construction, and the SLAC linear collider is getting underway at SLAC. ISABELLE is just being terminated, and the SSC, we hope, is getting kicked off. The Continuous Electron Beam Accelerator Facility (CEBAF) is a brand new project that the Southeastern Universities Research Association will be building for us in Newport News, Virginia. In fusion, TFTR has recently been completed. I believe that TFTR and the Energy Saver are fantastic successes in big science projects so we do have a record of success to be building on right now. The fusion program has studies for a Tokamak fusion core experiment similar to those that we have going for the SSC. This project is similar in magnitude to the SSC and thought to be in the one to two billion dollar class.

Altogether there are 25 projects in the "above 20 million dollar" category in the Office of Energy Research. Clearly one can't hit a group like this and review all of these, so I'll limit myself to three--the SSC working group, CEBAF, and the TFCX. In your packets, you have a copy of the talk, "SSC: The Next Big Step", that George Keyworth, the President's Science Advisor, gave here at the Users Meeting a few weeks ago. Keyworth noted that the SSC can and should be justified as a means to achieve excellence.

Quoting from the speech,

> To you in the physics community SSC represents something very specific--an experimental tool for probing the structure of matter at very high energies. But SSC can and should also represent something more fundamental. It should be concrete evidence of our recognition of the value of new knowledge. It should be a statement to our youth--the ones we'll depend on to maintain our scientific leadership in the future--that as a nation we value creativity, not just in physics but in all areas of science. And it should be evidence to ourselves and the rest of the world of our commitment to excellence in what we choose to do.

I think it's appropriate that Keyworth talks to the idea of excellence because I think that there's a record of excellence in this field and there's a good record to begin a big project like this on.

Figure 2 shows the staging for the project. These include a phase 0, which we're in right now, a phase 1, where we'll initiate R&D, a design, and complete a site selection process, and phase 2 where we do construction. Operation begins in phase 3.

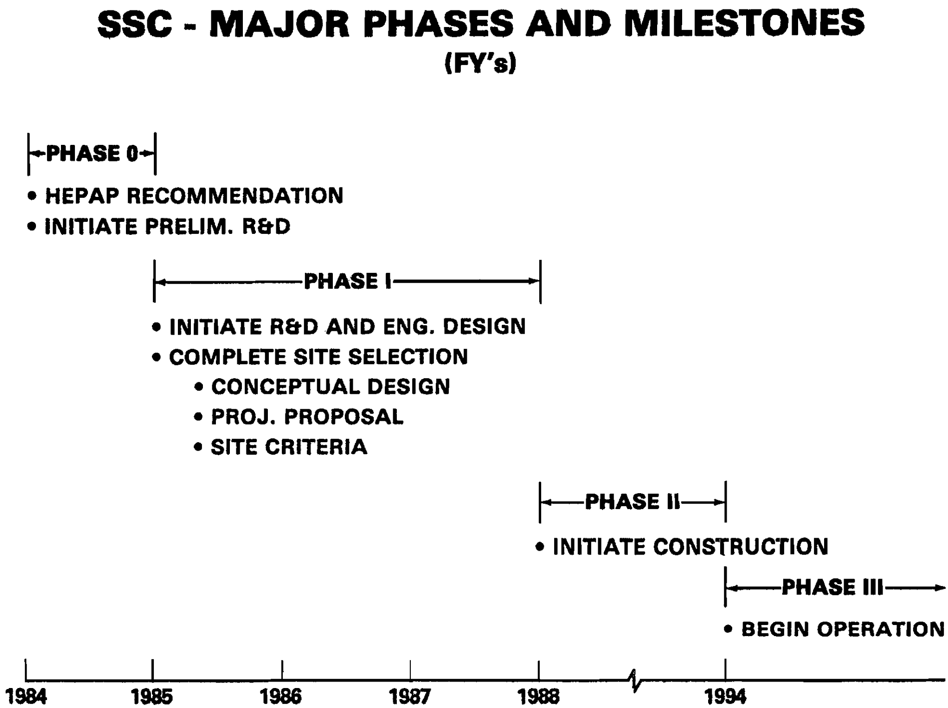

Fig. 2. SSC major phases and milestones. The dates are fiscal years.

Phase 0, the early R&D phase, began roughly in October of 1983. By now we have done the reference design and the cost definition at the feasibility study stage. The reference design was carried out by the organization I showed in Fig. 1. The HEPAP subpanel advised that the R&D be carried out in this phase. Phase 0 will end with the Secretarial checkpoint on proceeding with phase 1 which is scheduled for early August.

The design studies would be done in phase 1. Technical and cost assumptions made in phase 0 would be validated there including the use of supporting R&D. The site criteria document will be developed during phase 1. The conceptual designs will be completed, a proposal made, and systems tests completed. A systems tests here might involve a long string of magnets. Phase 1 will cover a period of three to four years. Funding requirements in that period are in the hundred and fifty to two hundred million dollar range. Phase 2 would be the construction phase. Our goal is to begin that in FY88 and complete it in FY94. Finally, Phase 3 is the reason we're doing this.

Next, a brief review of where we are, and where we're going. The University Research Association was assigned the front end management task in April. That's a very important piece of information for most of you, I believe. The draft reference design report was completed April 30. It was reviewed by the high energy physics laboratory directors and then reviewed this month by DOE. We have completed that review, and we've even completed a draft report of the review,. The bottom line is that we felt that the reference designs as presented were technically feasible, and the costs and schedules, as presented, were credible. We made a few suggestions for increasing the scope of the project and made one adjustment in the cost estimate. Those are basically our findings.

The reference design team did a fantastic job. They did not do that by themselves. They were put together from all the laboratories and some of the universities, and they were given much more information from laboratory staff back at the home laboratories. The proponents of the three designs provided superb documents to the reference design group covering the technical designs, the costs, and the schedule information. One of those proponents was Fermilab and they did a fine job. The final reference design reports will be submitted to the Secretary of the Department of Energy June 4. We'll make that schedule. The Snowmass Division of Particles and Fields Workshop will be the last week of June and the first two weeks of July. At present, the checkpoint by Secretary Hodel for proceeding with phase 1 is not a fixed date, but the result of that checkpoint will be announced on August 6.

For Architecture/Engineering groups, the operational piece of information is that the selection process for AE services and the management of R&D for SSC will be done by the URA integration group. This is the Universities Research Association. There's a member of the Board of Overseers for the administration group here on this panel, John Hulm. Jim Matheson, who's in the audience, is the Vice President of URA. A key part of their assignment is to prepare an R&D plan and a management plan for phase 1 and submit it to the Department by the end of June. The DOE field office for the SSC is the Chicago Operations Office.

In conclusion, there's one other point that I would like to make. It has to do with this idea of excellence and past performance. It is also related to the question Lundy raised, namely, how much industrial participation is desirable and how much is practical in this effort? Table I is a very

Table I. Selected Project Performance Records.

	Final Cost / Initial Cost Estimate
Early High-Energy Physics	$\lesssim 1$
Recent ER "Worst Cases"	1.6
DOE Average	2.5
New Senate Office Bldg	3.0
DOD Average	5.0
Alaska Pipeline	7.8
Recent "Worst Case" Reactors	10.0

brief summary of a list of final cost over initial cost estimates for some selected projects. The number for early high-energy physics projects is based on three projects: the original SLAC linac, the original Fermilab project, and the SLAC PEP project. These came in on cost or slightly below. Fermilab came in slightly below cost. The next line is recent Office of Energy Research worst cases. This was in a time when inflation was hitting us hard. I have not backed out the effects of inflation at all. This is the real world that everybody has lived in recently. We also had some technical problems with projects. For recent ER worst cases this ratio is 1.6. The Department of Energy average is 2.5. I would say that either of those numbers, especially the one for ER, is excellent performance. Then, because in the realm of science we're dealing with people wrapped up in very high technology, the claim is made many times that conventional facilities are easier to estimate. But conventional facilities can have significant overruns, as well. The new Senate office building had a ratio of 3 of final cost over initial estimate. The DOD average is a factor of 5. Now just taking the Defense Department and the Energy Department, there must be something different about the way in which they do business. The Alaska pipeline factor was 7.8 and the recent worst case for reactors is a factor of 10. Now a combination of lots of things go into how this number ends--the motivation, the drive, how hard you work, how smart you are. I think that how one does business here is partly what we're discussing when we discuss what is the desirable industrial participation and what is a realistic industrial participation.

Lundy:

For the ratio of final cost to initial estimate we want to shoot for numbers like one or less. I just wonder if Temple's ratio has a direct correlation with my 1-10 index. That would be a terrible result if it did. We could just dissolve the panel and go home right now. On my index there should be an optimum value somewhere between 1 and 10 that leads to a one in Temple's ratio.

Now to questions and comments. Would someone like to volunteer a comment about a successful project where the final cost met the original estimate and tell us what the value on my index scale was?

Ray Beuligmann (Convair/General Dynamics):

Convair-General Dynamics has been involved in the fusion program for at least seven years. Indeed a superconducting magnet industry has been producing large-scale superconducting magnets, at least for magnets in the range of 10 to 300 tons, for about seven years. There are companies here, such as Intermagnetics General Corporation (IGC), that have been involved in making smaller superconducting magnets much longer. The superconducting magnet industry exists now. It wasn't there when the Fermilab Tevatron was started. Speaking as someone who has been involved with the evolution of the industry, there have been some good stories and there are some that are not so good. A good example that has come out in discussions and is recognized in the fusion program by the committee that met to look at the role of industry in fusion was the Mirror Fusion Test Facility (MFTF). There Livermore Laboratory developed the magnet technology and did the conceptual design but chose to go out to industry for detail engineering design and analysis because the Laboratory didn't have the necessary skills. It knew that it needed the complementary skills that the aerospace industry possessed. That is they needed industrial skills to dot all the i's and cross all the t's. A competition was set up. We were deeply involved with that project. Bob Tatro, here with us today from Convair/General Dynamics, was the program manager for the project. That engineering job was done under budget and under schedule. We were learning about magnets at the same time we were building large coils. That project was a partnership. When they changed that program to MFTF-B and significantly increased the number of magnets, Livermore was then building the coils for which we had done the detailed design. Without going into detail they changed the physics and added a lot more coils to the machine. About $30M worth of magnets then had to be procured. Livermore came out with the specs and industry competed with its ideas. Convair-General Dynamics was successful in winning the competition. So far we have delivered 12 solenoids. We're winding transition coils and axial axi-cell coils now. I am pleased to note that this has been a cost plus incentive program. We have met every incentive milestone to date and collected every incentive fee. We are not going to get rich on those fees because we plow all of them right back into the technology so that we can work on programs like this. We expect to get the remaining incentive awards both on the costs and the schedule milestones.

This is a good example of a laboratory-industry transition. There is an industry now, there are skills. But industry doesn't have all the skills. There's a tremendous amount of skill unique to accelerator magnets within the laboratories. Industry has a different complement of skills. Together we can handle the problem. There will be risks and we are willing to accept those risks.

However, don't ask me to make a fixed price bid on something that you have designed and thrown over the transom to us to build from the print. The upside benefits are not worth the downside risk. We can't even do that successfully within our own companies, design it and do the engineering and then throw it through the transom. This is bound to be unsuccessful when you do it from a laboratory to industry. It must be a partnership from the beginning.

Leon Lederman (Fermilab):

Today we are fortunate to have several people from Japan and Germany here. I'd like to capitalize on that and ask them to give us a rough idea of the equivalent of Ed Temple's table for their countries. In other words, what is the general ratio of final costs over predicted costs for high technology projects?

Cord-Henrich Dustmann (Brown-Boveri):

In Germany, in the field of high-energy physics, the laboratories are also proud of meeting their initial cost estimates. For reactors, we have the same trouble in Germany in that the final costs are much higher than the initial estimated costs. In general, it seems to be the same picture in Germany as in the U.S.

Ryusei Saito (Hitachi):

In Japan we also have some projects suitable to be called Large Scale Science Projects. In most cases, economic conditions are usually not good, especially in the smaller projects or in the R&D projects prior to a big job.

Lundy:

One of the elements that Beuligman implied was an important factor in his success was the key phrase, "cost plus incentive." Now that's something that's almost never used. I think it's accurate to say that DOE discourages us from incentive terms in a contract because positive incentives also usually imply you must put in penalties, and we're never very good at collecting penalties.

John Hulm (Westinghouse):

I'd like to ask a question. I know of some DOE contracts in which there are incentives. For example, GOCO's are operated in that mode, are they not?

Lundy:

Do we have any incentives as a GOCO?

Hulm:

No, but how about contractor operated situations like Stanford or Oak Ridge? Aren't many of those places operated so they have an incentive?

Lundy:

But it's a fixed fee.

Harrison Wroton (Martin-Marietta):

No, at Oak Ridge it's an incentive.

Hulm:

Yes, sure, it depends on performance.

Lundy:

We've got to change our contract with DOE. So you endorse the concept of an incentive?

<u>Hulm</u>:

Yes, I do. Very much so. I think we should do more of it. That's what the free enterprise system is all about.

<u>Lundy</u>:

Basically, it's covering the industry against the risk that makes it work. Not asking them to take a risk in a field that's full of unknowns?

<u>Hulm</u>:

No, DOE could make it so they didn't get anything if they didn't do it right.

<u>Lundy</u>:

That's a fair proposition?

<u>Hulm</u>:

Yes.

<u>Lundy</u>:

Temple defined phase 0, 1, 2, and 3. Do you have a feeling when one can go out for the cost plus incentive or other modes? Can we get industry involved at the end of Phase 0, immediately into the R&D program or would they like a year while we struggle with it?

<u>Hulm</u>:

I believe it's already too late. We saw three designs that were made, one of which had industry participation. I think that's very good. I think industry should be involved from the very earliest possible moment. I would like to see industry involved in the other two designs, to be honest.

<u>Lundy</u>:

You recommend involvement that soon?

Hulm:

In some sense it's too late. But, of course, you can't go back in history. It really isn't too late. At this point, these things are mostly conceptually built. I gather some modules have been built. The next phase would be a great time to involve industry. Let me say from my viewpoint why its so important. There are going to be many industries involved in this machine. It's a very complex system; computer technology and controls technology are also involved. Altogether, a very wide variety of technology is going to be used.

However, I'm only going to say a few words about superconductivity. It's a good example and its the core of the machine. We really couldn't do this project without high field superconductivity. Such a machine could not be built with normal magnets. In my view, and excluding cryogenics, which is a fairly healthy industry in its own right, the superconducting industry is not very healthy at the present time. I make those generalizations, though my colleagues would perhaps disagree. There are a number of small to medium size companies in the industry. For examples, these include the wire suppliers and material suppliers. They are hanging on, although some of them I think are doing all right. Some of these companies are represented on this panel, and they may wish to comment on what I've said.

An important advance was made with superconductivity, the ability to get very high magnetic fields with low expenditure of power. This is a key development which we didn't have prior to 1960. When an important advance is made in a new technology like this, one gets an opportunity. This often happens with such a breakthrough, the first applications are in science. These are primarily by R&D people because they understand, more than anyone else, what can be done with an extension of a variable like the magnetic field. As a result, the industry has been mainly focussed on scientific projects in the past 20 years. The successes are in scientific instrumentation. For example, superconductivity has revolutionized nuclear magnetic spectroscopy. It looks as though it's going to revolutionize medical imaging. In fact, medical imaging may be the first commercial or industrial application in the field. That's going fairly well, but these are fairly small magnets. The other applications are projects like MHD, still basically R&D, fusion, which is R&D, and accelerators, which are clearly a scientific application. Superconductivity simply hasn't found its way into the general world of industry.

But that's typical of brand new technology. In other cases, like lasers, the same situation has occurred. The first applications are scientific and then commercial and industrial needs are identified. We're looking for commercial and industrial applications for superconductivity. They are coming very slowly.

It looked like we had a hot one in power station generators. The same kind of dipoles that were being built for Fermilab can be used as the excitors in thousand megawatt electrical generators. They save a great deal of energy by superconducting excitation. Unfortunately, the electrical industry is an economic disaster at the present time. It's just one of those facts of history that the United States consumption of electricity is on a plateau and hardly anyone is building power stations. We don't expect any new generation capacity for at least the rest of this decade and maybe not even until the mid 90's. So there's very little economic incentive to introduce new technology to the industry. Consequently, I think the application of superconductivity to power station generators is on the back burner at this time. It is being pursued all around the world to some degree--in Japan, the Soviet Union, in Europe, but we don't expect to see a commercial machine introduced for a long time yet.

The only other prospect at this point that I see of a major application is in levitated trains. That is also going on in Japan and nowhere else as far as I am aware. I'm glad they're doing it. It's a very interesting and important development. I'm sorry that we are letting the Japanese do it alone. I wish the Department of Transportation felt enthusiastic about superconducting levitation technology. In short, for large companies, the situation is discouraging. There's not a lot of incentive for our company, Westinghouse, or for General Electric, or even for General Dynamics, perhaps, to build and continue development of superconducting magnets.

So looking at the SSC opportunity, we see this accelerator's going to be the biggest job in superconducting magnets that is coming over the horizon for some time to come. I hope industry will have a major participation in as many phases as possible. If you nuts-and-bolts the job, "one" on Lundy's scale, industry will get nothing. The technology transfer will be zero and even the nuts-and-bolts people won't get any technology transfer. Obviously, at the other end of the scale, Lundy's "ten," there is radically new engineering. New ground is being broken in many fields of engineering. It is difficult to go to consulting industries and say, you guys do everything. I have no difficulty with Fermilab or any other group that is familiar with accelerator design playing a strong role. All the accelerator design knowledge is in the national laboratories and universities; it's not in industry. I have no problem with them providing the engineering leadership needed to put the SSC together since no one else is capable of it. However, it will probably have to be a pooled effort, because it's going to need all the accelerator design knowledge the country has. It may have to be pooled internationally with the entire Western world. However, it's possible to pick out parts of the machine, such as superconductivity, in which industry can have a major part. This would be a place where the engineering participation by industry is essential almost immediately.

I would like to see the SSC R&D group begin to commission magnet development projects soon. A variety of development contracts to build these dipoles put up for competitive bids would be useful. These don't necessarily have to be the three designs which have come out of the national laboratories. Of course, there have to be some specifications on the magnets. The point is to allow industry to innovate. This comes back to what Beuligman said about pushing the drawings over the transom. No technology is transferred by giving industry build-to-print orders.

Lundy:

The design study has shown a very short R&D phase since we want to start construction early in order to finish early. Would you say that a three or four year R&D period is really too short for industry involvement?

Hulm:

It is a little on the short side; however, these magnets are not so difficult. Technologically, they are not radically different from magnets that have already been done in industry. Industry could get on the ball right away and come up with some innovative designs and build some prototypes.

Lederman:

I would like Hulm to clarify the reason why he's anxious for industry to get in at an early stage. If you are discouraged with the pace of industrial applications of superconductivity, is it that you hope the applications will come eventually and that you want to keep industry's hand in?

Hulm:

That's exactly it. Of course, we don't want to do a WPA project. It would be better to lay engineers off than subject them to that. No, I think that because you're going to be building the biggest superconducting project in history, that you owe it to industry to qualify them and to advance the technology for future industrial applications.

Lundy:

We have two representatives from the Nuclear Magnetic Resonance (NMR) industry. Here's a question. How long was it from the time when the light bulb came on and you had the idea magnets should be built and sold until the time a salesman could take orders over the phone? What was the total time span and how does it compare with what you thought it would be when you started? Give us a real time-to-complete over estimated time-to-complete ratio.

Dick Rodenizer (General Electric):

That very factor is germaine to the question whether or not the three or four year R&D time period is appropriate. As far as GE's concerned, we started the initial development work for NMR magnets roughly two to three years ago. In South Carolina, we're currently getting the facility up to speed with very ambitious production targets, starting the end of this year and early next year. I think John Hulm oversimplified the complexity of the NMR magnet. The NMR systems use fairly large magnets. They produce fields which aren't high by your standards--one and a half tesla over large volumes. Undoubtedly, we will be going to higher fields. Uniformity requirements are 10^{-5} over these volumes which is not a simple engineering challenge. That has been done on a timescale similar to what you're talking about here. Another interesting analogy is that the initial work for the magnet was initiated in the R&D center in Schenectady. Their initial role is similar to what you're talking about for the national laboratories. They had the technology base, they started out with the design concepts, and they began to develop the design. However, Medical Systems was involved in the very early stages of the program. We worked with them through the design stages, input on manufacturing, and quality control. The transition from the research and development center to this commercial production business has been extremely successful. I never would have imagined that we could have done it as smoothly as it's now going.

A further point I'd like to make is that this finally is a very substantial commercial product based on superconductivity. General Electric has made a large commitment to the NMR development. There are capabilities and facilities that are now available which just simply hadn't been there in the past. Private industry will do that if there is an appropriate incentive. This can only help the national laboratories.

Lundy:

Carl, IGC is involved in both the nuts-and-bolts side of the business and in making magnets. Seemingly, there is no way you could lose. Would you like to make a comment?

Carl Rosner (IGC):

My association with superconductivity began with the discoveries in 1960. It's been a tremendously exciting technology to be involved in. I spent my first 16 years in superconducting technology with General Electric and was a party to getting superconductivity off the ground within General Electric. But then GE lost interest because there was no apparent industrial application at the time with opportunities to see markets in the hundred millions or even billions of dollars. I maintained my interest and founded Intermagnetics General (IGC) to try to be there when this industry would amount to something. Although some people in the larger companies don't like to acknowledge it, any hundred million or billion dollar industry still starts with the one million dollar industry. IGC chose to commit to that path. It's a bit frustrating to find that when the payoff is there, these large companies jump back in, make new commitments, and rediscover a technology and perhaps relearn something that we have known all along. New money gets wasted in many arenas to try to relearn or re-educate a new generation of participants, ignoring to some extent the accumulated experience that is still there. In fact, there is now a small superconducting industry that's willing to do anything and everything, i. e., both R&D and "nuts-and-bolts."

I remember when Bill Fowler from Fermilab first contacted us in the early stages of the Saver program. Intermagnetics General was quite anxious and willing to build the first magnets in industry. The only thing that kept us apart was the price. We felt that in order not to go into the factor of ten overrun regime, we needed to have sufficient money to do some of the R&D and some of the development work. However, somebody had divided the total cost of magnets that they needed and come up with a price of $10,000. That's what a magnet should cost. And we were asked to build the first magnet for $10,000. That was patently impossible. And so it's to this developmental arena that obviously some thought has to be given.

Now we've moved on to the SSC, and I should really compliment Fermilab for giving me a chance to be here and sound off. I'm grateful for the invitation to be on this panel, and I am excited about the prospect for commercial development of both the NMR and SSC technology. Finally, others have generally recognized the industrial opportunities. And yet, there is a level of frustration as to why couldn't we do this sooner and why aren't we doing it right now here in the U. S. after having learned all these lessons. In particular, John Hulm, who has been a similarly active proponent of getting superconductivity off the ground, also expresses a level of frustration in terms of his experiences in how to do this thing right. There are some real answers out there. I'm proud that, as a small company, Intermagnetics General (IGC) has made key contributions to the success of the Tevatron. Without our ability to produce a conductor of good quality at a reasonable price, the Tevatron might have been a much harder project. So that you don't get any wrong impressions, IGC probably collected less than one per cent of the total cost of the accelerator. Yet our work and accumulated experience was of crucial importance. Last, but not least, the help that we got from Fermilab in making the wire, towards the end of the project, led to a true partnership. These factors made it possible for industrial participation that in the final analysis turned out to be successful for both parties. Bruce Zeitlin in our company was instrumental in holding that effort together and maintaining the contribution and dedication. While this was obviously a corporate effort at IGC, Bruce Zeitlin and his colleagues have been the focal point of this activity, and we've been able to give him the support and the people to make this all possible.

Next, I would like to look at the problem of how to promote industrial, government, and university collaboration. On the basis of the accumulated experience that we have, I think the ideas are there, but the willingness is still the missing link. I think that Fermilab has been particularly successful in putting together a team that had the commitment and the staying power and the willingness to work long and hard hours. I think this has been the case at MMIS as well. We should build on this kind of teaming of partner relationships for the future.

From where I sit, the SSC will be built. We need it from an intellectual point of view, we need it from a national pride point of view, we need it from every conceivable aspect that you can see. The real challenge, however, is to do it constructively in such a way that at the end of the SSC effort there has been a technology transfer to a broader industry. This transfer should allow those participants that have been involved to really be established in a way that gives us a technological edge, if you like, so that we, in turn, can go on to bigger and better opportunities. Unfortunately it's very difficult to find examples at the Tevatron of industrial participation and technology transfer except perhaps for the very limited experience at Intermagnetics. My conclusion from this is that it hardly makes any financial sense to involve industry if indeed the reference design B is the one that has the most to commmend itself. One may ask, why shouldn't it be done at Fermilab? Fermilab already has the facilities, it has the people, it has the experience. How do you transfer that to industry without transferring the people? The way the SSC planning program is going, it's going to require a very hard and conscious effort to jump into industry participation at this point. The next fifty or one hundred magnets could logically be built at Fermilab. But then the opportunity is lost to start this technology transfer right now. That transfer and collaborative commitment literally has to be started from the planning point, because if you lose that first stage and first step, it's too late. People are no longer interested. Industry participation will only be reluctant and may immediately drop off when there either is no profit possibility or when industry is asked to build to print. This is the worst way of getting technology transfer going.

As an aside, I would like to make one more comment in a more speculative vein as to the future course of commercialization of superconductor technology. Would it not be nice if we could find a home use for superconducting magnets. Something like that may be in the offing. This was already a question which I discussed at a visit to the physics department at Stanford about 20 years ago. We were trying to do a market study as part of General Electric to see where superconductivity might be going. I was attached to that marketing effort as a technical advisor. Ultimately that experience led to a transition for me from applied scientist at GE to a businessman. The answer I got from Professor Fairbanks as to where superconductivity might be going was, "you ought to find a way to convince people that they can enormously increase their sexual pleasures if they sleep in a magnetic field." I submit that we may be close to at least testing that hypothesis with the whole-body NMR magnets. Certainly one person can be successfully surrounded by magnetic field and, lo and behold, two people can fit in some of the magnets that we are now building.

But perhaps there really is a potential home use. Whole body magnetic resonance (MR) spectroscopy is around the corner, and I can visualize that some health nuts may want to see what the food they eat in the morning does to their system at night. A handy NMR spectroscopy magnet at home could be used to check out what the food does to their system and how it gets converted. The delightful perspective is that magnetic resonance spectroscopy in addition to imaging will have an ever greater impact on the utilization of superconductivity. Futhermore, in the context of individual participation it is interesting and stimulating to realize that the number of magnets that will be built for applications to NMR or MR in the same time frame that DOE proposes to build 3,000, or 10,000, or 14,000 magnets, may not be so different.

Lundy:

Let me comment on something you said. In the middle and the late stages, the interaction between Fermilab and many industrial firms was very satisfactory and very productive. There has been mutual respect and trust on both sides. I'm personally convinced that with any reputable industry working on the SSC, you would have that same degree of cooperation and warmth. But the problem is in getting started when you don't know who wears the white hat and who wears the black hat. It's like mating porcupines. It's got to be approached delicately because it could go wrong quickly. In the limited time that's available, how can we sort out the pure in heart and the open-minded people that we can put together for the best interaction? How do we filter out the things that are going to end up badly? We really don't have time to work by trial and error on this question. I don't think competitive bidding does it. To my mind, competitive bidding involves a large risk of getting people in who don't understand the job or who have undervalued it, or who have planned to make a profit with change orders. Another option we've discussed is cost plus incentives. Obviously this Laboratory and perhaps others need to become experts in that; this is something we are not at the moment. How do we, during phase 1, get industry to start magnet designs or the analysis of existing designs? Perhaps Dustmann could enlighten us on how DESY and Brown-Bover, in Germany, handled this. Who made the proposition?

Dustmann:

The situation in Europe is different from this country with respect to the tradition of magnet builders. In the last 20-30 years, all the conventional accelerators in Europe have been built with magnets produced in industry. Thus, for conventional machines, there are a couple of companies in Europe which are able to deliver accelerator magnets. Brown-Boveri is one of these companies. On this basis, we came into contact with DESY in connection with the HERA project. This relationship started about 3-1/2 years ago when DESY began to design HERA. They started by contracting an industrial design study. This was contracted to two German companies.

The basis of this design study was, on the one hand, the Tevatron design, which in those days was the basis of plans for superconducting dipole magnets over the world, and, on the other hand, the magnet specification for the field of 4.53 tesla, the length for the magnets of 6 meters, and the harmonic quality which had to be met. On this basis, we started the design study and came to the conclusion that perhaps a cold iron magnet may be better in some respects. This was the basis of the contract between Brown-Boveri and DESY for producing three prototype magnets of our cold iron type. The first of these has been delivered to DESY. Numbers 2 and 3 will be delivered in June or July. The experience here is parallel to that which has been mentioned before--idustry should come into the job as soon as possible; the ideas of industry should be put into the design at an early stage. Finally, the R&D should be done in small steps which can be overseen so that there is interaction before the goal of the final magnet is reached.

Lundy:

I might comment that the relativistic heavy ion collider at Brookhaven (in some sense a replacement for the ISABELLE Colliding Beam Accelerator which was terminated) is probably going to draw heavily on the experience at Brown-Boveri and at DESY because the magnet requirements for the machine that is being discussed will be very similar to the work that's been going on at DESY.

Dustmann:

Let me give you a short impression of what the HERA magnets look like. First, I want to give you an overview of what the HERA project is. Figure 3 is a view of the accelerator enclosure which has a circumference of 3.3 kilometers and is about 15-20

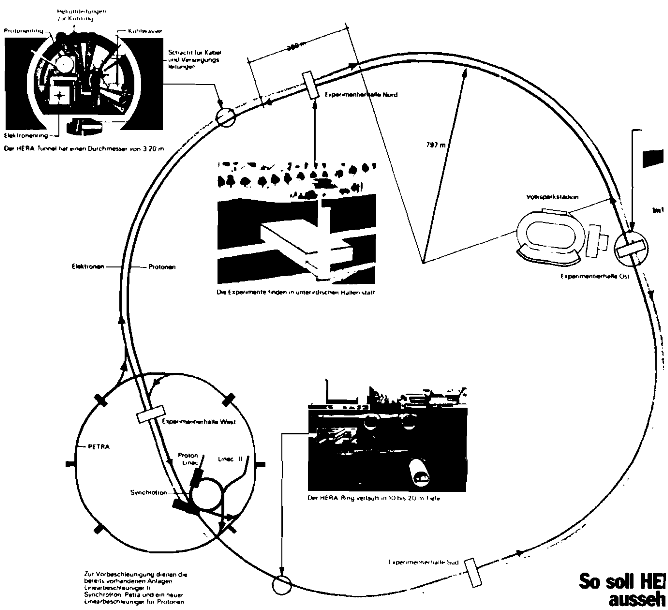

Fig. 3. HERA ring. Note PETRA accelerator at the lower left is used as an injector.

meters below ground level. Notice the PETRA accelerator which will act as an injector. As you can see, there are four interaction regions. Figure 4 shows the tunnel--the same tunnel size has been built for the subway in Hamburg. You see HERA in the tunnel. The young lady was the daughter of Zeus, the boss of the old Greek gods. It is said that all the successful

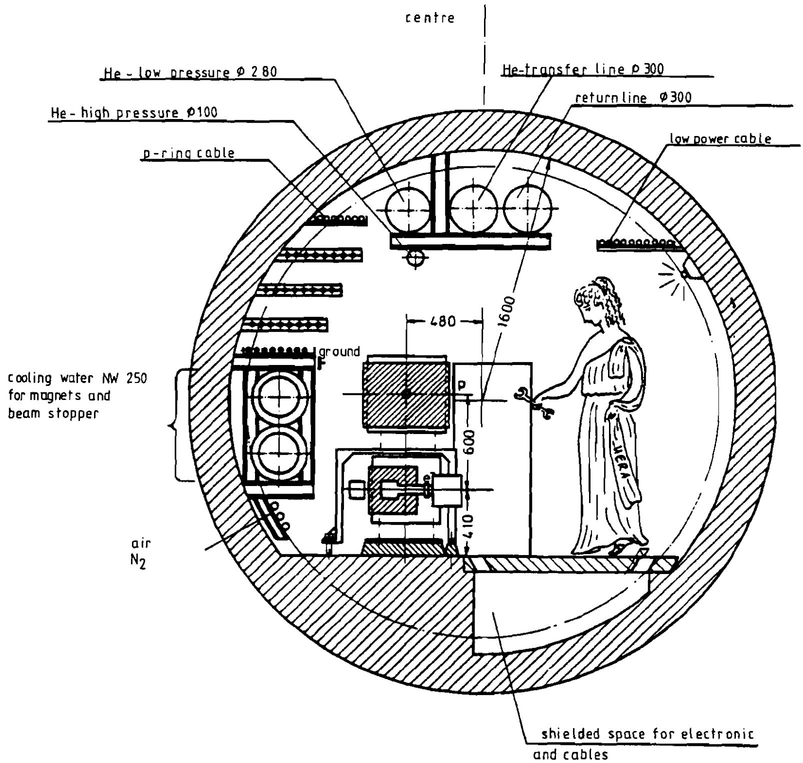

Fig. 4. HERA tunnel cross section.

superconducting projects in the past have had feminine names. As you can see there are two accelerators in one tunnel. Figure 5 shows a cross section of the electron beam magnet. It's a conventional magnet which has been designed on the same principle as the already existing electron accelerator magnet of PETRA.

Figure 6 illustrates the possible superconducting magnets. The upper design, which has been developed by DESY, is very similar to the Tevatron design. That was the basis for the first step of the project where DESY was convinced and knew from the experience at Fermilab that the magnet would work. This evidence was needed to convince the government that superconductivity would work and they could put money into it. This lower picture is our design, based on cold iron. I will not go into the details, but there are some advantages of the cold iron which are summarized in Table II. I will return to this a little bit

Table II. Investment Cost Advantage of Cold Iron Magnets.

Investment	Warm Iron	Cold Iron
Conductor	---	less
Collar	---	---
Iron	---	less
Cryostat	---	---
Fabrication	---	---

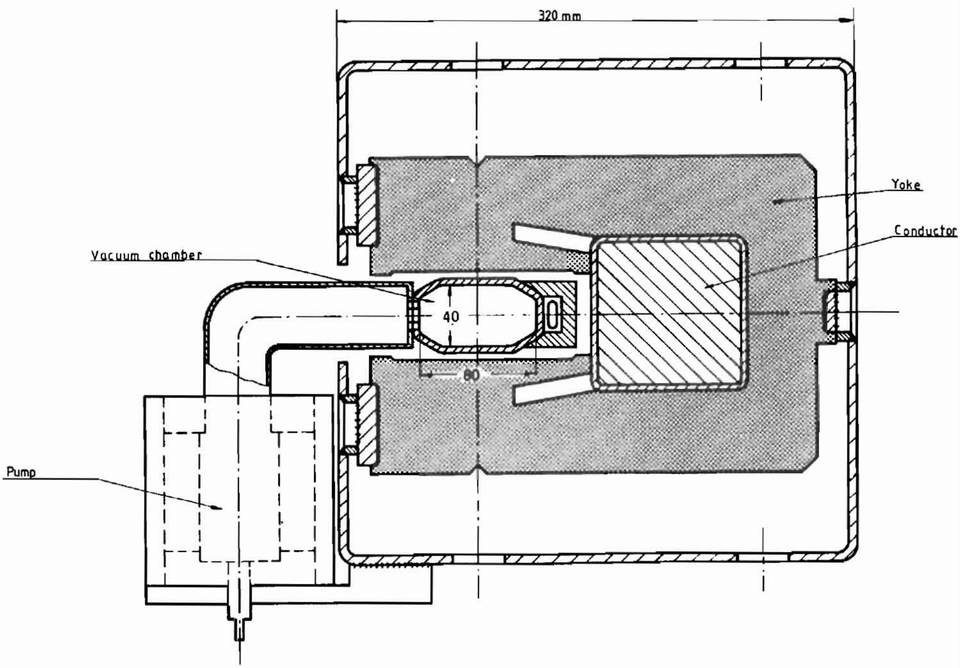

Fig. 5. Cross section of the electron magnet for HERA. This magnet uses normal conductor. Note the vacuum chamber and pump.

later. At some time there has to be a decision between the two designs because, in the end, only one type of magnet can be put into the tunnel. So we have been very lucky that a combination of both of these designs was found which minimizes the drawbacks of each and combines the advantages. This is the so-called hybrid magnet. It may be called hybrid because it comes from two institutions or perhaps it has parts of two different designs.

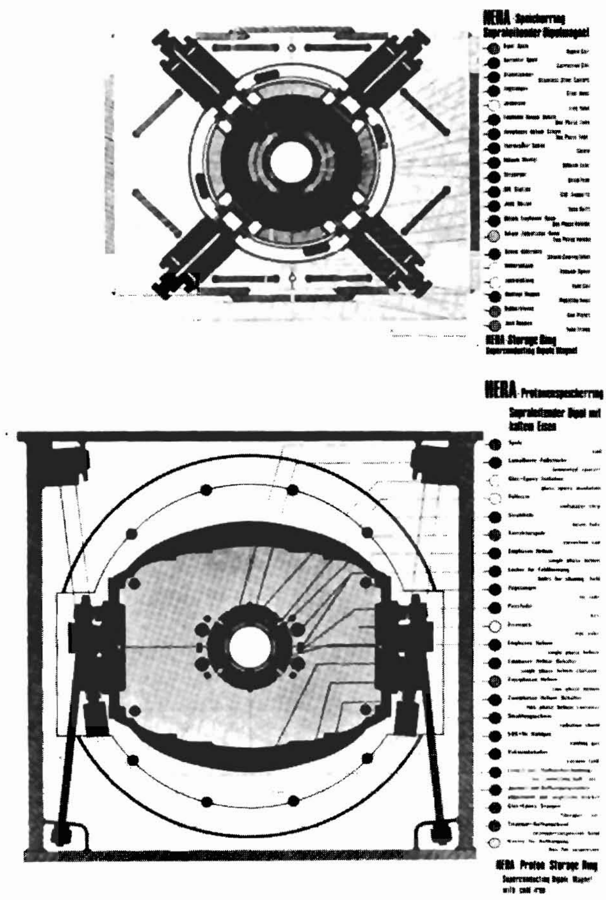

Fig. 6. Two superconducting magnet cross sections considered at HERA. The upper design is similar to the Tevatron. The lower design is based on cold iron.

The winding is inside aluminum collars installed in cold iron. This way the disadvantage of the cold iron is avoided, which is the nonlinearity of the fields. On the other hand, the lower heat losses which are possible with the cold iron are incorporated into the design. This is the magnet that will probably end up being used in HERA. We did some estimates to compare the net costs of the cold and warm iron magnet on the basis of the HERA design. The two designs have been worked out and can be combined for the benefit of the whole project. Now it's obvious that in the cold iron, there's less conductor and there's a little less iron needed. But, of course, you have to pay for this with a longer cool-down time. This item comes under the heading of operational cost. In Fig. 7 I have roughly compared the operating costs by taking the electrical power which

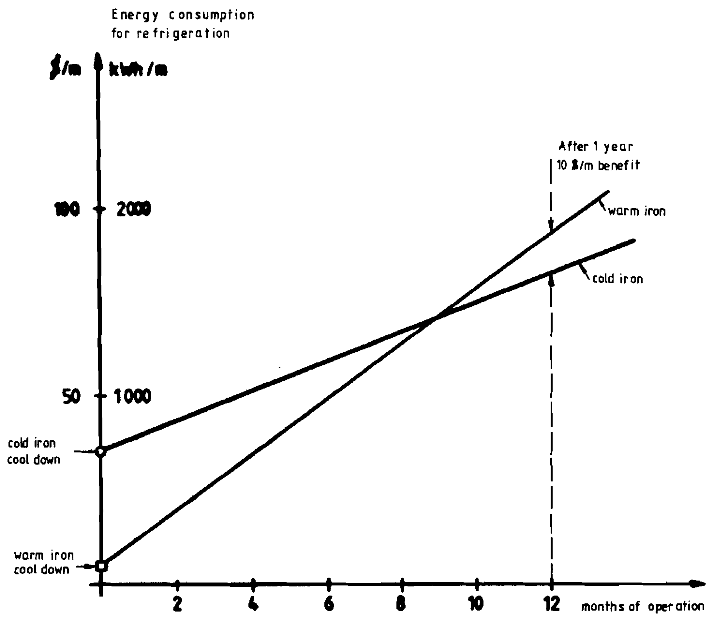

Fig. 7. Comparison of operational cost for warm and cold iron magnets in the case of the HERA magnets.

has to be put into refrigeration. Of course, much more power is needed for the cooldown of the cold iron magnet whereas, a lower level electrical power is needed for continuous operation. After about nine months, you get the benefit of the cold iron. Now, you may say this depends on how often you have to warm up the ring, but I think the high-energy physics people would like to run the accelerator the entire year. If a magnet fails, one would have to warm up only a section of the ring, not the whole ring. So, I think this argument in principle remains valid. I just put this together to give you an example of how collaboration between industry and a laboratory can come to a solution which is a benefit to both of them.

Let me just make three statements about the relationship between the laboratory and industry. It is very important to involve industry as early as possible. That means even during the design phase so that the people in industry know what the problems are and they can train their own people in the shop. Second, there are benefits from merging the knowledge and capabilities of both institutions, and, third, some competition is necessary just to force everyone to do the best they can do.

Lundy:

You raised the topic of magnet reliability. One of the easy aspects of the NMR business is that all those magnets operate as separate gadgets. If you had a failure rate of 1% for the magnets, you would be embarrassed but 99% of the installations are ticking along just fine. A 1% failure rate would be fatal to this accelerator. I certainly don't know how to write quality control standards that guarantee no failures. I'm sure that industry could help with that. At least in this country, it's felt that Japan has an edge at the moment on quality control, on zero defect manufacturing. How would you go about guaranteeing 15,000 magnets for a lifetime of 20 years minimum? What's the warranty policy?

Saito:

Instead of replying to your question, let me show you the Japanese status relative to Large Scale Science Projects (LSSP). At present, we have several Large Scale Science Projects in Japan. These include the construction of a large accelerator, studies for nuclear fusion systems, and the development of a new transportation system using a superconducting magnetically levitated train. The common feature of these LSSP is that while they are useful for humankind and science in the future, they are too advanced and too large. In the past, the scale for developing such a job was comparatively small. It could be carried out by the research people themselves, and the possibilities for industry to contribute were small. However, the recent trend for LSSP is for the scale to become larger and larger, more costly, and with correspondingly increased requirements on reliability. Under such conditions, the participation of industry is gradually increasing. In Japan, this tendency toward industrial participation was there from a comparatively early stage due to various circumstances in our country.

How does industry view LSSP? Strictly speaking, it seems not only attractive but risky. The plan itself is very beautiful. The personnel associated with it are wonderful. Often there is a great deal of money provided for the budget of the LSSP. So an LSSP should be attractive, but there is another element in the LSSP for industry. Industry earns profit by getting high productivity. The LSSP has some problem from this point of view. Typically, the specification of the LSSP is unique and usually difficult. The schedule is often demanding, but at the same time, trial and error is needed before the start of real manufacturing. Often an LSSP costs much more than was expected beforehand, not only by the planner, but also by industry itself. These costs are usually difficult to reevaluate. There is also a problem in that repeated production is rarely expected for the LSSP. However, industry does have a passion for work on an LSSP. There are rewards from the viewpoint of the status of the company and the spinoffs.

Industry has several needs that must be fulfilled to make it easy to participate in LSSP. Consider the case in Japan. There are two ways for industries to join such a project. The first possibility is for industry to act solely as a manufacturer. In this case, the scope of the responsibility and the specifications must be clear and acceptable to the industry. A good plan and design are needed to be sure that the industrial participation will be productive. The price must be reasonable and allow for necessary R&D and contingencies. The other possibility is for a more extended scope for industry in the LSSP. Industry itself has the abilities to carry out planning, engineering design, cost estimation, and scheduling. For some of these items, industry is rather professional. In order to have industrial contributions of industry in much more fundamental ways, the future of the project must be assured, to a certain degree, including the budget. The technical proposal on the cost estimate from industry must be well understood and reflected in the engineering or the budget. If not so, the latter case is not interesting for industry at all. In any case, communication between the planner and industry is very important from the early stage of the project. This early communication makes it easy for industry to participate in the project.

Finally, I'd like to discuss international collaboration. When industry participates in a project in a foreign country, there are some problems, especially for the LSSP. These relate to the status of each country. It is very desirable that the agreement be confirmed between the governments that are involved and that the division of work for each country is well established. The existence of a national research organization providing the appropriate coordination and advising (our own) industry is also desirable for us not only in the lead country but also in collaborating countries. Figure 8 illustrates schematically how such projects could be organized.

Finally, I hope each LSSP of the world, as well as the SSC in the United States, overcomes various barriers and blooms with beautiful flowers, and then gets fruitful results. I expect that Japanese industry can make many kinds of contributions to the LSSP as far as possible.

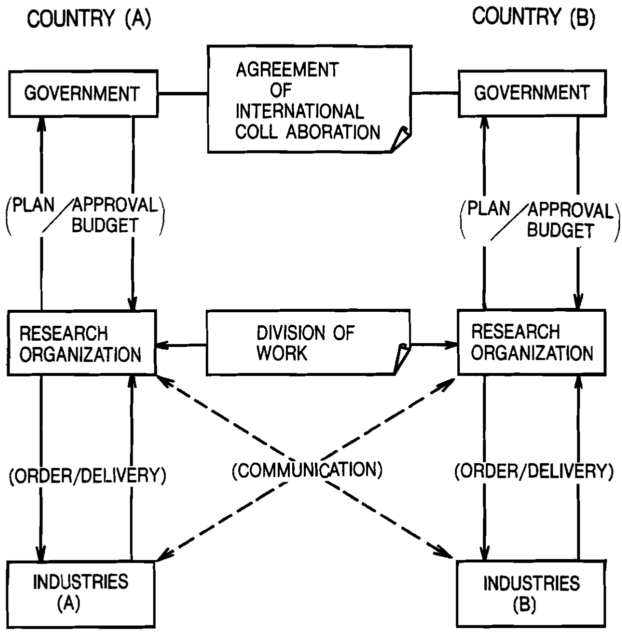

Fig. 8. Typical schema of organization for international collaboration.

As you know, international participation in particle physics has proved quite fruitful. Figure 9 is one such illustration, a picture of the Collider Detector solenoid being prepared by Hitachi for Fermilab. Another example is LCP coil Japan now already installed at Oak Ridge National Laboratory to develop the technology of nuclear fusion.

Lundy:

It comes as no real surprise that industry wants to get involved early in the SSC. However, it can't be that black and white. Some of you must have been buyers, not sellers. Who in the audience has had a bad experience with industry getting involved early?

Bob Tatro (Convair/GD):

Industry wants to get involved. But I raise the following question for the buyer. How do you let industry participate in an equal way so there is competition and do it in a time frame such that when the die is cast, people know what the game's going

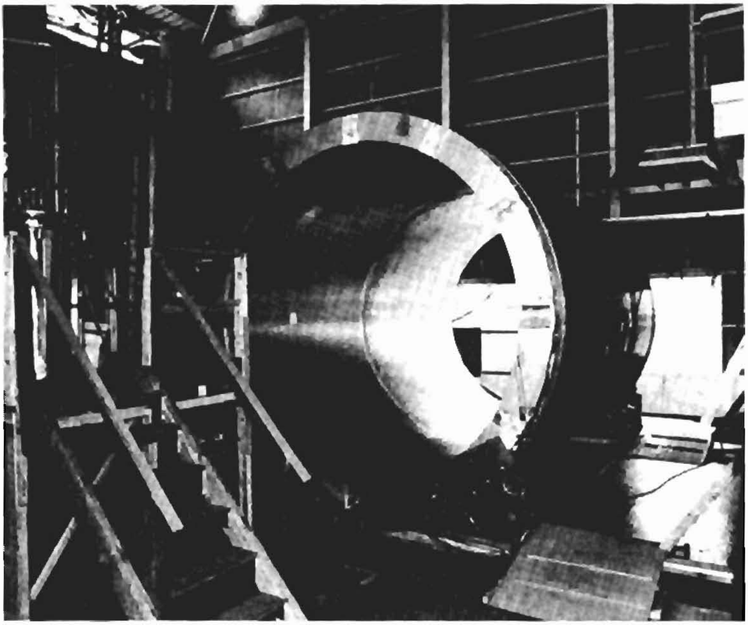

Fig. 9. Superconducting solenoid prepared by Hitachi for CDF at Fermilab.

to be, and all of industry is not strung along for years because everybody's not going to be a winner. There are going to be significant investments, contributions, and commitments by that industry and that company. How can the competition be structured so that we can get involved early, all of us that are interested, and yet the sands will sift and it will sift down to how it comes out. I've gone through this on the MFTF program now for four and one half years. I saw how that program evolved and how we were successful. I haven't heard all the stories on how the competitors felt it evolved. That does concern me because, although we have a very valid strong interest in the SSC, we want to know how we will get in and get out if we're not successful on the SSC.

Lundy:

Of course, you don't mean to imply that only one industry can be successful.

Tatro:

No, I'm not implying that. That's the point. I was going to ask Hitachi, Brown-Boveri, and the other people, how many other companies within their agencies or their countries are interested? How many will be brought along? Is it just one company from this point on in both cases?

Lundy:

Do you have a monopoly on magnets for HERA? A potential monopoly?

Dustmann:

I would hope so. The actual situation for HERA is this. HERA was funded only on the condition that there is European participation on the project. That means that different countries contribute to HERA and this contribution is in hardware. Different countries will deliver parts such as magnets, vacuum components, or other elements. These discussions have gone in the following direction. There is participation by France. They have developed the quadrupole magnets through their own knowledge and resources. There is discussion about Dutch participation. They are talking about making the correction coils. What we at Brown-Boveri don't like so much is that the Italians will also participate in dipole construction. It looks like there will be a division of suppliers, partly from Italy and partly from Germany. So, if you ask for the number of companies that may be involved in this business, it's between three and five in Europe, I would say.

Lundy:

My own thinking on this (and this doesn't represent DOE or even Fermilab policy) is that we have to take advantage of some of the facilities that are at the laboratories in order to compress the R&D timing. Say that at some point we finish a design that represents a laboratory's best shot at what would be a successful final magnet. The national labs commence to build these, somewhere between 10 and 100 units. At the very beginning of that process, you invite in teams from firms that are likely to be suppliers. I don't know how you'd limit it to a few or even what the right number is. Those people come to the laboratory probably at their own expense. After all, they've got to take some risk in this. They work alongside laboratory people and help build magnets. They learn all the good parts and all the bad parts. They keep their own counsel. They go home and make propositions for design changes, new methods, radical deviations, and somehow you evaluate and you select one, two, or three, and they get the job. The tricky part is selecting the necessarily limited number of initial participants. You can't entertain a hundred teams; you'll never make a magnet.

David Vroom (Raychem):

The question of how many companies can be involved goes back to how soon industry gets invited. The sooner you get industry involved, the more people you can have participating with relatively limited risk. A number of companies can be asked to submit early designs of their concepts. This will begin qualifying companies and perhaps uncover other design possibilities. On the other hand, if you wait until you get right down to the final bidding, of course it's going to be extremely difficult to choose who's going to build the magnets.

Lundy:

In fact, I think it's impossible because if we take the risk for that, that's over the transom engineering.

Patrick Stone (UOP):

SSC is probably five or maybe ten years late in involving industry in this arena. The time to involve industry in this arena is when the first idea is broached. I'm basing this on what has been recently called the justification for the federal role in anything (of course administrations change the way they look at this). But in science that's not the situation. In science, you're dealing with a non-proprietary but totally monopsonistic market. There is only one buyer for the SSC. And the last time I checked, the king never made mistakes. In fact, according to the government rules, he's not allowed to because that's the taxpayer's money and that would involve fraud. Anytime you enter a program where you're not allowed to make mistakes, you've already made one.

Now this is not meant to chide our leadership in any form. I've been in the aerospace market myself for the better part of 30 years, both as a technician and a marketer, and I've bled over it. But, if someone feels they have the vast knowledge necessary to provide leadership in a project as expensive, as involved, and as obvious as this particular one is, we ought to go back and let him throw the first stone. You can't do this by committee. Ferdinand Porsche had a great quotation. He said that there's never been a winning race car designed by committee. So the first thing you have to do is pick the leadership and this time you picked Fermilab. Now, the less Fermilab does other than lead, the greater the likelihood they'll come up with the best solution. If you want to involve industry, you start with the bottom of the pyramid--lots of folks--and you do it as early as possible. And in the early parts of the program, when you're talking about ideas, you very rapidly establish those people with whom you can deal in both a trustworthy and a competent way at very little expense. By the time you get up to where you are today, you have, in fact, established the club. And those who are competent to propose know who they are. You'd better put it on the street and you better put it on the street early and you better put it on the street when it's just ideas. This should happen even if it's only holding a conference, to say, hey, we're thinking about building a huge, new accelerator and get industry involved right there because the presumption of perfection is in itself the basic error.

Bob Remsbottom (Wisconsin):

One question that needs to be addressed is how do we stay away from a repeat of the Large Coil Project (LCP), where there's a large number of coils being made by a bunch of different people. Industry is involved in it from the start, but will it ever go on line? I don't think that what we're doing on the SSC could ever survive something like that.

Beuligmann:

In answer to that, the LCP will go on line. It may not go on line with all the magnets. There are some questions about one or so, at least at full current. LCP is a technology program. That's different than what we're talking about here. First of its kind, industry had never built anything like that and so there were problems. It should not be put in the same context as other magnets that have been built that are slightly different. The PMS-F magnet has been running three years now without a hitch. The MFTF Ying Yang's have been tested. Large coil is a different program, and I would be glad to go into the constraints that drove the technology and some of the excuses (some of them not so good).

Sure, we were naive at the start. There are some other problems within the industry of lack of commitment. That can be embarrassing to the whole superconducting magnet industry. But there won't be a repeat. I don't know if I've answered all those questions. I think it's pretty obvious that there is an industry out there now. We have been working on SSC for a year helping one of the labs do some work leading to production. Still, we are not working in the heart of the system. That's inherent to the work here that Bob Remsbottom, Dick Lundy, and the others have been doing at Fermilab and Clyde Taylor and others at other laboratories. We have been working on that problem for over a year now. Anyone who thinks that they're going to come on line and reinvent a wheel just doesn't understand where SSC is today and where it can go. We'll build those magnets for SSC. By we, I mean the whole business and the industry. I have no doubt that SSC can be made with the concepts that are being talked about here. It is not an infant industry anymore. I wouldn't say it's mature, but it sure isn't an infant.

Convair-General Dynamics is up in the order of 60 to 70 million dollars worth of business just in superconducting magnets. That's a lot of broad skills and experience and some of them were built on the magnetic skills that existed in the laboratories. We complement the set of skills at the laboratories, and we don't intend to replace the skill that exists in the laboratories. That is a very inherent ingredient to the success of SSC. I disagree with John Hulm. I don't think you allow industry to go out and reinvent some wheels and then propose. There are many man-years of knowledge and skill existing right here in the team at Fermilab. The same holds true at some other laboratories. I've got a lot of respect for industry, but I don't propose that even with our background we're going to go out and reinvent that wheel and get a better idea. We can find improvements to it. We can complement it, but we cannot reinvent it. Have I answered your question?

Remsbottom:

Basically, yes. You can look at General Dynamics as being successful on LCP. Here you have a very broad industry, many people involved and so forth. If we have a thousand magnets sitting out here it wouldn't be very good if one of them blew.

Lundy:

There are two comments that I haven't heard, and I'm surprised and I want to throw them out to see why I haven't heard them. One was triggered by the mention of Porsche. One of the reasons that Porsche goes in for auto racing, besides the sheer fun of it and the advertising value, is that they believe by participating in racing, they're able to attract engineers who also do the passenger car work that are much better than they would be able to hire otherwise. I would think that the project we're talking about here has enough sex appeal that it's a recruitment aid or a morale builder or a source of adrenalin for a firm. Is it a project that will get a company's adrenalin going?

Mike Morgan (Meyer Tool):

The laboratories have the expertise. They have to define the magnet and the magnetic field properties. And oftentimes, from the past experiences I've had in working at the labs, the requirements are not always realistic. Industry is now in the position to point that out to you. It makes sense for the laboratory to design the magnet and to go to industry, who have the background and experience and capabilities for putting things like this into production. The laboratories will have to show industry how they've done it, and explain what they will accept. They can't go to industry with requirements that are not attainable.

Lundy:

I understand that. If you, in fact, elect the route--build to print, you've got to prove those prints are good by building some.

Hulm:

Or come back later and change the design, piece by piece, as you go along through the manufacture.

Lundy:

Of course, the classical reason for getting industry involved is that they will economize, find cleverer ways to do the job.

Morgan:

There's another aspect that I think is important. I've been on both sides of the fence. The laboratories must come to a cognizance of the cost of making changes. This is in light of an earlier comment about unscrupulous companies taking the job on a low bid and making it up on change orders. I'm sure that that does happen. But by the same token, changes are extremely expensive. If you have good documentation and look at what it costs to do something, you sometimes scratch your head in bewilderment. If you had to estimate what that change order would cost, you wouldn't believe it. And you don't believe it after you find out what it did cost. And the people at the laboratories look at it and say, hey, you guys really stick it to us. But we haven't.

Lundy:

I couldn't agree with you more. One of my own biggest problems during the Energy Doubler was to prevent changes. Naturally we had lots of ideas, but we knew that if we incorporated them that we would have never produced magnets.

There would be 990 different dipoles. Of course, that takes some of the fun out of it. If you're going to have to make 15,000 magnets all looking alike, it's going to get boring. Be prepared to face that.

Rosner:

I'd like to make a point that is now a little bit on the other side of the fence from my earlier observations. There was some good judgment and rationale why Fermilab did as much as it did in-house. There were many changes in the early periods, because a superconducting accelerator was an evolving concept using new and evolving technology. As an old magnet designer and builder, I really appreciate what Fermilab has done in designing and building these magnets. It is a fantastic achievement to have every single magnet in a ring of 800 or a 1,000 magnets work. Most superconducting magnets operate in the dc mode, but Fermilab encounters the most demanding application, namely, pulsed operation where you have to worry about cycle fatigue and shorts and who knows what. I don't want to detract from what Fermilab has done and the way it's gone about it. The fact of the matter is that it was successful and that's a real tribute to the way they went about it. The question that I was trying to address is how can that experience and that accomplishment be translated and transferred to industry thus allowing us to go on to bigger and more productive projects. HERA couldn't have done what they're planning to do without the Fermilab experience. That's where the benefit from the Saver experience has gone. When you try to bring it closer to home, unfortunately I have a hard time seeing the benefits to U.S. industry at this juncture. For the future, that's what I hope will come out of the SSC experience.

Lundy:

Thank you for your compliments. At Fermilab we also believe that it was the right way to build the Doubler. That doesn't mean it's the right way to repeat the experience, and that's part of the reason for this discussion. I was going to ask Leon Lederman to put his hands over his ears so I could say how to transfer the technology--hire all the smart people.

Lederman:

I saw John Hulm nodding at the last comment and yet he has been rightly pointing out that collaboration in the construction of such a machine would be worthy for a lot of other reasons. Do you see a possible conflict between these two points of view?

Hulm:

No, but I certainly would agree with what Rosner said. The Fermilab Energy Doubler is an amazing engineering achievement. Fermilab deserves to be congratulated, but I also agree with Rosner in that if you organized the SSC work along the same lines and continue in the same way, the technology will remain buried in the national laboratories.

Lederman:

I'm sorry. I wasn't clear. My question has to do with international collaboration. I'm raising a very delicate point. I'd love to have a full and frank discussion. You have raised the issue and others have raised the issue of the importance of international collaboration in constructing the machine. I resonate very strongly with this idea. But I'm now asking you how this is consistent with the other virtues of SSC as direct benefit to U.S. industry? You don't have to answer that if you don't want to.

Hulm:

It's a very difficult question, of course. I assume that the other countries that might be involved in such a collaboration--Japan, Germany, Switzerland, France--would all want to get their industries the same kind of benefits that we would hope to get for U.S. industry. I would hope they would act in some kind of competitive mode in the procurement. The Japanese do this all the time. Almost all of their major projects involve several companies, at least in the first stage, and they try to get the best ideas from the companies and then somebody wins the follow-on project. Of course, we do this in many other areas. I think it's very dangerous if any kind of monopolistic situation results from the SSC. Something would have to be built into the agreements that we would have with our international friends.

Lederman:

You don't see any problems with Mr. Saito's model?

Hulm:

Not basically.

Wroton:

I'm so far down on the learning curve on SSC that I hesitate to speak, but I got a strong impression from some of the earlier comments that industry cannot support changes, that build-to-print is even a conceivable point of view in this kind of project. I'm not at all certain that it is. In fact, there are many ways to contract R&D, some of them very suitable for rational changes. As was noted already, the ability to hold changes to a desirable minimum is part of that rationality.

An example is the Viking Lander spacecraft sent to Mars, with a couple dozen experiments, an unknown planet, a year in transit, an unknown atmosphere, an unknown surface structure. Finally, the spacecraft had to be sterilized for 48 hours before takeoff with almost no testing following the sterilization. You're all familiar with the pictures from cameras and the negative results concerning life on Mars from the biologists.

That was a performance contract. The contract was very simple. It said, go to Mars and take data successfully. It was an incentivized contract which had as its final carrot some fifteen million dollars of incentive that would interest almost any corporate president or anybody under him. That amounted to about 3 or 3-1/2 per cent of the value of the contract. The total contract was about 400 million for Martin-Marietta's part which was to provide the lander. And that was totally successful. We got 100% of that award in the end. It took seven years to do that job, during which time the project operated against a countdown schedule which says 1,022 days to launch or 368 days to launch or what have you. We had an absolutely definite window during which we had to complete that job and get it off, as well as providing the entire design of the mission and the support of the spacecraft and the scientific team associated with it. Now, of course, there were lots of changes in a program like that. When we began, we didn't know what you do to accomplish that kind of a task except put in large contingencies, which was not an acceptable option. All those disciplines that we built into the Viking lander and which have been developed in the aerospace industry and in other high-tech industries are usable in the SSC application. Martin-Marietta is now the operator of the Oak Ridge Laboratories. There is a major incentive on that contract, although I'm not personally very familiar with it. My point is I don't believe that the fact that there will be developments after the initiation of the build process needs to be an overwhelming concern as to whether these types of products can be built in industry or must be built in a government laboratory.

Lundy:

Thank you for your comment, because in addition to being worthwhile, it reminded me of a question that I wanted to ask. In a recent article in Fortune, the thesis was put forward that the biggest challenge right now for U.S. industry is to learn to respond to change more rapidly. This is not because change in itself is a desirable end, but because, we find that the life cycle from product introduction to the profitability phase to obsolescence is getting shorter and shorter. It's approaching months instead of years. This is particularly true for computers and electronics and some consumer goods. A company has to be able to get with it and produce and get out and get on to the next boom, whatever it is. The times have to be shortened, the flexibility has to be increased. Some companies do that by building a skunk works so that the smaller operation can be more dynamic. Presumably, whatever you do to learn to respond to these accelerated product life cycles, also enhances your ability to respond to changes with minimum friction. It may be that corporations may have to learn to respond more quickly to the customer, be it a monopsonistic customer like a laboratory or to the millions of consumers out there. I think the U.S. auto industry, properly goaded by Japan and by Western Europe, has been able to respond more rapidly and had to in order to survive. They had lost touch not only with the customer but also with the rate of change of the customer's wishes.

Beuligmann:

I think we're talking about two kinds of changes. Some changes are inevitable. You can have something going in production and you can no longer get that semiconductor or part. This happens all the time. There are 50 changes a month for a program that I know about in the Pomona division making missiles, typically because of items no longer being available. You can't get rid of changes but you have to minimize them. However, when you've got not one Viking lander that you're making, but thousands of magnets coming down the pipeline, you've got to watch those changes. In particular, you can't make changes readily in a contract where you say, hey, industry take this fixed price or put these tough incentives on and then see changes flowing freely. You're going to end up with big teams in negotiations all the time trying to figure out how much the change impacted. Of course there will be changes. I think the changes Lundy's talking about are in the rapidity of getting marketing sense of what the consumer wants and implementing the change.

To reiterate, there are two different kinds of changes here. In the R&D phase you need changes, flexibility within the government, and fluidity between the laboratory-and-industry teams. But when you hit production, the outlook should be that the system is good and really cost effective, and that we're not going to play with it willy-nilly. At Convair/General Dynamics, we've had troubles with some of the laboratory programs in superconductivity. We've had to sit hard on people who wanted to make changes who came into the program late at Livermore. Essentially they've wanted to just do it their way. It wasn't better, it was just a different way. That is disaster for a program. Luckily the program managers up there understood that and took care of the problem. But there are always some engineers who want to do it a different way because it's their way, and they're more comfortable with it. They don't pay the bill.

Tatro:

Since January 1981 when we started the contractual effort (at Convair/General Dynamics for the MFTF-B), there have only been five contractual changes to the program, every one of them initiated by configuration changes at Livermore. We've had a cost plus incentive contract. We recognized at the beginning from our discussions with Livermore that they wanted us to be flexible, they knew things were going to change, and they didn't want to be dogged to death by contractual changes on our part. We scoped and priced the project initially, knowing we'd have to handle some changes. I assure you the magnets were not defined with performance specifications. In fact, they were not well defined. We are within the contracted budget through the entire program on every single type of magnet, the solenoids, the axicell, the transition coils, the high field insert coils, and over 3,000 thermal shields with 800 different configurations. In each of those areas many changes have been made, but we have done the design and fabrication and we are on budget and on schedule. That's the kind of situation that must be accommodated. The contract has to be that way but there is a strong program requirement to recognize there are people within the laboratories and the government, as well as within industry, that are going to want to do something that is just different. That attitude has to be stopped and stopped quickly when it develops.

Lundy:

To summarize, I want to ask Ed Temple if he's profited from this discussion and if he's figured out what the R&D director's going to do about all this. Perhaps his answer will be, "You haven't quite solved all the problems, but don't worry because I'm sure there are going to be many more meetings like this." It sounds like there should be. I apologize that this one is seven or even ten years too late. I didn't even think about superconductors at that time, thank goodness.

Temple:

The SSC as we see it now, is four to five times larger than the effort for the creation of this laboratory (when compared in comparable dollars). The number of superconducting magnets that we're going to build is something like 10 times the number that we did on the Doubler. Given that situation, it's just a fact that we have to use industry in a very big way to help us do this. Now the best way to do that is not known by me. I showed some cost figures early on that are the very foremost consideration in our mind. We have a situation where we can't make errors.

On Lundy's scale of industrial participation, the Doubler was something like 2.5. Remember the Doubler wasn't just buying up some bolts, but did involve contracting some pieces. A nine or ten on Lundy's scale is something like buying a Van de Graaf from the High Voltage Engineering Company. There are some other recent projects, such as TFTR where industry was involved in a big way building devices that had been designed by either Princeton or industry. I would put TFTR around four or five on the Lundy scale. MFTF took a different tack and in very many of their large systems, put out performance specs. The most outstanding performance in this area was in their vacuum chamber system, the huge tank that housed the Ying-Yang coils. It included the tank plus all of the normal vacuum pumping system for that tank plus all the cryogenic system. (By the way, it was another quote on an MFTF refrigerator that we used in the SSC cost estimate, so at least within the Department of Energy, we do have some transfer of knowledge and information. That's hard, as well, sometimes.) That contract was put out at about 30 million dollars and they finished it with less than a 5% increase. It was a performance spec and they had some changes along the way. I think that's fantastic performance. We've heard how they've done a major set of the solenoidal superconducting coils and some of the axicell coils. I would put MFTF maybe on a scale of six to seven.

Now, for those of you here, we have two projects that I think you might be semi-interested in at this time. They are CEBAF and the SSC. For different reasons, I think they're both going to be significantly higher on the Lundy scale than the Doubler. CEBAF is not going to be able to put together an organization fast enough to do all of their own work and they're going to have to do some awfully big pieces by putting them out to industry. For the SSC, as I noted, we don't have the manpower in our labs to do it, and we couldn't keep them there if we did. In any case, it's probably not a very effective way to do things. In my estimation, a well-managed project will probably end up in the four-six range. It would be helpful to have your thoughts on what you would like to do, what you have done well, and how you think we can sort out some of these questions. This should be helpful to the labs, to URA, and to us at DOE in actually getting our plans together.

I would like to note that the schedules that I showed earlier were basically those put together by the not so infallible planning bureaucracy about four months ago. Based on the feasibility study effort, the reference design study effort, and especially the conventional facilities work that the architects from Parson Brinckerhoff did, we have to get moving on the SSC now if we really plan to do it in the overall time frame that I showed. That means that if we really do this project in the time scale that has been outlined, we're going to have to get some changes in our planning way up front. From that standpoint, this meeting happened none too soon. I hope that there will be exciting and continued interaction amongst you all and the participants in the lead contracting groups in the SSC over the next year.

Lundy:

Thank you, Ed and thanks to the rest of the panelists. As the chairman, I declare this session formally closed.