SSC ALTERNATIVES: CRITICS COLLIDE WITH DYSON

I read with interest Freeman Dyson's Opinion column (February, page 77) on alternatives to the Superconducting Super Collider. He expresses his interest and support for further research in elementary-particle physics but suggests that the SSC may not be the best tool—nor even an appropriate tool—to press those investigations further. He therefore suggests that the start of the next accelerator be delayed until some more elegant technology becomes available.

His argument is reminiscent of one that was made just about a year ago. At that time the new ceramic, "high temperature" superconductors were just making their appearance and exciting us all. Many who were fearful that the cost of the SSC would eat into other potential research were quick to suggest that the SSC should be delayed until one could take a better or cheaper next step in that field of physics. All of us are still excited by the new superconductors and are hopeful that they will eventually realize their apparent potential. However, it is fair to say that cooler heads and cooler superconductors have gradually cooled the fervor of the most ardent preachers of delaying the SSC on that account.

Most experts in advanced accelerator techniques agree that the prospects for replacing "conventional" accelerator systems with new accelerator concepts are at least as remote as the application of the new superconductors to accelerator magnets. Nevertheless it is still legitimate to question the rush. Why not wait a decade or two to probe more deeply into the structure of matter? The answer is that such a question is not unique to the SSC. There is almost no scientific inquiry whose postponement could not be suggested on the grounds that it will probably become easier or cheaper in the future with the development of new technologies. To accept that argument is the road to stagnation.

Volumes have been written establishing a strong physics motivation for the SSC. Few scientists have challenged the importance of the questions that lie immediately ahead to be answered. Dyson does not contest that. The argument, then, appears to come down to a cost–benefit analyis. Many scientists are awed and frightened by the idea of an investment of several billion dollars in a research facility, and several billion dollars *is* a substantial investment. Admittedly this nation has many gnawing problems that beg for investments of that magnitude, but the long-term, fundamental solutions to most of those problems rest on our regaining the leadership we have lost. To do that we must regain the courage and the daring that have stood us in such good stead in the past. The national debt and deficits are almost irrelevant in that context. To recapture our vitality we can and must undertake promising new initiatives. The SSC is one such venture.

One common trait of nay-sayers is that they tend to tote up the costs of undertaking a new project while ignoring the costs of not proceeding. Postponement of the SSC would be quite deadly to prospects for continuing progress in man's longstanding and highly productive investigations of the structure of matter. "Big" science is not bad science, and "small" science is not necessarily good science. In fact "big" and "small" are not meaningful categorizations of research. True, today's accelerators are big in anyone's lexicon. But research is done on big accelerators by university faculty members and their graduate students. High-energy physics still provides a major attraction to, and a rigorous training ground for, young scientists, engineers and technicians, most of whom carry into industry and other fields the skills they have developed in pursuit of elementary-particle physics.

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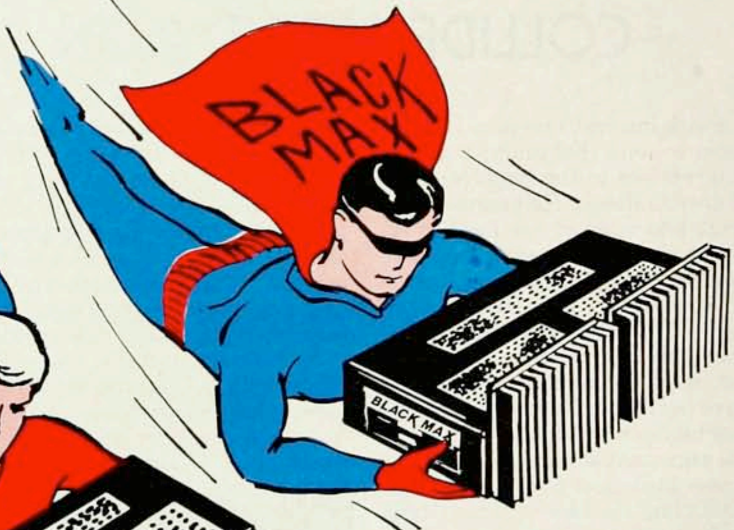

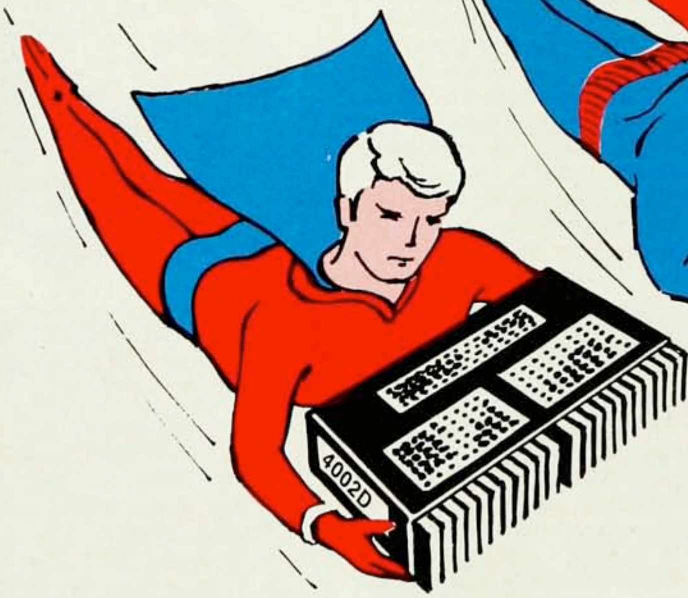

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research in this field. It involves a whole world, not only of accelerator specialists, but of theoretical physicists, particle physics experimenters, detector specialists, data-handling experts, pattern recognition specialists, *et cetera, et cetera*. There is no way in which this interwoven set of skilled and creative individuals could be placed "on hold" while an uncertain possibility for improving one or another of the pieces is fully explored. Any such delay would surely lead to atrophy and dismemberment of the existing infrastructure.

There is general agreement that SSC physics is important and that the SSC will provide information essential to further progress. There is general agreement that the SSC, as designed, is a technically sound instrument that will be able to function as proposed. The United States can certainly choose to abdicate its traditional leadership role in research of this kind. Such a decision would be characteristic of many we have made in recent years, decisions that seem to be leading us down a path of lost self-confidence and decreasing competitiveness in the highly competitive world of science and technology in which we used to excel.

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3/88

The exciting possibility of a TeV-energy e^+e^- collider has captured the imagination of a large number of accelerator and high-energy physicists. In the January issue of PHYSICS TODAY (page 26) Andrew Sessler writes about the accelerator R&D that will eventually form the basis of such a collider, and in the February issue Freeman Dyson emphasizes the importance of this work and raises the question of whether it offers an alternative to the SSC.

The Department of Energy asked the same question when formulating plans for and deciding to proceed with the SSC, and I served as the chairman of a High Energy Physics Advisory Panel subpanel charged with addressing it. The report was completed in December 1985 and endorsed by HEPAP. The substance was presented to a broad cross section of the particle physics community during the opening session of the summer study of the APS division of particles and fields at Snowmass, Colorado, in 1986. It is available to the public from the National Technical Information Service as DOE/ER-0255, "Report of the HEPAP Subpanel on Advanced Accelerator R&D and the SSC" (December 1985).

The first paragraph of the executive summary of the report reads: "The field of advanced accelerator research is flourishing with innovative ideas. Most of these hold promise for the future possibility of a multi-TeV, electron–positron linear collider capable of investigating processes involving fundamental particle masses which the Superconducting Super Collider will bring under study. However, the state of development of this research is such that it does not offer an alternative to present SSC plans. At least 10 years of development is needed for these advanced concepts to evolve into designs for practical, engineered devices in the multi-TeV energy region. There is no guarantee that this can be done successfully, and the costs are uncertain. At the present time, there is no persuasive evidence that such a collider ever could be built at a cost less than that of the SSC."

Two major reasons the subpanel reached that conclusion were that there was no operational experience with linear colliders, and the basic parameters of a multi-TeV-energy machine were unclear because there was no concept meeting the requirements of high-energy physics, which include energy, luminosity, construction costs and operating costs. All have strong influences on an optimized design; acceleration gradient is not the only important parameter. Since the fall of 1985, when we did our study, there has been progress in both these regards, but they are still obstacles. The Stanford Linear Collider is being commissioned. Many of the systems have performed at a level sufficient to reach the initial luminosity goal of 6×10^{27} $cm^{-2}\,sec^{-1}$, and operation for high-energy physics is expected to begin this spring. This luminosity goal is five to six orders of magnitude below that needed for a multi-TeV collider, and although successful operation of the SLC is a prerequisite for the evaluation of future linear collider designs, a higher-energy collider requires significant extensions of technology beyond that of the SLC.

Studies at the Stanford Linear Accelerator Center, CERN, KEK and Novosibirsk have clarified the design trade-offs, but several fundamental and almost all engineering aspects of a multi-TeV collider remain to be studied. A lower-energy e^+e^- collider does offer interesting particle physics opportunities even though it does not have the mass reach of the

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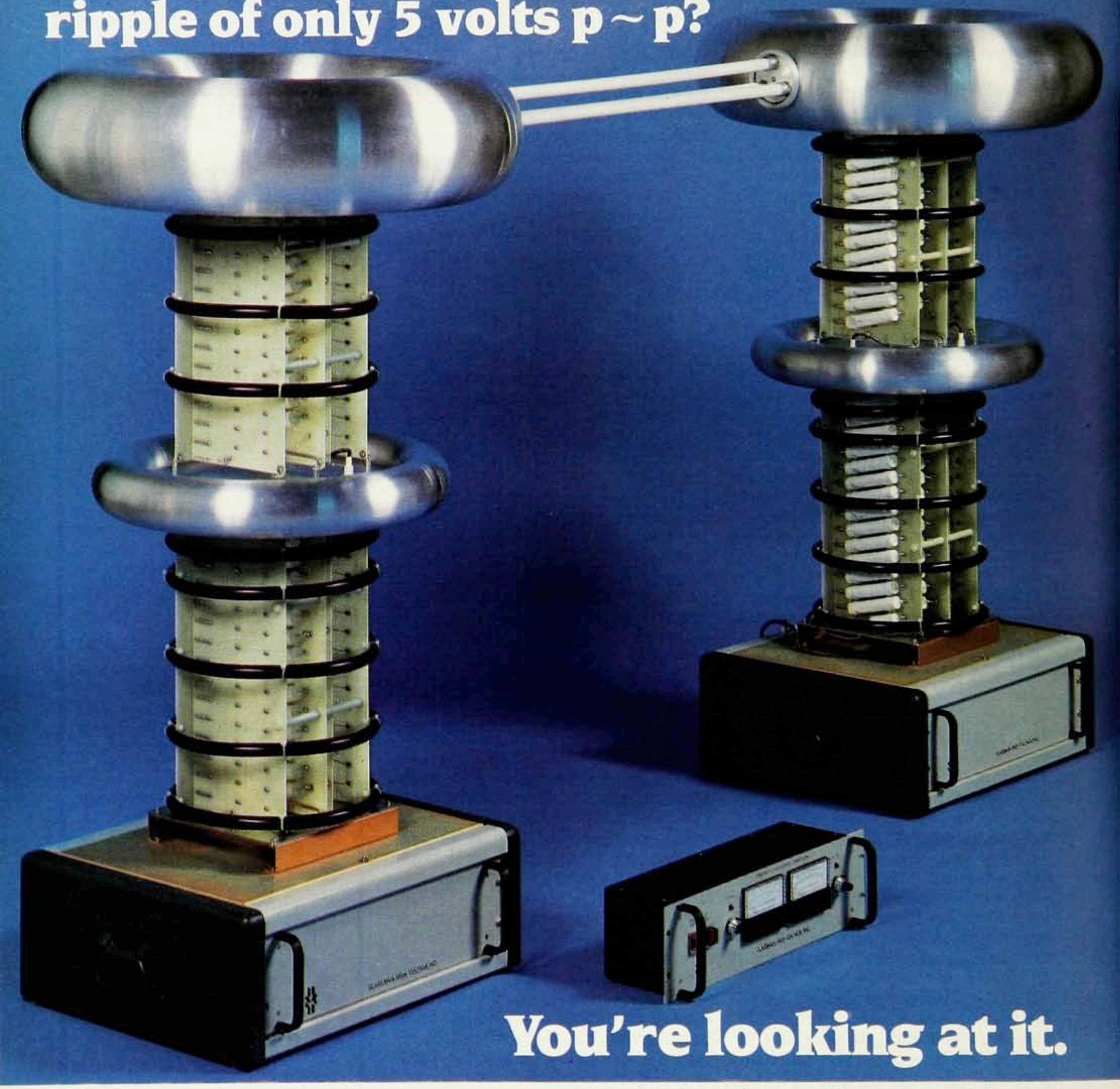

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SSC, and most accelerator physics and engineering problems are less severe at lower energies. For that reason some physicists are optimistic that a parameter list and initial cost estimate for a collider with 0.5 TeV per beam will be developed soon. These could serve as an R&D focus, and a conceptual design backed up by some engineering studies could be available in the mid-1990s. However, this work is at an early stage, and such a machine does not have the SSC's mass reach. I still concur with the conclusion that advanced accelerator research does not offer an alternative to the SSC.

I join Sessler and Dyson as a strong supporter of advanced accelerator research. Quoting from the last paragraph of the executive summary of the report: "There are important reasons to support and pursue work in advanced accelerators. Many substantive issues in addition to available energy arise when comparing electron–positron and hadron–hadron colliders. The relative simplicity of electron–positron annihilation events makes experimental programs possible which are not accessible with proton machines. Historically the complementarity in research at electron–positron and proton accelerators has led to major advances in high-energy physics. In the future, an electron–positron linear collider, operating in the energy range comparable to the SSC, may be attractive. In addition, linear colliders offer the long-range prospect of energies beyond those of the SSC. Intensive advanced accelerator research in the coming years will be required to bring these possibilities to fruition."

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3/88 *Ithaca, New York*

In the past, physics facilities have sometimes been built because it became technically possible to extend significantly the range of some parameters, such as energy, and sometimes for a definite goal, such as finding antiprotons or $W^{\pm}$ and Z bosons. Normally such facilities have been proposed and built with some, but not extensive, review. Now that the cost of new facilities has become so large, the review process has become very important. As may well be imagined, the particle physics community, more than anyone, wanted to be sure that the SSC would achieve the needed scientific goals and that there were no reasonable alternatives. To be as sure as possible, many activities were undertaken.

In addition to extensive normal research activities in the past decade that naturally came to focus on the need to study small cross sections at high energies, the division of particles and fields of The American Physical Society convened large, international studies at Snowmass, Colorado, beginning in 1982. In 1982 the focus was on what kinds of facilities could best achieve the scientific goals of particle physics. High luminosity emerged as an essential requirement, in addition to high energy. In 1983 a High Energy Physics Advisory Panel subpanel proposed the SSC as the logical facility to develop. From 1984 to 1987 the accelerator, detector and physics aspects of the SSC were intensively studied,[1] with a resulting increase of confidence that the SSC could achieve the goals set for it. An important advantage of the SSC is that both its energy and its luminosity can be significantly increased if motivated by physics results after a decade or two of experimentation.

At every stage alternatives were considered. In particular, a HEPAP subpanel under Robert Siemann of Cornell, reporting in December 1985, concluded that for the foreseeable future a multi-TeV, electron–positron linear collider was not a serious alternative to the SSC. [See Siemann's letter above.] Two and a half years later it is still not possible to say whether the problems that need solving to develop very-high-energy e^+e^- colliders will be solved or at what cost. Indeed, it is not at all clear that multi-TeV e^+e^- colliders will be less expensive than pp colliders per TeV of useful energy. At best we may hope that when the SSC is a mature facility it will be possible to construct a multi-TeV linear e^+e^- collider if the physics questions lead that way.

In particular, the luminosity problem is an uncompromising one that may be fatal for such techniques. For such an accelerator to be of use for particle physics, it must be possible to use it to study the kinds of cross sections that appear in gauge theories. The typical unit for such cross sections is that for $e^+e^- \to \mu^+\mu^-$, which is about 10^{-37} cm^2/s, where s is the square of the center-of-mass energy and is in units of TeV^2. To be competitive scientifically with the SSC, an e^+e^- collider must have a center-of-mass energy larger than about 3.5 TeV. Since most cross sections fall with s, the luminosity must increase correspondingly. In addition (contrary to what Freeman Dyson suggests) studies show that backgrounds generally grow worse at electron colliders as the energy increases, due to two-photon processes,

two-W processes and the large number of additional final states that are possible. To deal with the backgrounds by using appropriate cuts, even higher luminosities are required. A few interesting cross sections do not fall with s, but their scales are set by the weak interactions and again require large luminosity for detection. Further, for the kinds of collisions that will be studied in the future, evidence and studies from CERN and SLAC indicate that the traditional "cleanliness" of e^+e^- colliders decreases relative to hadron colliders and that for many investigations the two kinds of colliders are comparably clean. This is because for collisions at large momentum transfers the hadronic debris is unimportant, and because an increasingly large fraction of e^+e^- collisions have undetected energy from radiated photons. Although for all of these reasons higher luminosity is required as the energy increases, confining the beams to tiny regions to get high luminosity is very difficult. Beam spreads of at most tens of angstroms in one or both transverse dimensions are required. We all hope it is possible to solve those problems, but no one can say how long it would take even with an intense effort.

To return to the initial remarks, it is important to realize that the SSC cannot be a "flop." Calculations that any interested physicist can follow through show that if the quanta of the Higgs field (which many theorists believe provides the mechanism for particles to get mass) exist, then they can be produced and detected at the SSC. If they do not exist, it can be determined experimentally that they do not. In that case the interactions of W bosons have to deviate from the perturbative predictions for their behavior, and they can be studied at the SSC. The correctness of these statements is not obvious, but emerged from the studies mentioned above and from the research of a number of people before, during and after the studies.

In addition to this rather specific goal concerning the study of "Higgs physics" and the origins of mass, the SSC extends the ranges of mass scales and interaction scales where a "surprise" could occur by factors of 10–100. Of course no one knows whether a surprise will occur, but even the absence of new phenomena over such a large scale would be a tremendous constraint on ideas and would likely be very fruitful, similar to the way the absence of certain rare kaon decays led to the prediction of the charmed quark and a good estimate of its mass (the "GIM" mechanism).

Progress in answering the questions of particle physics requires data on particle interactions in the TeV region. No easy way is known to get such data. An advantage of the SSC is that it is based on known technology, and it can be built on schedule and for the expected cost, so that we can move ahead. Even then the lengthy time scale, requiring almost a decade before the physics begins to emerge, is unavoidable. The particle physics community has strongly supported research on alternative acceleration techniques in hopes that some approach would become feasible as a tool to study particle interactions. It is true that if one waited long enough, alternative technologies might lead one to proceed differently. However, if this time scale is as great as the expert studies suggest, the US will have retired from forefront research in this field by that time, for one cannot now foresee any manner of addressing comparable physics issues during the period when the SSC would otherwise be the premier world facility. Progress requires the periodic injection of significant new data to maintain the knowledge base and talent that now exist and to attract bright young people to careers in elementary-particle physics research. Supporters of particle physics such as Dyson should certainly be concerned about the deleterious effects of a potentially lengthy period in which the research frontier is not being advanced. If there were any better way to address the scientific goals set for the SSC, or if there were any alternative way to achieve them, we would be trying it.

Reference

1. R. Donaldson, J. G. Morfin, eds., *Proc. of the 1984 Snowmass Wksp. on the Design and Utilization of the Superconducting Super Collider.* R. Donaldson, J. Marx, eds., *Proc. of the 1986 Snowmass Wksp. on "Physics of the Superconducting Super Collider."* Proc. of the Wksp. "From Colliders to Supercolliders," Madison, Wisc., in Int. J. Phys. A **2**, 1055 (1987). R. Donaldson, M. Gilchriese, eds., *Proc. of the 1987 Berkeley Wksp. on "Experiments, Detectors and Experimental Areas for the Supercollider."*

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3/88

In the February issue, Freeman Dyson stated his opposition to the construction of the SSC, advocating instead an electron–positron linear collider using laser acceleration

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techniques. In his advocacy of electron colliders over proton colliders, he cited the productivity and cost effectiveness of SPEAR and CESR as examples of the superiority of electron colliders.

As an experimentalist who has conducted research at a substantial number of the world's accelerator laboratories and as a former administrator who was intimately involved with the construction of CESR, I am dismayed at the lack of understanding of such issues portrayed in Dyson's Opinion column.

Succinctly, my amazement at the recommendations made is that Dyson, who is world renowned for his understanding of space and time, has neglected time as a factor for consideration in the planning of facilities. As the Book of Ecclesiastes states, "There is a time for everything," and the wise man knows this statement well.

The successes of SPEAR and CESR were spectacular at least in part because the timing was right. In both cases, the decisions to develop the facilities were the result of the initiative and drive of a few individuals who correctly perceived the situation and seized upon the opportunity to construct the facilities without a broad consensus from the scientific community or formal approval for construction projects. The potential gains were high, the costs were low and the timing was right.

The exact opposite is the case with Dyson's recommendation. No linear accelerator using laser acceleration techniques has ever been built, nor has the feasibility even been demonstrated. The duration of time needed to develop these areas into practical devices is totally unknown, as are the attendant costs. This is clearly not the time to consider these concepts as viable alternatives to the SSC.

One other time factor needs to be taken into account, and that is the time derivative referred to as momentum. Several years ago it was recognized that new facilities needed to be constructed if the pace of exploration was to continue. A High Energy Physics Advisory Panel subpanel recommended the consolidation of the options into a single facility, the SSC. I did not agree with that decision then, feeling that the time scale was too long at that time and that an intermediate facility prior to the SSC was necessary to maintain momentum. The outcome of Dyson's proposal would be to further lengthen the time before a new facility became available, with the inevitable consequence being the decay and conceivably the demise of the US high-energy program in the foreseeable future. I cannot but question whether Dyson has projected his thoughts into the future and clearly foresees what the consequences might be of following his recommendations. The arguments do not appear to ring true and generate instead further questions as to the rationale behind the negative attitude toward the SSC.

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DYSON REPLIES: I thank my critics for helping me to achieve my purpose, which is to start a discussion of alternatives to the SSC. I do not expect to convert them to my way of thinking, nor do they convert me to theirs.

I see the basic issue between us to be whether we regard the world of particle physics to be one-dimensional or multidimensional. I am not advocating a huge monolithic effort to build an electron–positron collider as an alternative to the SSC. I am advocating continued exploration of the frontiers of particle physics in many directions. If the universe of particles is one-dimensional, with energy the only relevant variable, then the frontier is a single point and a single machine can explore it. I say that the universe of particle experiments is at least three-dimensional, the three most important variables being energy, accuracy and rarity. Rarity means the fraction of particle collisions that produce the particular process that the experiment is designed to study. To observe events of high rarity we need an accelerator with high luminosity and a detector with high discrimination.

I am not denying that energy is important. Let us by all means build accelerators of higher energy when we can do so cost-effectively. But energy is not the only important variable. I did a little historical study of particle physics discoveries for which Nobel Prizes were awarded. I found that roughly one-third of the discoveries were on the energy frontier, one-third were on the accuracy frontier and one-third were on the rarity frontier. In some rough sense, the three frontiers are equally promising places to look for new laws of nature. Only one-third of the frontier lies in the direction of higher energy. My alternative to the SSC is a mixture of many different programs, looking for opportunities to do great science on all three frontiers.

In conclusion, I urge my critics to remember that the universe is, as the biologist J. B. S. Haldane said, not only queerer than we suppose but queerer than we *can* suppose. There is no illusion more dangerous than the belief that the progress of science is predictable. If you look for nature's secrets in only one direction, you are likely to miss the most important secrets—those that you did not have enough imagination to predict.

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SSC: Opinion Splitter

One of the many considerations in a decision about the proposed Superconducting Super Collider project should be the extent to which physicists favor this project. The physics community in the United States is well represented by the membership of The American Physical Society. Unfortunately this society has no mechanism for polling its membership on an issue.

I therefore undertook such a poll myself. Because I had to rely on my own personal resources, the sample had to be modest. It was selected on the basis of a pattern of location in the directory of The American Physical Society. The questionnaire was sent to 247 names.

The letter included a brief explanation of the poll, a slip to be returned with the vote, and an addressed but unstamped return envelope. The slip to be returned contained the following text:

"Return this portion in the attached addressed envelope.

"In view of the circumstances as I perceive them, I am/am not (cross out one) in favor of the construction of the Superconducting Super Collider as projected in the present plans submitted to Congress."

The forging of ballots was guarded against in two ways. The return envelope was addressed with a rubber stamp, the forging of which would be difficult. In addition, the return slip was embossed with a personal "*ex libris*" embosser, the forging of which would also be difficult. A cursory inspection of the returns indicated no cause for anxiety about forging.

By the deadline of 5 February 1988 (six weeks after the original letters were mailed), 26 original letters had been returned to me as undeliverable and unforwardable. Thus 221 ballots presumably reached the addressees.

Of these, an astounding 109 responded. Such a 49% return, with only one mailing and no return-paid provisions, appears to indicate that the issue is of interest to physicists.

Of the 109 responses, 2 were illegal in that the addressed return envelope did not accompany the ballot, in spite of the underlined request. Of the 107 valid ballots, 59 (55%) were in favor of construction, 45 (42%) were opposed and 3 (3%) were undecided.

Of those 107 ballots, 87 came with US postmarks and 20 with foreign postmarks, the latter presumably from foreign members of The American Physical Society.

Among the 87 domestic returns, 46 (53%) were in favor of construction, 39 (45%) against and 2 (2%) undecided. Correspondingly, among the foreign respondents, 13 (65%) were in favor, 6 (30%) against and 1 (5%) undecided.

Considering the modest size of the sample, one can conclude that the community of physicists, as represented by the membership of The American Physical Society, is about evenly divided on the issue of the construction of the SSC.

Insofar as the SSC issue affects all scientists in the United States, it would be of interest also to have a similar poll of scientists outside physics.

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Remembering Richard Feynman

Thank you for publishing Richard Feynman's article on his "inside view of the Challenger inquiry" (February, page 26) when you did. I realize that it was only by a quirk of fate that its appearance coincided so closely with his passing. Still, you could not possibly have printed a better epitaph if you had tried. The article embodied the essence of Feynman's character that made him a physicist's physicist. He is sorely missed.

ROBERT J. BARKER
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What Washington, DC, needs is for an army of Feynmans to descend and start investigating everything in sight. What Richard Feynman had to say about NASA could be said about a lot of organizations, including the many faltering private-sector companies in the US that once were global leaders. When the leadership starts listening only to those who tell it what it wants to hear, decline is