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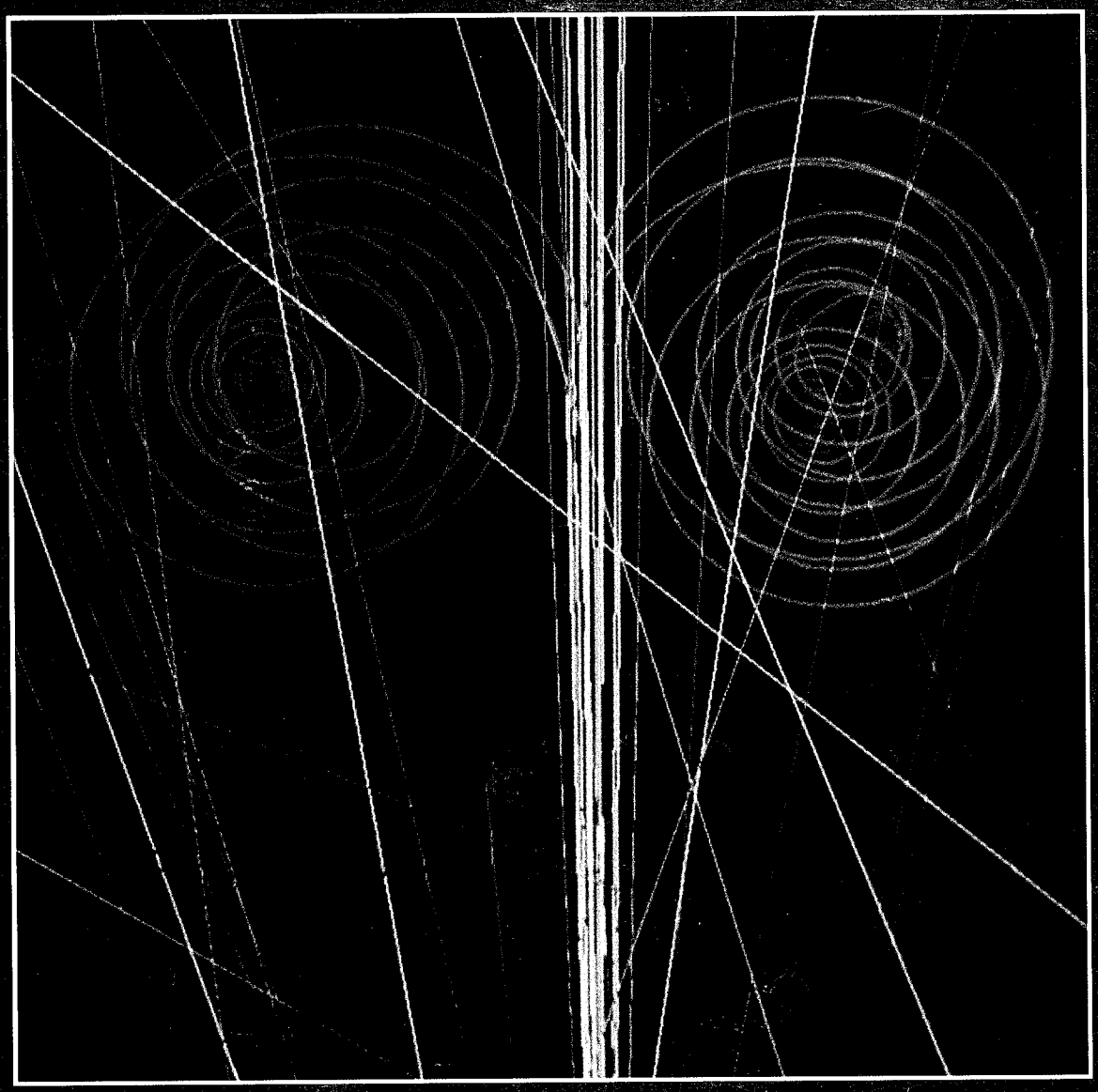

Beam Line

A PERIODICAL OF PARTICLE PHYSICS

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Cover: A Monte-Carlo simulation of an electromagnetic shower in a liquid-hydrogen bubble chamber. Unaffected by the applied magnetic field, one of the (yellow) photons traveling upwards within the "bremsstrahlung core" interacts to produce a (red) positron and a (green) electron. A more expanded view showing all of the shower radiation is provided on page 9.

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DEPARTMENTS

LOOKING AHEAD: The Next 15 Years in U.S. High Energy Physics

by STANLEY G. WOJCICKI*

The challenge is to construct the new SSC Laboratory on a timely schedule while maintaining the vitality of the U. S. high-energy physics program and planning for a proper transition of the existing laboratories.

THE U. S. HIGH ENERGY PHYSICS program is in the throes of a major transition. A chain of events has begun with the approval of the Superconducting Super Collider Laboratory (SSCL) that will have a great impact on our field in this country. This important new initiative will offer unparalleled new opportunities to extend the energy region that can be explored and hopefully will provide answers to some key questions in high-energy physics (HEP) today. But at the same time, as with any major change, it presents some important new challenges. Those challenges are the focus of this article. I caution the reader that the opinions expressed here represent my own views and do not necessarily reflect either the results of High Energy Physics Advisory Panel (HEPAP) deliberations or formal policies of the U.S. funding agencies.

*Stanley Wojcicki is presently chairman of the the U. S. Department of Energy's High Energy Physics Advisory Panel (HEPAP).

At the September 1990 meeting of HEPAP I gave my view that in thinking and planning for the future we should focus on three major challenges:

i) The need to construct the SSC on a timely schedule and to insure at the same time that in spite of its large scale the traditional goals of HEP laboratories, namely free and unencumbered pursuit of basic knowledge by all qualified scientists, will not be compromised.

ii) Maintaining the required vitality in the U.S. HEP program during the intervening years to insure research opportunities for young people and significant U.S. participation in addressing the current questions in the field.

iii) Planning for a proper transition for the existing U.S. HEP laboratories into a future when they will no longer find themselves at the energy frontier. This is especially relevant for Fermilab and SLAC, our two labs that for many years have provided the highest energies attainable in the proton and electron realm, respectively.

I elaborate on these challenges in the remainder of this article.

SSC IS UNDOUBTEDLY the largest pure basic research construction project this country has ever undertaken. Its size is frequently used as an argument that one needs to have a major change in the construction procedures that will have to be followed to assure its successful completion. It might be thus useful to compare the essential differences (in scale and other features) between SSCL and our previous largest construction project, i.e., Fermilab, which was also a new "green-field" laboratory.

Initial Fermilab construction cost was $250 million, in then-year currency. Subsequent additions and improvements probably double the overall investment. The accepted, DOE-endorsed SSCL cost is $8.2 billion. Clearly these numbers are not directly comparable because the SSC collider will be completed about 30 years after completion of the Fermilab 400 GeV accelerator. How much the purchasing power of the "scientific" dollar will have decreased during that time lapse is a subject of mild controversy. It is certainly not less than a factor of 4, which is the approximate consumer price index increase during that interval, and probably not more than a factor of 8. But in addition, the accounting methods have changed somewhat in the intervening years. Certain costs included in the SSCL estimate (detectors, some R&D costs) were not a part of the Fermilab official cost. I would thus guess that the SSCL probably represents about a factor of 4 ±1 real increase over the initial Fermilab investment.

What about increases in scale or technical complexity over existing capabilities? Fermilab's 400 GeV nominal energy represented a factor of 13 increase over BNL's AGS or CERN's PS. SSC will give us a factor of 20 increase over the Tevatron's eventual energy of 1 TeV. SSC is certainly more complex than the initial Fermilab accelerator, but that increase has to be viewed in the proper context. During the last 20 years there has been enormous progress in understanding beam dynamics, in available instrumentation, and in general experience with high energy accelerators and colliders.

I would thus argue that from the point of view of scale, complexity and even cost (after all U.S. GNP has also increased significantly during the intervening period) the SSCL does not present a radical departure from our past experience. There are, however, several other factors that do contribute to the difference and may influence significantly its development and the progress of U.S. HEP. I would like to elaborate on my views on this issue.

PROBABLY THE MOST important factor is the significantly greater size and complexity of the SSC detectors. The general-purpose SSC detectors will effectively become laboratories within a laboratory. The old procedures of selecting them and their leaders, their design, construction and associated management, and the old pattern of detector funding are no longer applicable. New ways have to be found. I believe that the construction and management are especially difficult issues, since SSCL will need to play an important part in the management even though, at this time at least, it is very much short of the required expertise to do so.

The required heavy dependence on industry is another major new change. The Fermilab magnets (both main ring and Tevatron) were constructed in house; a large fraction of SLAC klystrons were also built within the laboratory. On the other hand, the SSCL will be entirely dependent on outside suppliers for

its magnets, with the resultant exposure to potential cost overruns and schedule slippage. This new *modus operandi* will also require significant administrative overhead within the laboratory.

The increased oversight by the government agencies is another important new development. Over the last thirty years, we have seen a steady growth in the required amount of reporting, in the number of reviews, and in the complexity of the approval system for laboratory decisions. This growth, if left unchecked, has the potential of diverting a significant fraction of laboratory resources into efforts related simply to satisfying formal government procedures.

Finally, I wish to point out that for the first time in our history the new facility is replacing a single-purpose HEP lab as the frontier energy institution. The Fermilab 400-GeV machine took over from AGS at BNL (a multi-purpose laboratory) as the highest energy machine; the AGS replaced the Bevatron at LBL (another multi-purpose institution) about a decade earlier. I believe that this difference is important for socio-political reasons because it creates an issue about Fermilab's future that appears more difficult to solve. I shall return to that point later.

IN THE ABOVE DISCUSSION I tried to describe the circumstances in which we find ourselves today and within which we have to work to accomplish our goal: the timely construction of a first-rate laboratory in the true tradition of other U.S. HEP institutions. Let me now address two additional issues of extreme importance to this first challenge that also have a close connection to the second one—*i.e.*, the vitality of the U.S. HEP program in the next decade—namely the issues of timeliness and manpower.

The "construction time" for the new SSC Laboratory is long compared with previous HEP projects, and very long when compared with almost any other scientific endeavor.

The SSCL was recommended by HEPAP in 1983, received its first construction funds in 1988, and is scheduled for completion in 1999. Thus the "construction time" for this new laboratory is somewhere between 11 and 16 years. This is a time scale that is long compared with previous HEP projects, and very long when compared with almost any other scientific endeavor. It is also very long compared with "natural" personal time scales in HEP: a graduate student's research career (~3–4 years), a post-doctoral appointment (~3 years), or the length of service of assistant professors before being considered for tenure (~6 years). Thus every effort must be made to insure that this schedule will be adhered to. There will undoubtedly be pressures to delay the completion time as a way to respond to future fiscal crises, but to give in to these pressures would be self-defeating. Not only would the total cost rise significantly but one might also experience significant personnel losses as quality people became discouraged by the ever-lengthening timescale.

This brings me to an issue that I consider most crucial in the whole SSCL picture, that of manpower and recruitment. The SSCL is going through a crucial design phase right now, needing to finalize some of the features of the booster complex within a matter of months. Thus it is essential that the relevant and as yet still vacant senior positions in the accelerator area be filled as soon as possible. This will require participation by the U.S. HEP community in the SSCL to an extent greater than has been the case up to now, and it will mean that some of the personnel from the existing labs will have to help out significantly, either by joining the SSCL or by having their home labs assume some responsibility for part of the SSC design and/or construction. This undoubtedly will have an impact on at least some of the programs at the existing labs.

THE NEXT CHALLENGE IS that of maintaining a vital U.S. program in the intervening decade. Clearly that program will be based principally on the existing U.S. accelerator laboratories, with a smaller but nevertheless significant effort at non-U.S. facilities or centered around non-accelerator experiments. I have already noted some of the reasons why the current program has to thrive, mainly in connection with

the long SSC time scale. It is only through the vitality and excitement of ongoing research that we, the U.S. HEP community, will be able to attract and train the next generation of particle physicists and keep the current practitioners in the field. If we accept the goal of a strong U.S. HEP program for decades to come, we have to insure that there is no lack of important research opportunities in the intervening time before SSC completion.

But there are many other reasons why we have to insure that our existing program remains strong. First of all, some of our current facilities are today optimally poised to explore some of the most important questions in the field. Second, the history of the field has over and over again demonstrated the importance of maintaining sufficient diversity in the available facilities. Different questions frequently require different lines of attack, and complementary approaches, based on different kinds of accelerators or colliders, are often necessary. Thus it is very likely that the presently operating facilities will still be relevant and essential in the SSC era. Finally, it is to the currently operating labs that one must look for the test beams, operating experience, and real experimental environment, all of which will be essential in designing, building, and testing SSC instrumentation.

But how do we deal with the unavoidable conflict between current programs and SSCL needs in terms of their demands on financial and personnel resources? It is the second of these that I am mainly concerned with here, because it is rapidly becoming clear that this may be the most critical issue in assuring timely SSC success. No easy solutions exist, but I believe that we must make our community more aware of the need to make judicious compromises here. One possible general guideline would be that we should not embark on any major new initiatives (in the experimental or accelerator area) if their scientific payoffs will not come until after the SSC startup and if they significantly affect potential SSCL personnel needs.

There is ample historical evidence documenting unique contributions of both fixed target and collider experiments. Even more dramatic is the record of the very fruitful complementarity of electron and hadron machines.

WE FINALLY COME to the third major challenge, namely defining the proper future of the four existing accelerator laboratories in the SSCL era. The role of the existing laboratories will change in the future, just as has always happened in the past whenever a new, higher energy facility became operational. Our challenge is to optimize this transition and to preserve the unique capabilities of the presently operating laboratories.

Even though SSC and its high energy *p-p* collider will be the cornerstone of the U.S. high energy program in the future, the need for diversity of the program requires strong complementary activities at other laboratories. There is ample historical evidence documenting unique contributions of both fixed target and collider experiments. Even more dramatic is the record of the very fruitful complementarity of electron and hadron machines. Over time the pendulum of success has swung from one type of machine to another. The late 50s and early 60s probably belonged to the hadron machines; then the electron machines came to the fore during the succeeding 10 years, to be somewhat overshadowed again by the hadron colliders in the 80s.

The present emphasis on hadron colliders is at least partly attributable to our current lack of technical capability to build an electron linac that can achieve the same energy scale as the SSC. However, recent advances in the technology of electron machines can provide important new tools in the electron sector through the means of high luminosity e^+e^- colliders optimized for the 10-GeV energy region (B factories). The physics goals of such a machine have been strongly endorsed by the recent HEPAP Subpanel on the U.S. High Energy Physics Research Program for the 1990s (Sciulli Subpanel) as an excellent way to study the question of CP violation, one of the most fundamental problems in particle physics today. Thus if resources could be found to build such a machine, the particle physics program would be greatly enriched.

TURNING NOW to a more specific discussion about the future of our four existing accelerator laboratories, one can be relatively sanguine about Brookhaven and Cornell. BNL's future direction is rather well-defined and envisages a gradual transition from its present principal emphasis of an experimental program based on the high-current capability of the AGS to a program based on the new Relativistic Heavy Ion Collider (RHIC), a machine that stands at the interface between the traditional fields of particle and nuclear physics.

The *B* physics program at Cornell, building on the present capabilities of CESR and the upgraded CLEO detector, and presumably strengthened in the future by additional machine improvements, will most likely provide exciting frontier physics for the rest of this decade. Even if a new B factory were to be built somewhere else in the world during the next several years, the 10 GeV e^+e^- region is rich enough so that there will be plenty of opportunities for exciting physics left to the Cornell machine. Of course, eventual upgrade of the present machine to a B factory is the most obvious step towards maintaining frontier capability of that laboratory into the next century. But whether or not this will come to pass, it seems likely that the ingenuity and inventiveness of the Cornell staff and their relatively small scale operation will somehow allow them to maintain an important niche in the U.S. HEP program.

The Fermilab and SLAC situations appear more difficult, both because of their single-purpose nature and because of their larger scale of operation. If the Main Injector is constructed at Fermilab, with a completion date around 1995, one can look forward to at least 10 more years of frontier physics at Fermilab based on gradual luminosity increase of the Tevatron; specialized fixed-target work based on 1-TeV protons; and high-intensity, medium-energy fixed-target work at the Main Injector. It seems that even in the SSC era Fermilab will have a unique niche, albeit somewhat reduced in scope, based on its multi-faceted fixed-target program and collider experimental capabilities that will probably offer more flexibility and a lower pressure environment than will be available at the SSCL. But there is no doubt that in parallel a significant fraction of the Fermilab effort will have to be directed towards SSCL experiments along the lines already initiated.

SLAC'S SITUATION appears to be significantly more difficult. There is no doubt that SLAC's past contributions to the field of high energy physics have been immense—not only in the key experimental breakthroughs that have shaped our current understanding of the basic constituents of matter and the forces that govern them, but also in the very important area of accelerator technology. And yet today the laboratory finds itself in a difficult situation. At least three major factors can be identified as contributing here: the community's commitment to the SSC, which results in pressures on the resources going to the existing laboratories and tends to hamper any major new initiatives; greater than anticipated difficulty of bringing on line the high-luminosity linear collider based on the retrofitted SLAC linac; and the major commitment by CERN to Z^0 physics and the resultant success of LEP and its associated detectors. Thus for the present, SLAC faces the challenge of generating sufficient frontier scientific activity at the laboratory. How to achieve this is the subject of intense discussion within SLAC, with the currently preferred solution relying on SLC and a B factory. For the future the main challenge is how to preserve the scientific vitality of the laboratory and the expertise that is located there, and how to ensure that there is an orderly progression towards true linear colliders, where SLAC can make major contributions and where it is bound to play a major role. Such machines will be essential if we are to maintain in the future the dual capability of experiments with both hadron and electron machines.

I would like to express next some personal views on one possible course for SLAC in the next decade. I use a decade as the time of reference because I doubt that a very large linear collider could be initiated much before then. When it happens, I have no doubt that SLAC will be a major player in that enterprise, regardless of where it is built. But it seems to me that until that time the natural focus for SLAC's main activities lies in three general areas: accelerator research based around the final focus test beam facility, exploitation of local facilities and work at other laboratories. The last two points need a few explanatory comments. The local facilities include: SLC, 50 GeV electron beams, possible restart of PEP, and *B* physics if a B factory is

built at SLAC. Even though the B factory, as discussed above, would address several key questions in HEP today, it would probably not have a great impact on physics in this decade because such a facility would start producing significant results only towards the end of this century. The extent to which SLC should dominate the local SLAC program will presumably be determined in connection with the project review in early 1992. Regarding the outside activities, the proper role of SLAC would be to participate in those efforts where the laboratory could provide unique expertise or facilities rather than act as a competitor to university groups. A major institutional involvement by SLAC in the SSCL experiments would undoubtedly help both laboratories in the long run. In addition, SLAC's expertise in many areas of accelerator technology, if applied to the SSC accelerator construction effort, would make an important contribution to the task of creating that new facility. Furthermore, some diversification of SLAC into areas outside of particle physics, but having some intellectual connection to it, might be a good thing for the overall long-term health of the laboratory.

I would like to end this essay by making some brief comments about the role of the U.S. High Energy community in the future evolution of our program and about the outlook for international collaboration in the future. The HEP community is viewed from the outside as being immensely successful in bringing their views to the attention of our policy makers and having them act frequently along the lines we desire. The initiation of the SSCL is pointed to as one major example of this pattern. HEPAP is viewed by outsiders as a major contributor to this success, a success that is mainly attributable to the fact that it allows us to speak with one voice vis à vis the outside world. I believe that our ability to unify after spirited internal discussions is very important and must be maintained if we are to be successful in the future.

The other point to make here has to do with our increased visibility on the national scene. As our field becomes more visible and as the funds allocated to it are viewed as competing with other discretionary expenditures, it is only natural that the public at large and its elected representatives should insist on understanding what they are getting for their money. Thus it will become even more important for members of our community to take time to explain to the nation at large the reasons why investment in basic research is important, and to repay the country for the support provided to our field. We have to do this not only by speaking regularly to our elected representatives but also by writing to local newspapers, by being willing to speak to interested civic groups, and perhaps most importantly by participating actively in improving science education in this country, and thus helping to improve the scientific literacy of our population.

TURNING TO the international scene, I see that some of the same forces and resultant tensions that are being experienced in our country exist also on the world scene. The trend towards higher energies inexorably brings increased costs per facility and hence greater centralization and fewer frontier laboratories. It would thus argue for closer collaboration and more cooperative ventures. The desire, however, to protect the investment in the existing labs is undoubtedly one of the centrifugal forces that acts against more collaborative planning. Of course our own system of year-to-year appropriations without firm long-range commitments is also a significant negative force in that direction. In addition, the scale of the resources required for meaningful collaborative efforts is such that government bodies at the highest level will have to be thoroughly involved. All of these are factors that tend to dampen any optimism that I might have about a significantly higher level of international collaboration in the future.

In conclusion, let me stress once again that the above views about the future represent my own crystal- (or cloudy-) ball gazing and should not be interpreted as official views of HEPAP, DOE, NSF, or any laboratory. But these issues will and should be discussed in the future by HEPAP, and thus their serious consideration at this time by the community as a whole would be quite useful. The future of our field requires that we come up with thoughtful and optimized solutions.

EGS
A Technology Spinoff to Medicine

by RALPH NELSON and ALEX BIELAJEW

The EGS code has been adopted by medical physicists who are making improvements that are important to high-energy physics.

OVER THE YEARS, medicine has been the beneficiary of many technologies derived from high-energy physics. Electron accelerators, for example, are now routinely employed in external-beam radiotherapy. Particle detectors have spawned ideas for Positron Emission Tomography (PET) and Computed Axial Tomography (CAT). Theories of electron multiple scattering and slowing-down are increasingly used in present-day treatment-planning algorithms to calculate the amount of energy deposited in the human body during the course of radiotherapy.

The EGS4 computer program, developed originally for detector design and shielding analysis, is yet another medical application derived from high-energy physics. It has been widely adopted by medical physicists who not only are sophisticated users but also provide feedback for further developments of the code.

Recent collaboration between the University of Wisconsin and the NRCC, called the Omega Project, has been funded by the National Institute of Health to deploy Monte Carlo-based electron treatment planning in a working cancer clinic.

EGS, WHICH STANDS FOR Electron-Gamma Shower, is a general-purpose package for the Monte Carlo simulation of the coupled transport of electrons and photons in an arbitrary geometry; it can be used for particles with energies above a few keV up to several TeV. Stated more simply, EGS provides a way of calculating the flow of radiation energy carried by electrons and photons as they travel randomly ("walk") through matter. Although the individual interactions made by the particles are fundamental and well understood, the transport process taken as a whole leads to a mathematical problem prohibitively difficult to solve in a way that is of sufficient generality to be useful. But the problem can be solved fairly easily on a computer using statistical game-playing techniques (hence the name *Monte Carlo)*, which is what the EGS Code System accomplishes. The "seed" for EGS was a computer program brought to SLAC by Hans-Hellmut Nagel of Bonn University around 1965. There were several other programs receiving wide attention during the early 1960s, one of which was developed at the Oak Ridge National Laboratory for use in the design of beam stoppers, collimators, and targets at SLAC. Although the results from the Oak Ridge program were extremely useful during the construction of the Two-Mile Accelerator and its beam lines, the computer code itself was never released to the scientific community. Consequently this program, like many others of its genre, eventually faded from the scene.

The EGS Code System, on the other hand, has become a standard tool in the design of calorimetry systems in high-energy physics, as well as a benchmarking tool in radiation dosimetry (i.e., a standard by which other calculations are measured). This success can be attributed directly to the continuous efforts and support provided by SLAC, its collaborators, and a user community that is well over 1000 strong.

It was clear from the beginning that Nagel's program was simply too restrictive to be useful in solving current problems of interest to high-energy physics. The code needed to be completely rewritten in order to achieve the generalization required. Working independently at first, Ralph Nelson at SLAC and Richard Ford at the Hanson High Energy Physics Laboratory on the Stanford campus decided to combine their programming efforts and produced the first version of EGS in 1978.

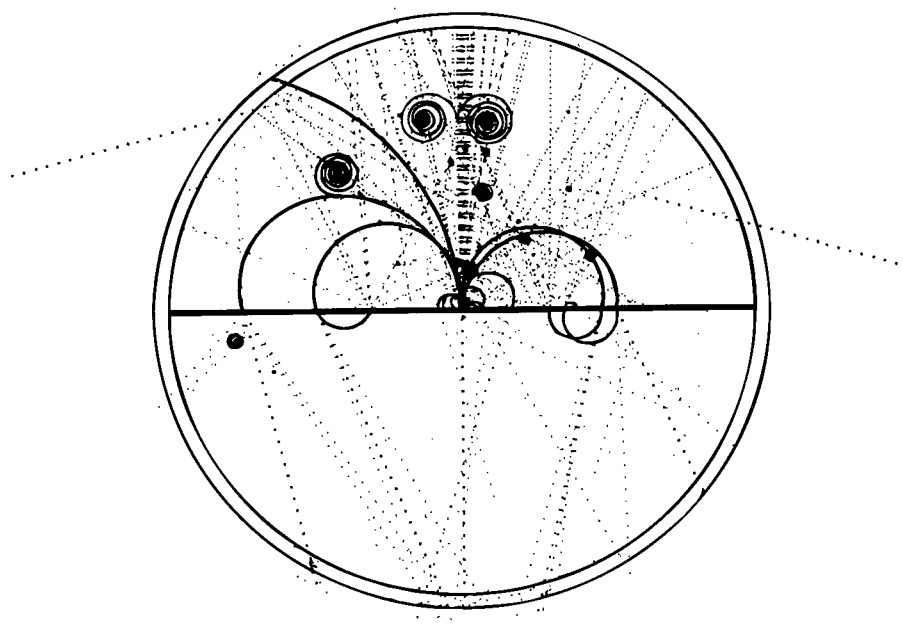
THE EGS3 CODE SYSTEM, as it was known at that time, was designed to simulate the flow of electrons and photons in arbitrary geometries at energies ranging from well below an MeV to several thousand GeV. A vast bookkeeping effort is accomplished by the program as it keeps track of the variety of interactions and steps involved in the radiation-transport process. As demonstrated in the picture on the facing page, EGS allows one to visualize the myriad particle tracks produced by an electromagnetic cascade shower.

This example, a color-graphic portion of which is shown on the cover, represents an EGS simulation of a photon-initiated shower developed within a 40-inch bubble chamber similar to the one used at SLAC during the 1970s.

But it takes more than pretty pictures to make a computer program credible. The documentation released with EGS3 in 1978 contained important comparisons with benchmark experiments, both at high and low energies. The most important tests, however, were performed by the growing number of EGS users themselves.

Because EGS was well documented, as well as *versatile, user-friendly,* and *upward-compatible*—buzz words common in the lexicon of today's software industry—a large user community soon developed. The fact that anyone could get the EGS Code System free, together with support for its use, was a significant factor in its growing popularity.

In retrospect, probably the single most important event that made EGS an everyday word in high-energy physics was the discovery of the J/ψ particle in the fall of 1974. This discovery led to a dramatic increase in the use of storage rings and a need for sophisticated calorimetry surrounding the interaction regions of these beams. EGS has played an important role in the design of many, if not most, of the electromagnetic shower counters built since then.



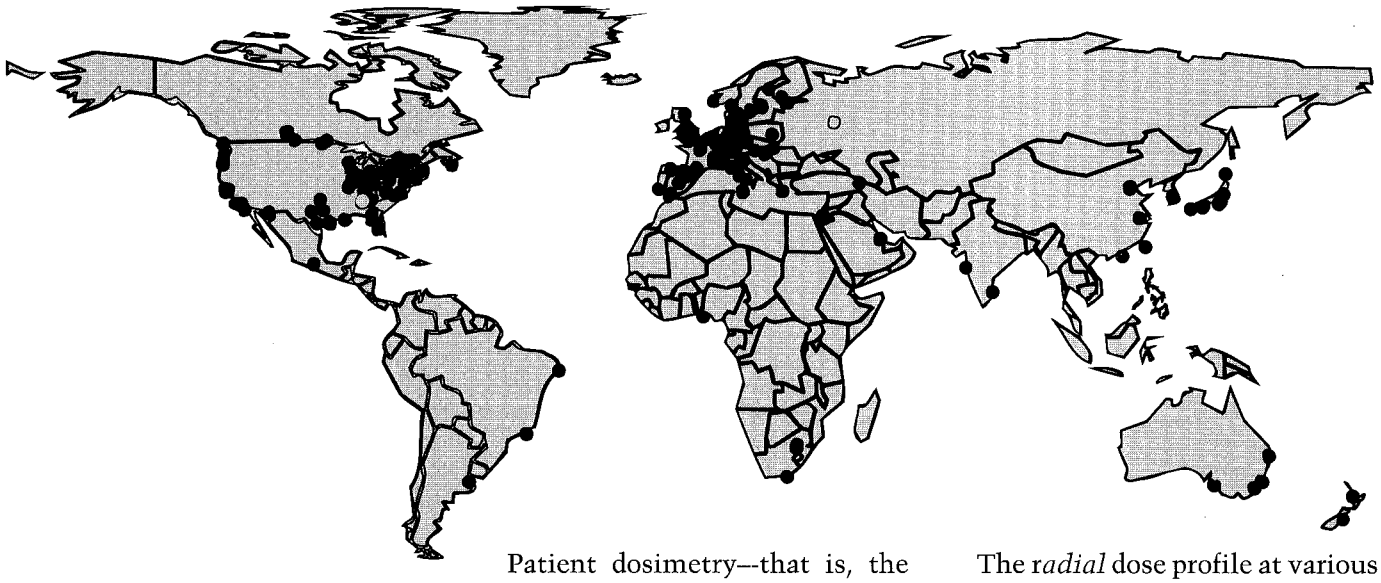
EGS simulation of a hydrogen bubble chamber. A single 1-GeV photon strikes a 3-mm lead slab from the bottom and produces radiation. Positrons and electrons (solid lines) bend in a 10-kG magnetic field, whereas photons (dots) do not. A close-up color rendition of this same event is shown on the cover.

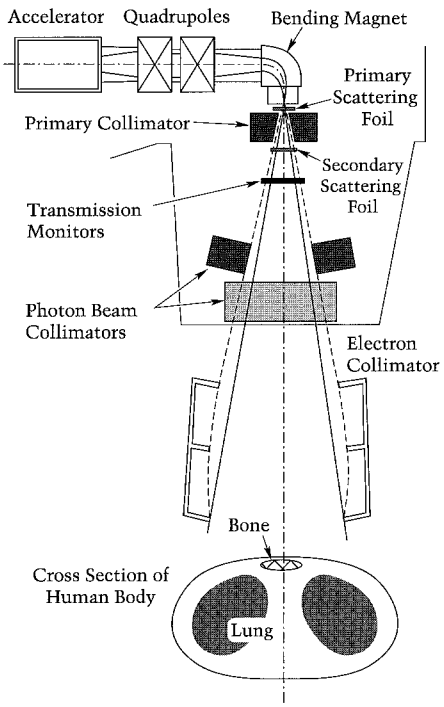
HOW HAS EGS been influenced by medical physics? In the latter part of 1982 SLAC's Radiation Physics group, together with its counterpart at KEK in Japan, started a collaboration to extend the flexibility of EGS in a general way, but with a specific class of problems in mind—those involving the design of future high-energy accelerators. From the beginning, however, there was also a growing awareness of the need to extend EGS downwards to much lower energies. The program was becoming increasingly popular as a low-energy tool in a variety of problems outside the field of high-energy physics, and various people had mentioned problems and limitations in the existing code. It was anticipated that these low-energy effects would eventually show up in designs of future detectors for high-energy physics.

Serendipitously, a detailed low-energy benchmarking effort was being conducted at this time by a group of EGS users at the National Research Council of Canada (NRCC) in Ottawa. Initiated by David W.O. Rogers, the NRCC work involved adopting EGS for use as a theoretical tool in ionizing radiation standards work to serve the low-energy radiation protection community (e.g., diagnostic x rays) as well as the radiotherapy community (energies less than 50 MeV). There was also strong need in the medical physics community for theoretical tools with good predictive capability, given that the major mode of radio-therapy treatment was changing from relatively low-energy Cobalt-60 gamma rays (about 1.25 MeV) to radiation produced by higher energy electron linear accelerators (4–50 MeV). This conversion had as much impact on the use of EGS as the J/ψ discovery in high-energy physics. Analytical tools dominant at Cobalt-60 energies were being rendered ineffective at higher energies because of the complications of electron transport.

With its elegant treatment of electron transport, EGS was ideally suited to these problems. Hence, a collaboration was formed between SLAC, KEK, and NRCC groups resulting in the EGS4 Code System, which was introduced in the fall of 1985. Since then, the interest shown in the program by medical physicists has been overwhelming. As the map indicates, this interest is worldwide.

TODAY OVER 60 PERCENT of the requests for EGS come from scientists working in medically related disciplines. Based on current trends, roughly one in eight of us will find ourselves undergoing radiotherapy during our lifetime, barring miracle cures for cancer. A complete geometry for electron accelerator treatment is shown in the following sketch, which serves to illustrate the extraordinary complexity of the clinical situation.





Top: EGS user sites throughout the world. Bottom: A typical geometry for radiotherapy treatment.

Patient dosimetry—that is, the accurate determination of the radiation dose given during a treatment procedure—must account for scattering from components inside the machine as well as structures within the human body itself. Bones and lungs, for example, produce "interface effects" because of differences in the transport of electrons set in motion by photons in adjacent material. An experiment was performed to determine how accurately EGS could model features within the human body. Using a 20-MeV accelerator, scientists at the NRCC placed small cylinders of aluminum and air within a large tank of water and irradiated them with electrons, as shown on the facing page.

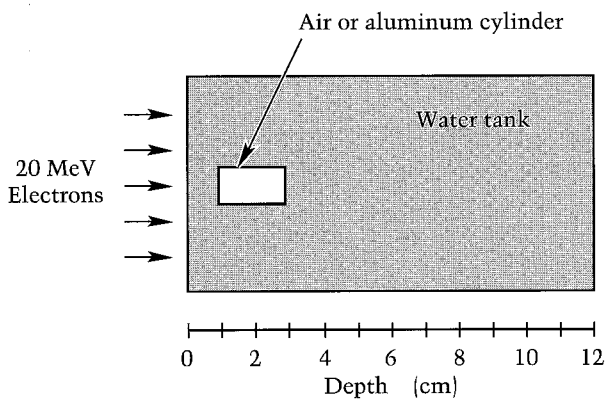
Measurements were made at various locations in the water, particularly near the surface of the cylinder, and computer simulations were performed using EGS. In the results presented in the figures, the smooth curves represent the measured data and the histograms are EGS calculations.

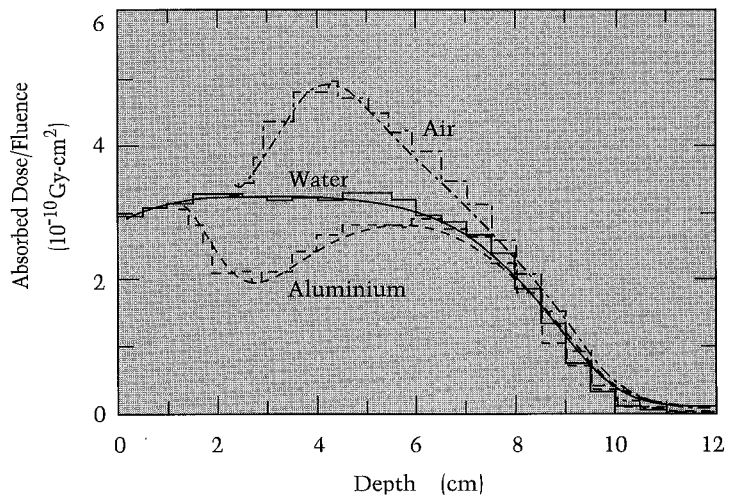
A typical depth-dose curve in a homogeneous "phantom" of water—i.e., containing no voids or solid materials—is shown in the top right figure on page 11. The other two curves demonstrate how an aluminum cylinder attenuates, and an air cylinder enhances, the dose along the central axis within the phantom.

The *radial* dose profile at various locations downbeam from the air cylinder was also measured and the results are shown in the final set of figures. Clearly the dose perturbations caused by discontinuities are well predicted by EGS, lending considerable confidence to the ability of the program to simulate the passage of electrons through the human body.

SINCE 1985, the new low-energy focus has led to improvements to EGS that directly benefit the high-energy community. Calorimetry, for example, requires measuring electron-photon showers until the particles are greatly degraded in energy. By means of an EGS option called PRESTA, developed recently for low-energy dosimetry research, electron transport can now be simulated very accurately in the design of calorimeters and other detectors.

Feedback from medical physics has also come in the form of basic instruction. Medical physicists have so far organized five "hands-on" teaching courses on EGS4. Complete with computer laboratories, these four-day courses attract not only medical physicists but also students from high-energy physics, the nuclear power industry and military research. Efforts along these lines have led recently to the publication of a book titled *Monte Carlo Transport of*

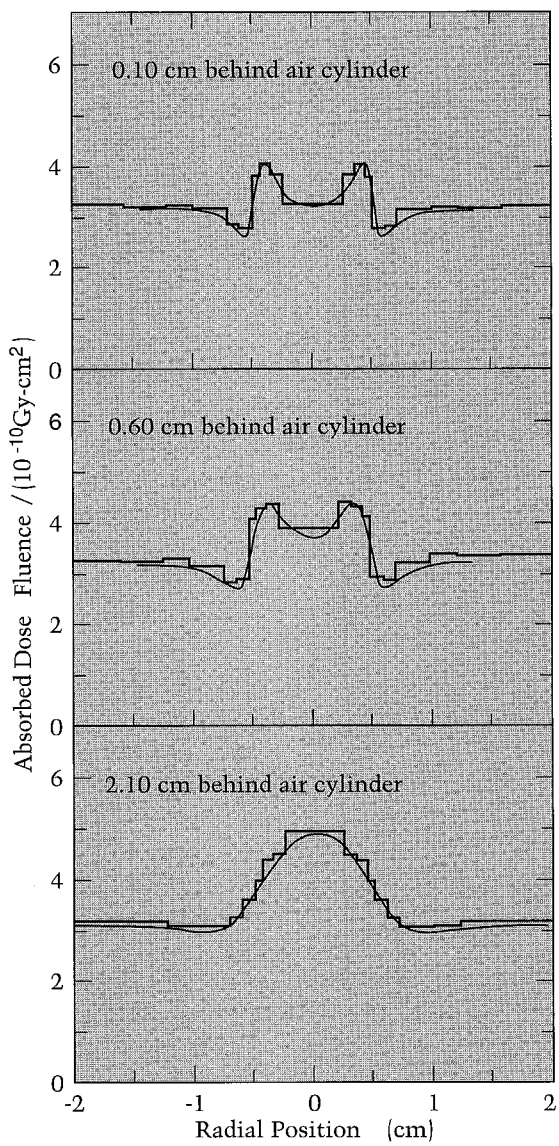




Electrons and Photons (reviewed in the Spring 1990 issue of the *Beam Line*). Also, the new reference standard, *The Dosimetry of Ionizing Radiation,* devotes almost two hundred pages to EGS-related calculations and discussion.

WHAT ABOUT THE FUTURE OF EGS? Future releases of the code will reflect more strongly the newly formed symbiotic relationship between high-energy physics and medical physics. In addition, there are many interesting applications in cosmic-ray physics, space science, nuclear power, radiation processing, and even such a diverse field as the paper industry. Low-energy electron beams can be used as quality assurance tools in the measurement of paper thickness!

In 10–15 years, based upon current initiatives, it is likely that most external-beam radiotherapy planning will be accomplished using EGS Monte Carlo methods. With computers rapidly becoming faster and cheaper, the Monte Carlo technique, popularized by von Neumann, Ulam, and Fermi in the 1940s, is no longer the tool of a select few with access to high-power supercomputers. EGS has become the standard tool for the hospital physicist; it has been run on all computer architectures, from PC's to Cray's. The number of Monte Carlo papers in the journals *Medical Physics* and *Physics in Medicine and Biology* increased fivefold from 1983 to 1988, and this trend continues today. EGS is expected to stand at the forefront of this surge.



Top left: Schematic diagram of an experiment designed to verify the EGS code for medical physics applications. Top right: Dose vs. depth within a water phantom. Bottom: Radial dose profiles down beam of the air cylinder.

Accelerator Structure Development

GREGORY LOEW, HARRY HOAG, AND JUWEN WANG

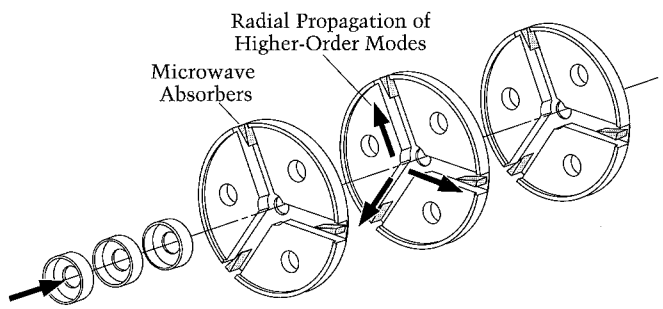
THE NEXT LINEAR COLLIDER (NLC) will probably consist of two seven-kilometer long tunnels pointing at each other: one will house the electron linac, the other the positron linac. Of these fourteen kilometers, about twelve will be occupied by the microwave structures that accelerate the beams. *What kind of accelerator structures will we need for the NLC?*

The accelerator structure is the heart of the entire linear collider. It must be efficient, reliable, easy to align, pump and cool. It must accelerate not just one but a train of high current bunches in each pulse to meet the luminosity requirements of the collider. It must do this while preserving the tiny size and energy spread of the beams as they are injected from special damping rings. Operating at high field gradients, probably between 50 and 100 MV/m, it must not generate parasitic electrons (called "dark current") produced by field emission, which can absorb energy, get accelerated and produce detrimental x-ray radiation and undesirable steering effects. Finally, the structure must not be too expensive to fabricate and install.

Building on a long tradition of work on linac structures at SLAC, much progress has been made toward these goals. Considerable advances have taken place by the use of specialized computer codes for structure design and for studies of the effects of higher-order modes on the bunches as they are accelerated. Experimental work has included studies of field emission and rf breakdown at high gradients, use of materials other than pure copper, design of prototype structures and future fabrication techniques.

The choice of frequency has been moved upwards above SLAC's frequency of 2.856 GHz in order to achieve the required energy in a machine of reasonable peak rf power, stored energy and length. An upper bound to the operating frequency is imposed by the minimum size of an accelerating structure that can be built to the necessary cross-sectional tolerances, by the iris hole diameter which, as it decreases, rapidly increases the deflecting fields left behind a bunch, and by the klystron amplifiers which can be built to supply the driving power (see Fall/Winter 1990 issue of the *Beam Line*). As a practical compromise, a frequency of 11.424 GHz (four times SLAC) has been chosen.

The new structure, however, cannot be a simple scaled-down version of the SLAC disk-loaded waveguide. This is because the very high-intensity electron and positron bunches leave behind energy in the long-range higher-order mode (HOM) wakefields, which can cause unacceptable energy spread and bunch-to-bunch misalignment.



Shown schematically at the left is a conventional disk-loaded waveguide accelerator structure made of stacked cups. In such a structure the beam and the fundamental accelerating wave propagate together along the axis, but higher order modes are trapped and disrupt the beam. At the right is one version of a new accelerator structure in which the higher order modes are allowed to escape through short radial waveguides and are absorbed in microwave terminations, while the beam and the fundamental mode propagate along the axis as before.

TWO WAYS OF STOPPING the multibunch interaction are being investigated. One, in line with suggestions by Bob Palmer, is to provide channels for the power in these modes to propagate radially outwards, thus lowering the field levels on the beam axis below the tolerable levels. Several methods of incorporating these channels have been investigated. One method was referred to in Ron Ruth's article, "The Next Linear Collider," which appeared in the Summer 1990 issue of the *Beam Line*. A related approach, which is currently being examined because it looks simpler to build, is illustrated in the drawing above. The left-hand side of this figure shows an exploded view of the simple disk-loaded waveguide in which the beam-induced wakefields are trapped and left to damage the beam properties. The right-hand side shows the new structure, in which three rectangular waveguides diverge symmetrically and radially from each central accelerator cavity. The size of the waveguides is carefully chosen so that the fundamental travelling mode which accelerates the beam (and carries up to hundreds of megawatts of peak rf power) cannot escape. It "thinks" the structure is the same as the one on the left of the figure. However, all the HOMs have higher frequencies and can propagate out of the radial waveguides, where they are absorbed in microwave terminations. Experimental tests at SLAC have shown that structures which rapidly damp the undesirable modes can indeed be designed and constructed.

A second way of dealing with the HOMs is to progressively alter the geometry of successive cells in such a way that the resonant frequencies of the HOMs are changed without altering the propagation characteristics of the fundamental accelerating mode. Then, the HOM waves "decohere" (i.e., they become all mixed up), and cumulative interaction with the beam can be minimized. An experiment to test this idea has recently been carried out with the help of Jim Simpson at the Argonne National Laboratory, using two structures assembled at SLAC. In the Argonne facility, two

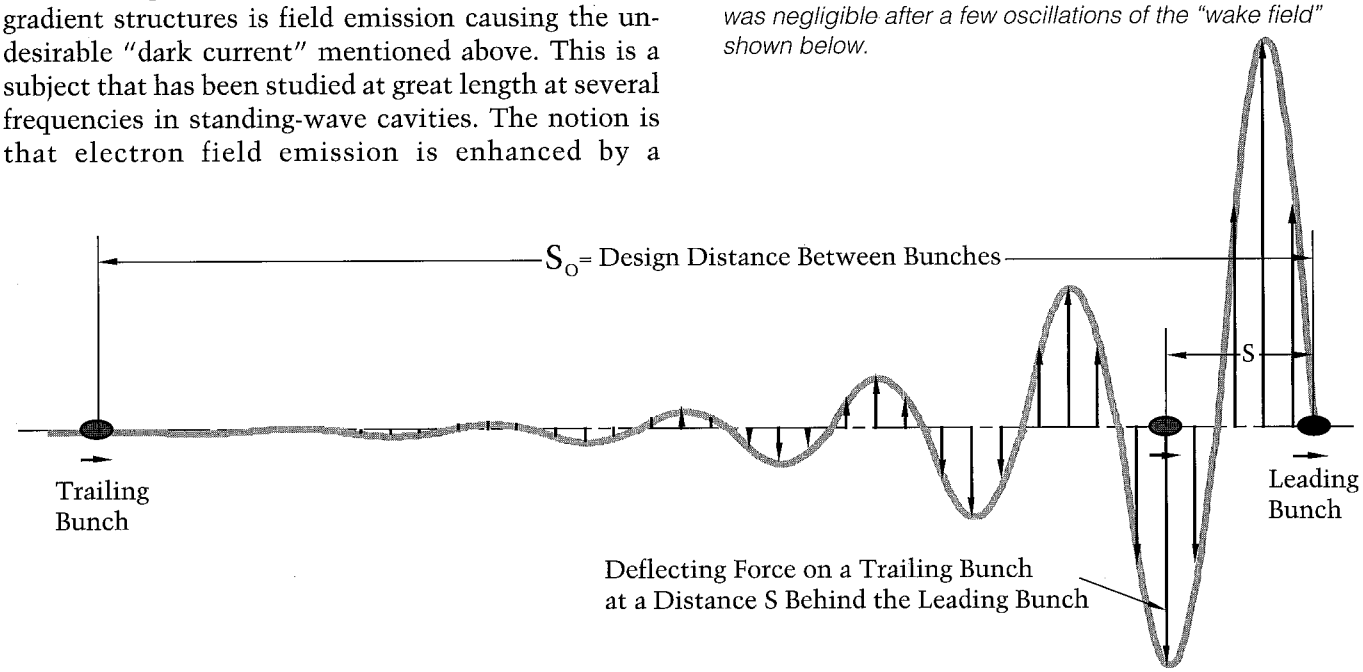
electron bunches, a driving bunch and a witness bunch trailing behind at adjustable distance (up to about 30 cm), can be made to traverse a test structure. The facility is equipped with a spectrometer that can measure the longitudinal and transverse forces created by the driving bunch on the witness bunch. The effect is illustrated below. The measurement showed a rapid decrease of the forces behind the leading bunch for the "detuned" structure tested and was in good agreement with theoretical predictions.

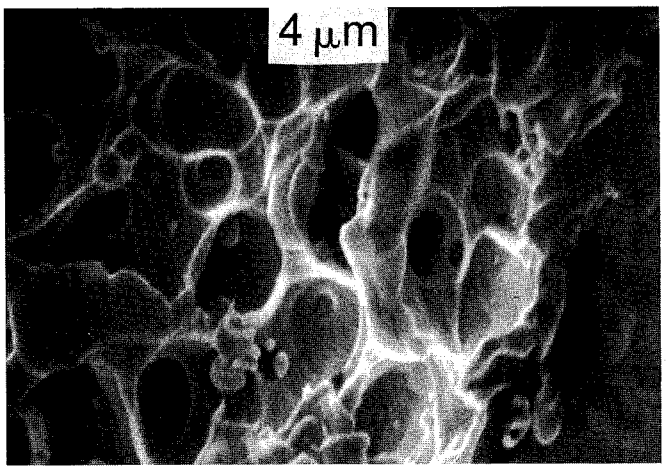
One concern arising from the work of Kathy Thompson at SLAC is that the waves may "recohere" at large distances behind the leading bunch and still cause beam disruption. Thus, the first method of HOM damping cannot be entirely discarded, and there are advantages to judiciously combining the two methods. For example, an adequate solution may be to build a structure in which the dipole mode frequency is detuned by 10% over the length of the structure and its quality factor Q is loaded down to between 20 and 100.

Another problem that must be considered in high-gradient structures is field emission causing the undesirable "dark current" mentioned above. This is a subject that has been studied at great length at several frequencies in standing-wave cavities. The notion is that electron field emission is enhanced by a combination of metallic protrusions (mountains) and dielectric impurities partially covering these mountains (snow). RF processing can gradually remove some of this "snow" but, when pushed to surface fields exceeding 300 MV/m, can cause a mountain to erupt into a volcanic flare, vaporizing the metal and leaving behind it a broken-down landscape of molten copper and "lunar craters," as shown in the photograph at the top of the next page. Interestingly enough, it appears that the presence of these craters does not seem to be detrimental to the operation of the structure at somewhat lower fields. Earl Hoyt and others at SLAC are helping to analyze the surfaces before and after high-gradient operation.

The drawings on the right-hand side of the next page show two experimental cavities that are presently being used to study these effects. The top one is a

The force exerted by a leading bunch on a trailing bunch is indicated by the length of the vertical arrows. In the Argonne National Laboratory experiment, the force on a trailing bunch was negligible after a few oscillations of the "wake field" shown below.

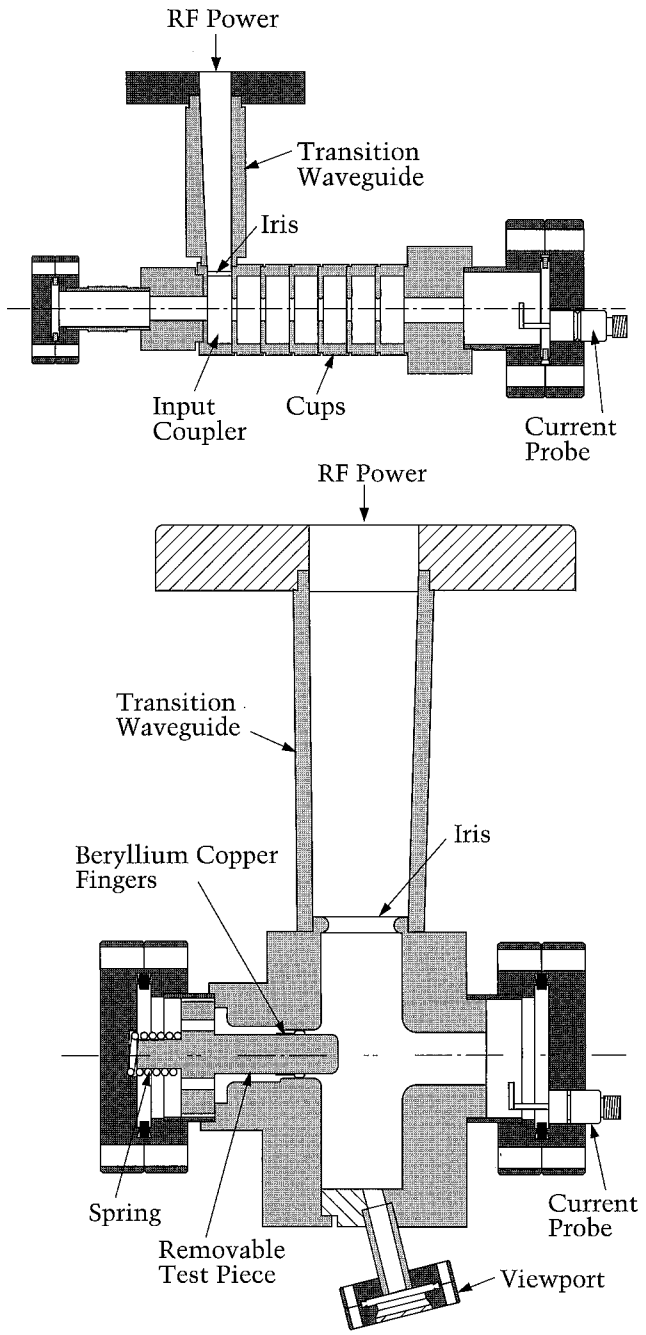




An electron microscope "photograph" of damage caused by surface fields in excess of 300 million volts per meter. The craters are a few microns across. The planned acceleration field for an NLC is well below the value that is expected to cause this type of damage.

seven-cavity X-band structure that will be tested for field emission, using a new high power klystron. The bottom one is a demountable S-band cavity designed to examine various geometries and metallic surfaces.

ONE OF THE GREAT CHALLENGES ahead is to determine how to fabricate the future structures, once the design is optimized. The total structure length is estimated to be about 12 km. The length of each cavity has to be one-third of a wavelength at 11.424 GHz, or 0.875 cm. This means that we need about 1.4 million cavities! If we want to avoid tuning every individual cavity, the critical dimensions have to be held to within about one micron. Cavities such as those shown on page 13 must be aligned, water-cooled and supported in an outer vacuum envelope. A lot of fun and hard work to make these structures affordable lies ahead!



The first structure shown in the upper right is a 7-cavity X-band test structure. In the lower right is an S-band demountable test cavity.

LETTERS TO THE EDITORS

Dear SLAC Beam Line Editors:

I very much appreciated your article by Rocky Kolb, "Particle Astrophysics and the Origin of Structure." I found it a very coherent and good review. I was pleased to see the COBE data in the article and proud to see the COBE DMR map in color on the cover. I was disappointed that the article did not mention that the map was produced by a team headed by my group at LBL. COBE is a collaboration of about 20 scientists; however, the leadership and prototype instruments for mapping the Cosmic Microwave Background Radiation (CMBR) were produced by Lawrence Berkeley Laboratory. More than a decade ago we realized that the large angular scale CMB anisotropy provided information on physics at very high energies. I think as Kolb does that the primordial fluctuations were created by quantum fluctuations during the inflationary epoch at an energy of about 10^{15} to 10^{16} GeV and remain as perturbations in the CMBR isotropy. Measurement of the fluctuation spectrum will provide exciting new information joining particle physics and cosmology together at birth.

George Smoot*
COBE DMR Principal Investigator

From the Editors:

We are very pleased to acknowledge here the seminal contributions of George Smoot, his colleagues at the Lawrence Berkeley Laboratory, and the other members of the COBE collaboration. We were also reminded in another letter from Bob Birge of LBL that "Earlier similar pictures were produced from U-2 flights some years ago (again funded by LBL) when the asymmetry was first discovered." The significance of these pioneering studies was recently recognized in the award to George Smoot of the NASA Medal for Exceptional Scientific Achievement (see below).

We welcome letters to the Editors about the articles that we publish. We also renew our invitation to prospective writers to contact us about possible articles in particle physics or related (even marginally related) fields.

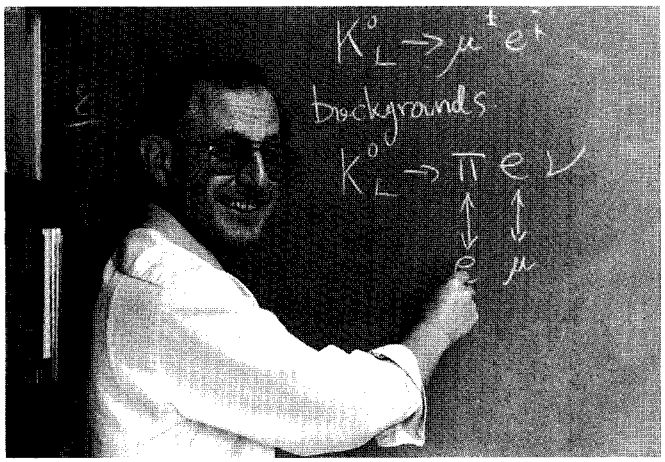
Rene Donaldson
Bill Kirk

**Editors' Note: George Smoot of the Astrophysics Group at Lawrence Berkeley Laboratory was recently awarded the NASA Medal for Exceptional Scientific Achievement in a ceremony at Goddard Space Flight Center in Greenbelt, Maryland, "in recognition of his outstanding scientific achievements in observational cosmology through conception and leadership on the Cosmic Background Explorer (COBE) Mission."*

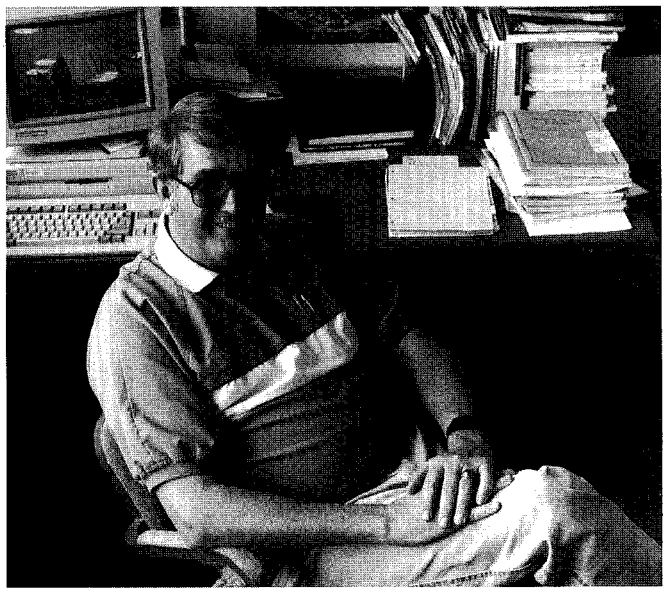
DATES TO REMEMBER

Jun 10–14	Workshop on the Design of a Detector for a High-Luminosity Asymmetric B Factory: Summer Session at SLAC, Palo Alto, CA (Anamaria Pacheco, SLAC, Bin 95, P. O. Box 4349, Stanford, CA 94309, BITNET ANAMARIA@SLACVM).
Jun 17–Aug 9	Summer School in High Energy Physics and Cosmology, Trieste, Italy (ICTP, P. O. Box 586, I-34100 Trieste, Italy, BITNET VARNIER@VX1CP2.INFN.IT).
Jul 25–Aug 1	15th International Lepton Photon Symposium at High Energies, Geneva, Switzerland (by invitation)(L. Griffiths, LP-HEP91 Conference Secretariat, CERN, 1211 Geneva 23, Switzerland).
Aug 5–16	SLAC Summer Institute on Particle Physics, Lepton-Hadron Scattering, Palo Alto, California [Jane Hawthorne, SLAC, Bin 62, P. O. Box 4349, Stanford, CA 94309, BITNET SSI@SLACVM or (415) 926-2877].
Aug 18–22	Particles and Fields '91, American Physical Society Division of Particles and Fields and Division of Particle Physics, Canadian Association of Physicists, Vancouver, Canada [PF91 Secretariat, TRIUMF, 4004 Wesbrook Mall, Vancouver, BC, Canada V6T 2A3, (604) 222-1047, BITNET PF91@TRIUMFCL].
Aug 23–Sep 2	1991 CERN School of Computing, Ystad, Sweden. (Ingrid Barnett, CERN CH Division, 1211 Geneva 23, Switzerland, BITNET BARNETT@CERNVM).
Aug 27–30	International Conference on Neutron Scattering, Oxford, England. (ICNS '91 Secretariat, Rutherford Appleton Laboratory, Chilton, Didcot, Oxon OX11 0QX, England)
Sep 16–27	CERN Accelerator School: Advanced Accelerator Physics, Noordwijkerhout, Netherlands (S. von Wartburg, CERN Accelerator School, SL Division, 1211 Geneva 23, Switzerland BITNET CASNIK@CERNVM).

CONTRIBUTORS



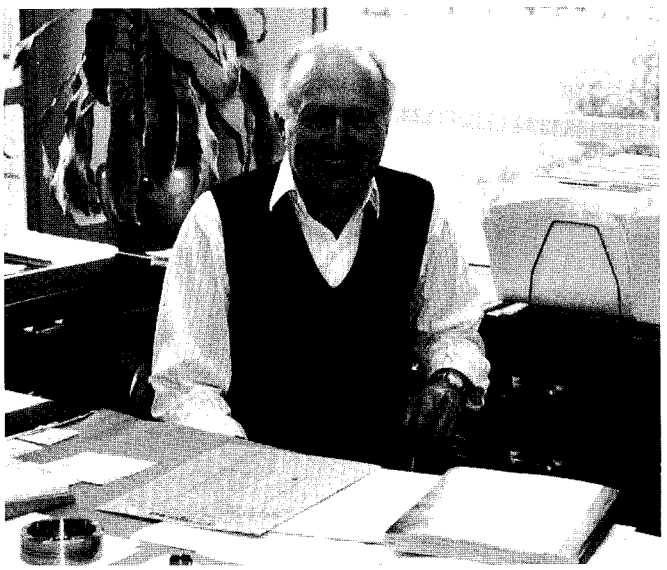
STANLEY G. WOJCICKI is chairman of the High Energy Physics Advisory Panel (HEPAP) and a member of the Executive Board of the American Physical Society. He chaired the HEPAP Subpanel in 1983 that recommended initiation of the Superconducting Super Collider (SSC) project and subsequently served for four years as Deputy Director of the SSC Central Design Group, the organization responsible for initial design and cost estimates for the SSC. He came to Stanford University in 1966 and was honored with the Dean's Award for Distinguished Teaching in 1979. His field of research is elementary particle physics, and at the present time he is working on an experiment to search for rare *K* decays at Brookhaven National Laboratory.



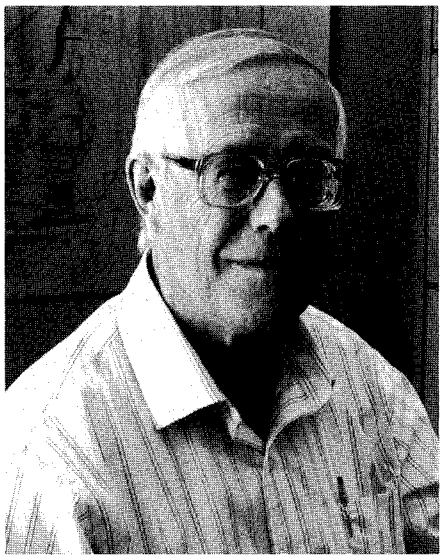
RALPH NELSON joined SLAC in 1964 as one of the original members of the Radiation Physics Group. His interests have always revolved around the science of radiation transport, of which the EGS computer code is but one part. His work ranges from electron-photon dosimetry (he co-authored a book on the subject in 1974) to the theory and measurement of muon shielding. He has been a consultant to numerous organizations including the SSC Laboratory, Varian Associates, and Sincrotrone Trieste. He is an Adjunct Professor with the Nuclear Science Facility at San Jose State University where he teaches shielding and dosimetry to both medical and health physicists.

Alex Bielajew is a research scientist with the Institute for National Measurement Standards at the National Research Council of Canada. Since graduating from Stanford University in 1982 he has pioneered the use of Monte Carlo techniques as a theoretical tool in ionizing radiation dosimetry and developed theory related to radiation standards. Concentrating his efforts at improving accuracy for low energy electron transport, e.g., the PRESTA algorithm, Alex devotes considerable time to lecturing and organizing courses extolling the virtues of EGS, supporting the burgeoning UNIX-based EGS community, and serving as a center of software distribution and expertise.

Gregory A. Loew is Deputy Director of SLAC's Technical Division and a member of the SLAC Faculty. In 1958 he joined Project M which was later to become SLAC. Starting in the 1960s he became Head of the Accelerator Physics Department, a job which he held for about 20 years before assuming his present position. His first assignment when hired was to design the constant-gradient structure for the two-mile accelerator. Throughout the years, he has maintained an active interest in the field of linac structures, the subject of his article in this issue of the *Beam Line*.



Howard Martin



HARRY HOAG is a physicist-turned-microwave engineer who came to SLAC in 1964 to work on phasing systems, beam position monitors, and other microwave devices used on the linear accelerator. He was a contributor to and an editor of the SLAC "Blue Book." Over the years he has worked on many projects, including rf superconductivity, SLED, and the development of very sensitive beam position monitors which were a key part of polarized electron scattering experiments. At present, he is involved in the development of new structures for the NLC and is also working on a number of microwave projects connected with SLC.

Karen Fant

JUWEN WANG, physicist in the Accelerator Theory and Special Projects Department, came to SLAC in 1980 as a visiting scientist. He has a long history of expertise and proficiency in the field of linear accelerators that dates back to the 1960s when he was trained at Tsing hua University and subsequently at Stanford University. He has actively participated in the research of accelerating structures for future linear colliders. His activities span theoretical calculations of rf parameters, wakefields, and beam loading as well as the mechanical design, microwave measurement, and high-power testing of various linac structures.

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