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Cover: Computer simulation of the pair production of the tau sneutrino, the supersymmetric partner of the tau neutrino, at 500 GeV center-of-mass energy in a future electron-positron linear collider (see the article by John Ellis on page 14).

(Courtesy Richard Dubois, SLAC)

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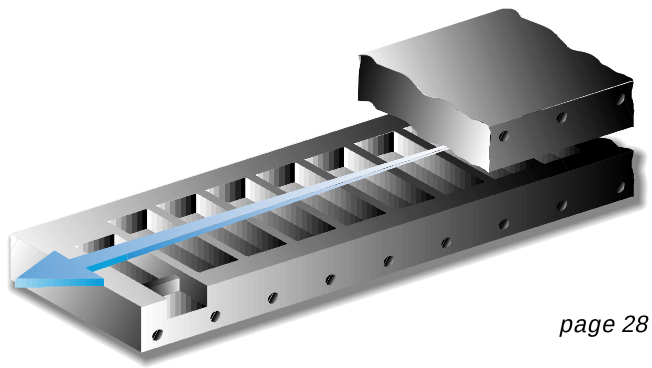

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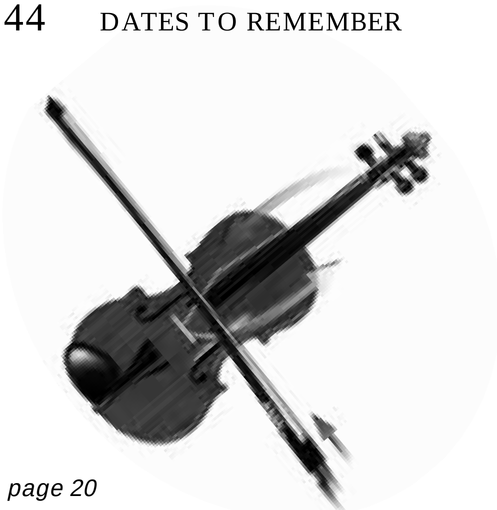

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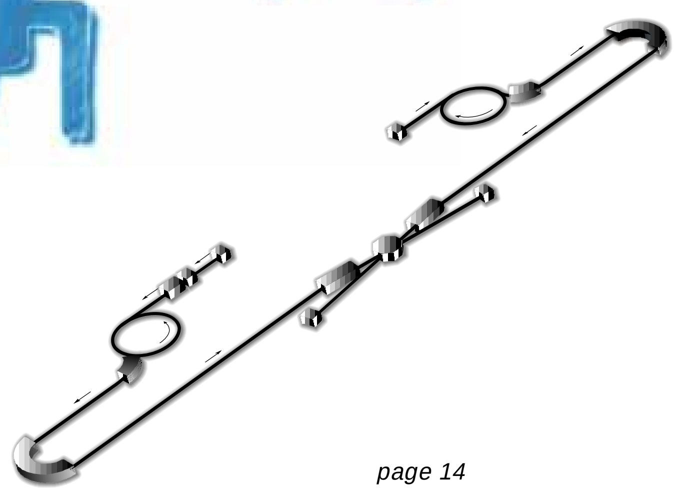

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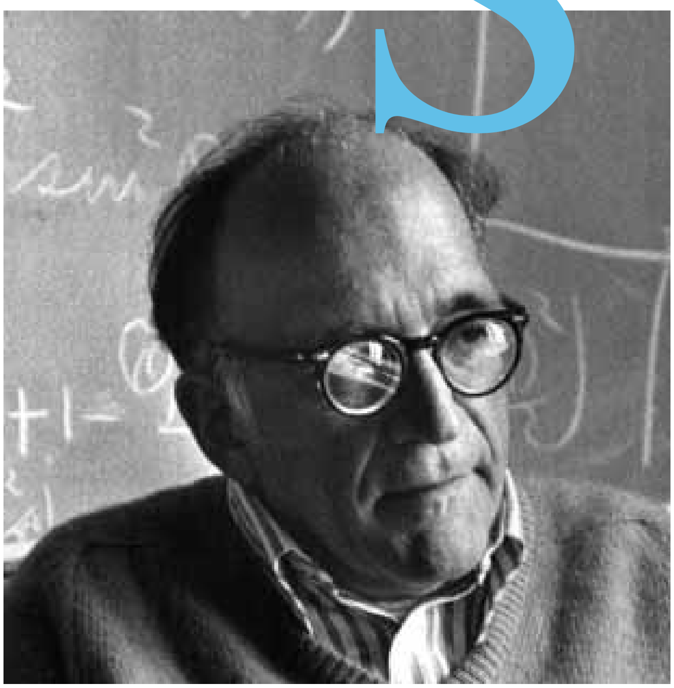
FOREWORD



SIDNEY DRELL and SLAC have histories which are inseparable. Right from the beginning, when SLAC was no more than Project M housed in a warehouse on the Stanford campus, Sid left the more familiar and comfortable world of academe to join the adventure of creating a great new laboratory. A major challenge he faced, as a theorist, was to create the kind of intellectual climate more typically found in university departments, and in those days not commonly found within this country's accelerator laboratories. This turned into an incredible success story. Quickly the SLAC theory group became very well known, not only for the variety of talent it attracted, but especially for its unique personality. To this day this personality persists: uncompromisingly high intellectual standards, a good mix of applied and formal theory, a close interaction with the experimental community, a breadth of vision, an essential humanism, and an informality and lack of pretension which keeps the pursuit of physics something not only deeply satisfying but also just plain fun to do. It is no accident that this "SLAC style" is a mirror of Sid's own persona. Generations of students and postdocs who have passed through the laboratory—such as John Ellis of CERN who discusses future options for particle physics in this issue—as well as we old-timers, will attest to his overwhelming influence in creating this environment.

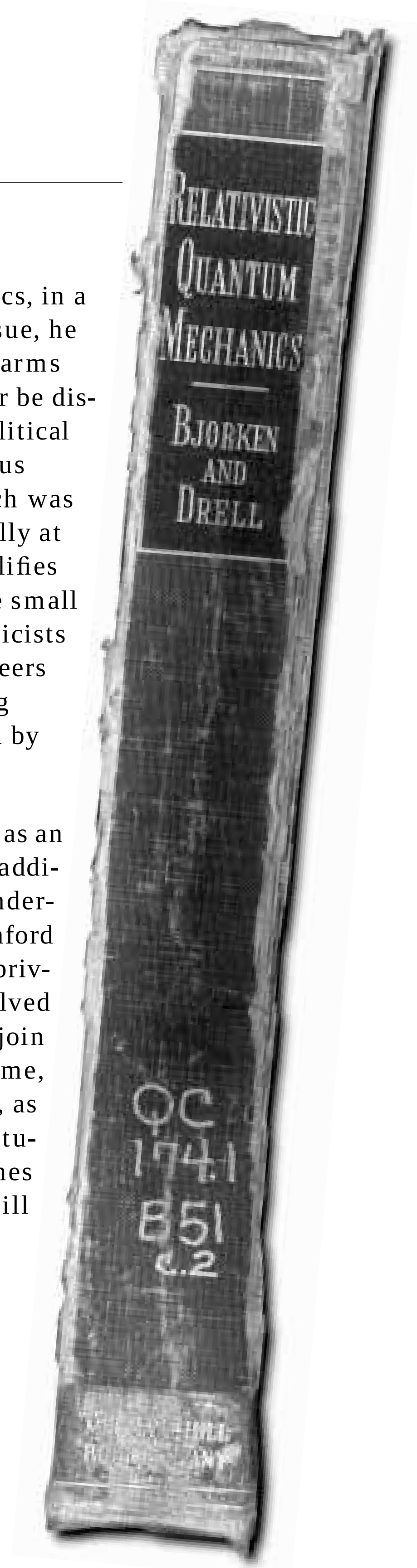
Sid's research contributions, within and beyond the SLAC program, have been legion and broad-ranging, of fundamental importance. And his impact on SLAC has extended well beyond that of theoretical physics. From the beginning of SLAC he was invaluable in the building of the laboratory, its staff, and its program. He has, as deputy director, played a quiet, but vital role in the overall management and orchestration of the SLAC program. His calm guidance, based on the highest scientific standards, an unwavering integrity, and an astute understanding of human nature, has been in most demand in just those periods of SLAC history that have been the most stressful. And his wise counsel is always sought by the international particle physics community on how to deal with the broad issues facing the field.

BUT SID'S INFLUENCE has extended far beyond particle physics, in a real sense to the entire planet. As he himself recounts in this issue, he was drawn into the public policy issues of strategic defense and arms control already in the 1960s, and his credo and contributions can better be discerned by his own words than by any of mine. Making headway on political and social issues is much slower going than on physics, and Sid's modus operandi has been to patiently work "from the inside," a method which was strongly challenged during the Vietnam years. Sid, confronted personally at that time, responded with steadfastness and great integrity. He exemplifies for everyone the value and necessity of his form of public service. One small expression of his influence is that some of his students and other physicists passing through the SLAC environment have chosen to move their careers out of physics and into public affairs. Quite a few others, while staying within physics, have been especially active in the social issues created by the big-science character of particle physics.

THEN THERE IS SID'S IMPACT on me. I first encountered Sid as an undergraduate at MIT, where he taught courses I took, and in addition sponsored evening informal journal-club seminars for undergraduate physics majors in his home. We happened to emigrate to Stanford together in 1956, he on the faculty, I as a graduate student. I soon was privileged to be one of his many thesis students, something which then evolved into writing our textbooks. It was absolutely natural that I too should join SLAC, and to continue the close association. Sid has been everything to me, as a teacher and mentor, as a second father, as an esteemed colleague, as best man, and as simply a dear friend. Sid, it is a time for warm congratulations on your long and distinguished career at SLAC, and for best wishes for many happy retirement years—years which we at SLAC trust will continue to be shared with us.

bj

James Bjorken



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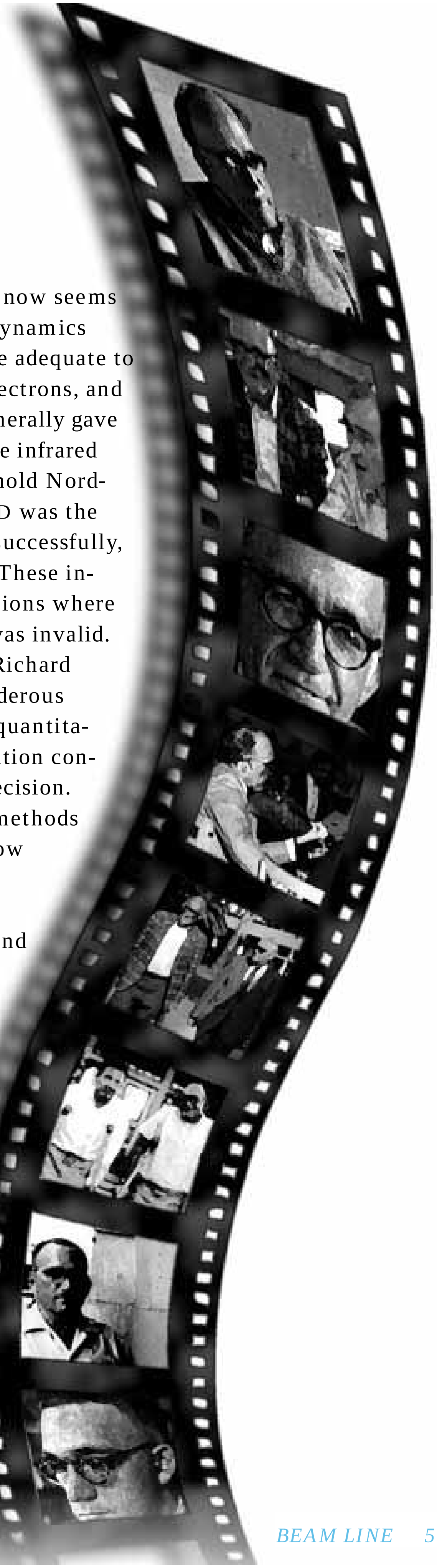
MY CAREER IN RESEARCH as a theoretical physicist dates back to fifty years ago shortly after the end of World War II. And it has been the best of times. Back then, it was a dream time to have been a graduate student! There was no need to worry about a job, unless for some strange reason you felt that Harvard was the only place to be.

Vannevar Bush had laid out a map for the support of science in his perceptive report to President Truman in 1945 entitled *Science, the Endless Frontier*. With clear and brilliant insight he presented the design and foundations of the nation's post-World War II scientific research program that has become the envy of the world. Here was his far reaching, visionary blueprint: "Science, by itself, provides no panacea for individual, social, and economic ills. But without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world." Furthermore, he reminded Washington that research is a difficult and often very slow voyage over uncharted seas and therefore, for science to flourish with governmental support, freedom of inquiry must be preserved, and there must be funding stability over a period of years so that long-range programs may be undertaken and pursued effectively. Physics was then a growth industry with an unreal coefficient of inflation that nurtured us all.

by SIDNEY DRELL

"It was the best of times but it could have been the worst of times."



Looking back at the particle physics of fifty years ago—which now seems like the Dark Middle Ages—we had a theory of quantum electrodynamics (QED) with which we could do lowest order calculations that were adequate to account for what was observed in processes involving photons, electrons, and positrons. But beyond that, exceedingly laborious calculations generally gave infinity, a result that was usually equated to zero and ignored. The infrared divergences alone were understood, thanks to Felix Bloch and Arnold Nordsieck. Although fragile, limited, and frustrating to work with, QED was the only "successful" field theory, and was variously, and usually unsuccessfully, used as a model to try and to understand other physical systems. These included nuclei and nuclear forces and what was occurring in situations where mesons were assumed to be the quanta and perturbation theory was invalid.

The first lightning flashes of real progress came in 1947 from Richard Feynman, Julian Schwinger, and Sin-Itiro Tomonaga, with a thunderous rumble from Freeman Dyson. They turned QED into a beautiful, quantitative theory whose divergences would be isolated into renormalization constants, and whose predictions could be calculated to very high precision. Feynman propagators turned horrendous calculations by the old methods into (well, almost) baby's play, and Feynman graphs helped us know what we were doing. It was a very heady time as we found theory agreeing with the beautiful precision measurements of the Lamb Shift, the electron g–2 value, hyperfine splitting in positronium, and higher order radiation processes.

Shortly thereafter there was great excitement as we discovered that there were two mesons—the cosmic ray one and the nuclear force one—and large new accelerators produced a veritable zoo of strange particles. Not only was the physics very exciting but also we were buoyed up by the strong support for science, and physics in particular, inspired by the demonstrated importance of the contributions that physicists had made to the successful conclusion of World War II through development of radar and the atomic bomb. As the cold war intensified there was growing concern that, perhaps, we would be needed again and so we better be nourished and rejuvenated as a strategic asset.

During the next three decades, into the 1970s, particle physics sped ahead with what now seems like a dizzying pace. A number of great laboratories were built in the United States and around the world. With the creation of

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CERN in Geneva, we truly became one international community collaborating productively on experiments and theories. Parity fell, and we learned the beauty of broken symmetry, spontaneous and otherwise. Muons and neutrinos came into the fold and a theory of weak interactions was completed. We dispersed, analytically continued, and Reggeized to study strong interactions. The proton and neutron revealed their inner structures and acquired many relatives in a strange particle zoo with new symmetries and selection rules; eventually quantum chromodynamics or QCD—a non-abelian gauge theory of quarks and gluons, with confinement and asymptotic freedom—was developed as a fundamental field theory of the strong interactions. Here at Stanford a new laboratory was created based on the peculiar idea that very high energy electron beams were also valuable probes for advancing our frontiers of understanding in parallel with the still higher energy protons. Thus the Stanford Linear Accelertor Center came to be and soon generated its own miraculous decade of discoveries—partons, charm, tau leptons—and developed progressively higher energy electron-positron storage rings and colliders as extraordinarily productive new tools for exploration. Our sister labs on the high energy frontiers also made landmark scientific and technical achievements of comparable importance.

Today, fifty years later, we have a Standard Model that unifies weak, electromagnetic, and strong interactions. We are able to put to the test our ideas on energy scales that reach back almost to the Big Bang fourteen billion years ago, and on distance scales hundreds of million times smaller than the Bohr radius. Currently we are awash in a sea of revolutionary and powerful new, and perhaps even correct, ideas of supersymmetry, strings, branes, etc., that have incorporated gravity into a unified theory of everything, as even its most modest practitioners describe it.

The union of our progress in particle theory with the probing by our astrophysicist colleagues into the farthest reaches of the Universe makes our extraordinary voyage of the past fifty years even more exciting. We are now beginning to read the history of the Universe almost all the way back to the Big Bang. Puzzles abound, but astrology has mutated into a *science* of cosmology. It has ceased to surprise us to wake up in the mornings to new pictures of clashing galaxies, stars being born and dying or being sucked into Black Holes, and other sensational evidence of what was going on far out there way back when! What a gig this has been! And what fun to have been ringside to so much of the action. The strong interaction with our experimental colleagues that has long been a SLAC hallmark has added greatly to the stimulating and enjoyable climate for our work.

In recent years we have suffered occasional disappointments in receiving less than hoped-for financial support for our activities and plans for the future. Gone are some of the momentum and optimism of the earlier years. More patience is required of us in fulfilling our aspirations. This is especially tough on the younger scientists looking for opportunities to spread their wings and fly. But the future remains rich with promise. The Department of Energy and the National Science Foundation program offices in Washington, with help from the High Energy Physics Advisory Panel, continue their strong and enlightened support for our work with due respect for the principles established by Vannevar Bush's report. We have also forged increasingly strong bonds of a single international community, cooperatively at work on a truly international Large Hadron Collider at CERN.

Looking back retrospectively, we cannot forget that, at the same time as we dreamed of the theory of Grand Unification, there were the nightmares of what could have been the worst of times. Scientific progress had also led to new technologies of nuclear weapons, missiles in space, and the possibility, for the first time in human history, that the new weapons we had created had such great destructive potential that they could lead to the end of civilization as we know it.

THE CHALLENGE TO PREVENT that nightmare from materializing led many physicists to work with the military and the government. Their commitment took many forms, some working at weapons laboratories and others working as technical advisers in the arms-control negotiating and policy initiatives. Some of us had the good fortune of being able to divide our lives between our academic research trying to understand Nature's mysteries and our technical efforts to help better understand and thereby try to reduce or counter the dangers we face. It is my personal conviction that the scientific community—not each individual but as a whole—bears a responsibility, a moral obligation, to project the implications of the technological changes initiated by our scientific progress, and to

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help citizens and their governments shape their practical applications in ways beneficial to all society. This responsibility is most cogently manifest in dealing with nuclear weapons, whose enormous destructive potential leaves so little margin for error.

In my case the dual tracks of academic research and teaching and involvement in government work opened in 1960 when the JASON group was organized. Its purpose was to enlist fresh scientific talent to work on problems of importance for our national security. We were in the dawning new age of nuclear weapons, space and intercontinental missiles, and the challenges they presented to formulating national security policy. At the same time, the great physicists and other scientists, whose contributions were so important in the winning of World War II with radar and the atomic bomb, had other responsibilities and were twenty years older than at the start of that war. I was inspired and greatly influenced in considering JASON by the example of two of my heroes. As physicists and wise counselors, Wolfgang K. H. (“Pief”) Panofsky and Hans Bethe had made great personal commitments and enormously valuable contributions to informed policy choices by the United States concerning arms control and national security. I very much admired what they had done. JASON thus became a new component of my scientific work. It served as an introduction for me to new problems that were often scientifically fascinating and strategically compelling. Subsequently many other doors opened for my involvement, both inside and outside of the government. Over time I ended up working on a variety of interesting technical issues of national security and arms control.

EARLY ON I BECAME INVOLVED in the technical possibilities of gaining intelligence from space-based satellite systems as a way of piercing the Iron Curtain erected by an obsessively secretive Soviet government. Photoreconnaissance from satellites circling the earth above the atmosphere at altitudes above 100 miles enabled the United States to pierce the shroud of secrecy by means that were effective, and that were accepted as non-provocative. With the photography brought back to earth we could more accurately assess the growing threat of Soviet nuclear warheads mounted on intercontinental range missiles and bombers. Subsequently it also opened the path to arms control. Since we could count and size the Soviet’s threatening strategic forces from the satellite photographs, we could negotiate treaties and verify compliance with treaty provisions to limit their deployment and to initiate reductions. Photoreconnaissance satellites were the first big step toward achieving the Open Skies that President Eisenhower had first called for in 1955.

Working in this area of technical intelligence was compelling for its obvious strategic importance. The more accurately we can gauge the nature and imminence of developing threats from our perceived or potential foes, the more responsibly and confidently we can act in crises and plan for our national security. This truism is consonant with the fundamental tenet of an academic career—the more we learn and the better we understand a situation, the better prepared we are to address it and act wisely. I also found this work, continuing up to the present, extraordinarily fascinating on technical grounds as I interacted with scientists and engineers from both the academic and the industrial world whose accomplishments were remarkable.

Throughout the cold war the issue of how best to discourage, deter, or defend ourselves against the use of nuclear weapons was on center stage, front and center. Debates about the potential value, versus the dangerous illusions, of nationwide anti-ballistic missile (ABM) defenses were ongoing, with periodic crescendos, for more than three decades. Though often driven by political considerations, these were serious debates about strategic policy that touched a fundamental instinct of all human beings to protect our families and homes. Nuclear warheads with their enormous destructive potential had greatly changed the requirements of an effective defense from the pre-nuclear era. But how different, and what constituted sensible programs and goals? Was it practical to try to defend society with ABMs? What was the best way to maintain a survivable missile force in order to establish a strategic stability that relies on mutual assured destruction to deter a would-be attacker? There is an essential technical core to any informed debate between defense and deterrence. It has commanded the attention of many scientists for a long time, and I did not escape involvement in this important issue of national security.

At the root of this issue are two technical realities: the relative ease and economy of designing and deploying offensive countermeasures to overpower any conceivable defenses; and the requirement that a missile defense against nuclear-tipped missiles must be near perfect if it is to be effective in protecting society. In addition, and of utmost importance, one has to consider the almost certainly harmful impact of an arms build up between competing offenses and

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defenses, and their countermeasures and counter-countermeasures, on strategic stability and future prospects of reducing the nuclear threat.

These considerations are still central to the continuing debate about ballistic missile defenses in 1998. Technology has changed enormously over the years and new ideas have come to the fore, such as directed-energy weapons and space-based sensors. Furthermore we face new strategic challenges in the post-cold war years. However, I still see the situation pretty much as stated by President Eisenhower in 1953 in his "Atoms for Peace" speech at the United Nations: "Let no one think that expenditures of vast sums for systems and weapons of defense can guarantee absolute safety. The awful arithmetic of the atom bomb does not permit any such easy solution. Even against the most powerful defense, an aggressor in possession of the effective minimum number of atomic bombs for a surprise attack could place a sufficient number of his bombs on the chosen targets to cause hideous damage."

Simply put, as much as one would like to have an effective defense against nuclear attack, one cannot escape limitations dictated by laws of Nature in a futile effort to achieve a policy goal that is technically unrealistic, even if desirable.

In his famous "Star Wars" speech in March 1983, President Ronald Reagan sought to escape these limitations and build an effective nationwide defense by relying on the new and emerging technologies of beam weapons and advanced space-based sensors. Some of the most ardent supporters of his proposed Strategic Defense Initiative indulged in hyperbole with claims that they could and would create an "astrodome," or impenetrable defense, of the entire nation against a massive attack by intercontinental ballistic missiles. In the absence of a careful analysis of the practical technical realities, fanciful claims preceded more measured judgments, and a largely political and highly acrimonious debate ensued. Subsequently, much more modest, but more realistic, goals for a limited ABM system emerged after a lot of hard work and careful analyses by many physicists in academia, think tanks, and industry, who analyzed the broad repertoire of new and prospective technologies along with relevant operational issues.

This experience was the most compelling and clearest case I know for restoring a high-level non-partisan presidential science advisory mechanism that is actively engaged in technical national security problems. President Eisenhower created one in 1957 following the Soviet launch of Sputnik and development of long-range missiles as a potential threat to the United States. The scientists involved in this mechanism were his resource for direct, in-depth analyses and advice as to what to expect from science and technology, both current and future, in establishing realistic national policy goals. They were selected apolitically and solely on the grounds of demonstrated achievements in science and engineering. Two things set them and their work apart from the existing governmental line organizations and cabinet departments with operational responsibilities, and from non-governmental organizations engaged in policy research. First of all, they had White House backing and the requisite security clearances to gain access to all the relevant information for their studies on highly classified national security issues. Second, the individual scientists were independent and presumably, therefore, immune from having their judgments affected by operational and institutional responsibilities. Therein lay their unique value. Unfortunately the advisory mechanism that served the White House and the nation well when it was created, eroded in the late 1960s during the political strains and public discord of the Viet Nam conflict and has not been reenergized effectively in national security matters.

Most recently I have been involved in helping to provide the technical basis for the U.S. decision to sign, and to lead the effort to ratify, a worldwide Comprehensive Test Ban Treaty (CTBT) that would, once-and-for-all, end all testing of nuclear weapons of any yield, anywhere, anytime after more than fifty years and more than 2000 nuclear test explosions. The political and strategic importance of such a treaty for accomplishing our non-proliferation goals was made clear in the debate in May 1995, at the United Nations. One hundred and eighty one nations signed on to the indefinite extension of the Non–Proliferation Treaty based on the commitment of the nuclear powers to work toward the cessation of all nuclear weapons tests. Before committing itself to honor this commitment, the United States had


Nina Stolar

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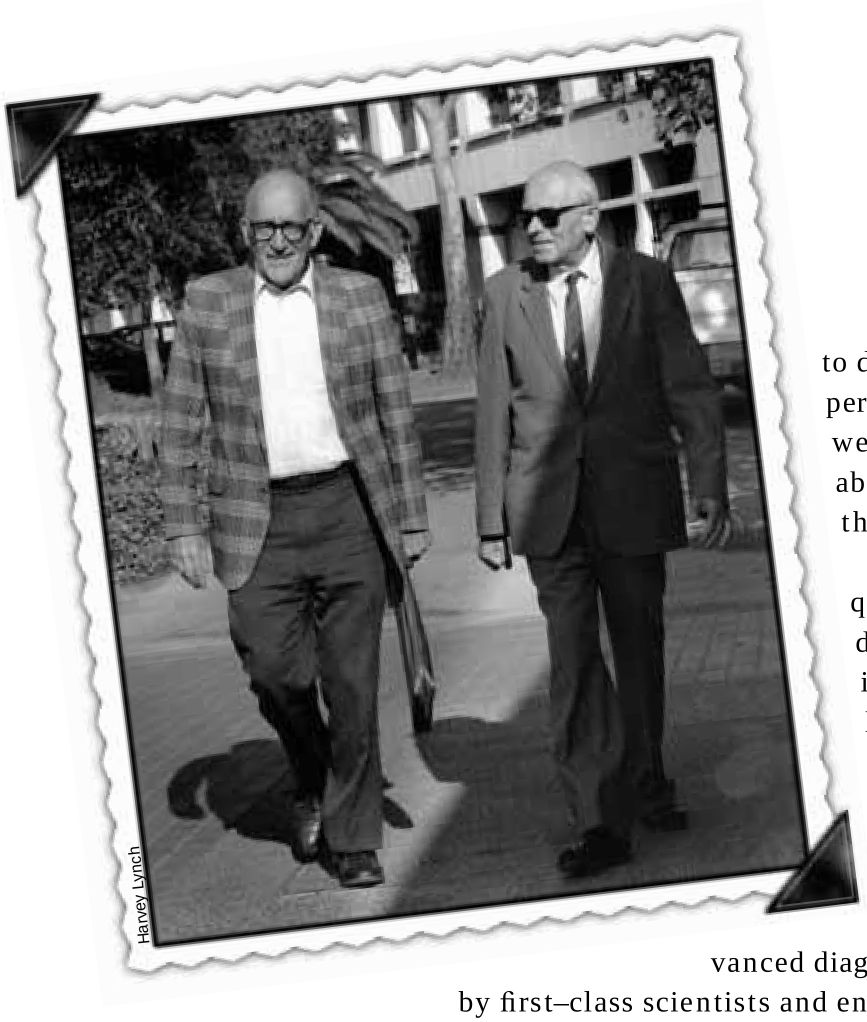

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to determine what would have to be included as permitted activities in a negotiated CTBT, so that we could retain confidence in the safety and reliability of our enduring nuclear warheads into the future.

A JASON study was organized to answer this question in 1995. It was our finding that confidence in the safety and reliability of the enduring stockpile can be maintained, even if very low yield tests are banned under a true CTBT, so long as the United States sustains a strong scientific and technical infrastructure in nuclear weapons. Simply put, with a strong science–based stockpile stewardship and management program, equipped with advanced diagnostic equipment and led, as it presently is, by first–class scientists and engineers at the national weapons laboratories, there is no need to continue nuclear testing at any level of yield. Instead we will rely on enhanced attention to surveillance and diagnostic information, and accurate simulations that will be made possible by major advances in computational speed and power to deepen our understanding of the physical processes in a nuclear explosion. By filling in the substantial gaps in that understanding that we could accept so long as we could directly monitor the performance of our bombs by testing, we will establish a basis for retaining confidence in our ability to hear whatever warning bells may ring—however unanticipated they may be—alerting us to evidence of deterioration of an aging stockpile. There will also be facilities to provide for warhead refurbishing or remanufacture in response to identified needs. This program is consistent with the spirit, as well as the letter of the CTBT: without testing the United States will not be able to develop and deploy with real confidence more advanced weapons at either the high or the low end of destructive power.

Our conclusion was endorsed by the weapons laboratories and proved to be persuasive in Washington. It provided the technical base for President Clinton's decision for the United States to support and seek a true, zero-yield Comprehensive Test Ban Treaty in August 1996. The scientific challenge to develop and successfully accomplish this mission is a major one for the weapons labs, and for all involved in the process.

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LOOKING BACK, it has been the best of times. SLAC has been and remains a wonderful home with great science, colleagues, students, and friends. Indeed it is one of the great pleasures of a career in physics to have such wonderful colleagues worldwide. The future of our field—and of SLAC too—depends on the continuing extraordinary inventiveness of its scientists—the accelerator builders and experimentalists on whom we rely for data, the lifeblood of science. By all signs the future looks bright, with no end in sight. With amazing inventiveness, theorists have introduced new concepts that one couldn't even have dreamed of fifty years ago. The questions we must still answer are certainly sharper and at least as compelling as they were fifty years ago: Where has all the antimatter gone? What is the origin of CP violation? Of particle masses, especially for fermions? Whether or whither sypersymmetry and sparticles?

On the nuclear front we have had the good fortune to avoid the worst of times, but much work remains to be done. The end of the cold war has greatly reduced the immediacy of the nuclear fear that was a recurrent element of that long contest. But that danger persists, inherent in what we know how to do, concrete in the between 20,000 and 30,000 warheads possessed today by at least eight nations, and present in the ambitions of others. And new threats are emerging, involving other weapons of indiscriminate destruction—chemical and biological—in the hands of sub–state entities and terrorists. They can no longer be ignored, as the attack in the Tokyo subway system by the Aum Shinrikyo reminded us in 1995. The community of scientists will have to remain strongly involved, as we have been up to now, in efforts to build a safer twenty-first century as we advance our understanding of Nature.

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by JOHN ELLIS

The Next Steps in Particle Physics

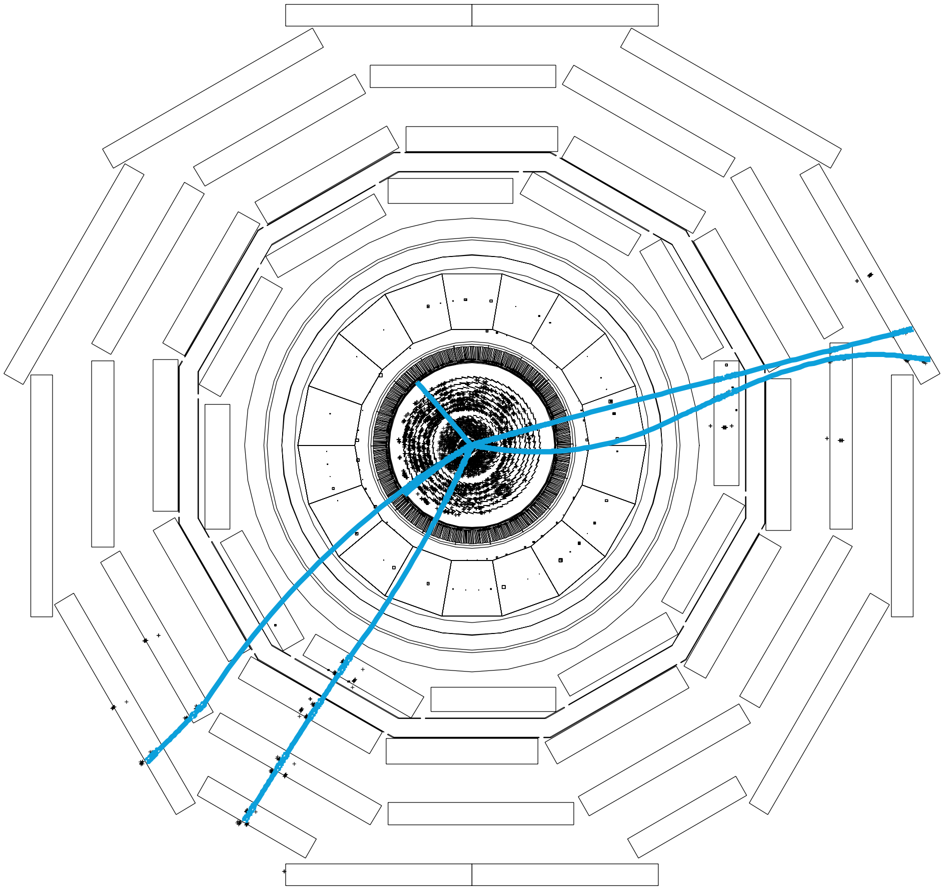
PARTICLE PHYSICS IS NOW POISED to take its next steps. A well-established framework is provided by the Standard Model, which has been tested in experiments at CERN's Large Electron Positron (LEP) accelerator and the SLAC Linear Collider (SLC) to a precision approaching the tenth of a percent level. According to the Standard Model, the fundamental forces are due to the exchange of intermediate vector bosons, such as the Z boson probed at LEP and the SLC, analogous to the photon responsible for electromagnetism. These interact with the fundamental matter particles—the strongly-interacting quarks and the non-strongly-interacting leptons. Experiments at LEP and the SLC have established a limit of six each on the numbers of conventional quarks and leptons. The sixth and last quark—the top—was found at Fermilab in 1995 with a mass consistent with predictions based on its quantum effects in the precision electroweak data (see "Discovery of the Top Quark" in the Fall 1995 *Beam Line*, Vol. 25, No. 3). Higher-energy experiments at LEP are now producing pairs of the charged vector bosons W^+ or W^- that carry the weak nuclear forces, with the aims of measuring their masses and couplings to the photon and Z to see, in particular, if they agree with the predictions of gauge theories. By the end of its operations in the year 2000, LEP will also have searched systematically for all possible new particles weighing less than about 100 GeV. Meanwhile, the Fermilab proton-antiproton collider will be exploring masses up to a few hundred GeV for some new particles.

The very success of the Standard Model in describing to date all confirmed accelerator data raises fundamental problems to be addressed by the next generation of experiments. Foremost among these is the *Problem of Mass*—What is the origin of the different particle masses, and in particular how do the Z and W acquire masses around 91 and 80 GeV, respectively, while the photon remains massless? Newton taught us that weight is proportional to mass, and Einstein taught us an equivalence between mass and energy, but neither said anything about the origin of mass itself. In the initial formulation of the Standard Model, particle masses were generated by an unseen field permeating the Universe, named after one of its proponents, Peter Higgs. There should be a particle associated with this field, corresponding to quantum fluctuations, which has been the quarry of searches at LEP and elsewhere. The absence of such a Higgs boson, at the time of writing, implies that its mass must exceed about 90 GeV.

Theoretically, this picture of a simple Higgs field and its accompanying elementary boson is unsatisfactory. Some theorists have suggested that it might be composite, and one of a multitude of new strongly-interacting particles, but the precision electroweak data disfavor many of these hypotheses. Alternatively, an elementary Higgs boson may exist accompanied by a plethora of supersymmetric particles—one for each known particle, with identical electric charge but different spin. Experiments at LEP and Fermilab have sought such supersymmetric particles, which should weigh less than about 1000 GeV, but to no avail so far. The search for a Higgs boson and its colleagues, be they composite or supersymmetric, and the elucidation of their properties, is one of the main items on the agenda of the next generation of particle accelerators.

Another key concern is that of the *Problem of Flavor*: Why is there such a proliferation of quark and lepton species, and what explains their bizarre pattern of masses and couplings to the *W*, including the appearance of CP violation? The Standard Model has no explanation for the number of particle species, and just rewrites the couplings of the *W* boson in terms of the couplings of the Higgs boson. This is unsatisfactory and has led some theorists to postulate that quarks and leptons are composite. However, despite recent alarms from Fermilab and the HERA electron-proton collider at DESY in Hamburg, there is no experimental evidence for this radical hypothesis. The new generation of experiments on mesons containing bottom quarks, at *B* factories at SLAC and KEK as well as other laboratories, may elucidate the mechanism of CP violation and help us understand flavor.

Finally, there is the *Problem of Unification*: Can the gauge theories of the electroweak interactions be combined with quantum chromodynamics (the theory of the strong nuclear interactions) in a Grand Unified Theory (GUT), and perhaps with gravity in a Theory of Everything? Data on the vector-



This is how a Higgs boson weighing 150 GeV might look in the CMS detector if it decays into four muons. Particles are first tracked in the central part of the detector, and muons then pass through the cylindrical calorimeters that absorb other particles, to be measured in the outer chambers.

boson gauge couplings favor GUTs with supersymmetry, and there are hints that neutrinos might have masses, as predicted by many GUTs, but it may be difficult to accumulate direct evidence for GUTs, or for a more ambitious Theory of Everything based on string theory.

Several accelerators have programs that address these fundamental problems. I have already mentioned the continuing runs of LEP and the Fermilab collider, and HERA will also continue to run for several more years. And, as already mentioned, about to start operation are the *B* factories at SLAC and KEK in Japan that will address the Problem of Flavor (see "Why Are We Building *B* Factories" in the Spring/Summer 1996 *Beam Line*, Vol. 26, No. 1).

IN ADDITION, construction has started on the Large Hadron Collider (LHC) at CERN, which is scheduled to come into operation in 2005. The LHC will provide proton-proton collisions at a center-of-mass energy of 14 TeV, with a luminosity of $10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. In the longer term, the LEP machine components could be combined with the LHC to make electron-proton collisions at a center-of-mass energy of 1300 GeV and a luminosity of $10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$.

The LHC proton-proton collisions will provide the first exploration of physics at an effective scale of 1000 GeV or more. At the top of the agenda in this energy range is the resolution of the Problem of Mass. The Higgs boson, or whatever replaces it, is expected to weigh less than 1000 GeV. Indeed, the precision electroweak data from LEP, the SLC and elsewhere indicate that the Higgs boson may weigh around 100–200 GeV. In this mass range, the Higgs boson of the Standard Model should have observable decays into photon pairs or four charged leptons. Two major detectors for discovery physics at the LHC are under construction: ATLAS and CMS (see companion articles by M. G. D. Gilchriese and Dan Green respectively in the Winter 1997 *Beam Line*, Vol. 27, No. 4). Each is designed to be able to explore all the possible range of Higgs-boson masses, and particular attention is being paid to the precise measurements of photons and leptons.

The range of Higgs-boson masses favored by the analysis of precision electroweak data coincides with that predicted in supersymmetric models, giving encouragement to their advocates. The major LHC experiments should be able to find strongly-interacting supersymmetric particles—the squark partners of quarks and the gluino partners of the gluons of QCD—if they weigh less than about

2000 GeV, as expected in supersymmetric models. They may also be able to reconstruct decays of squarks and gluinos, and to identify some weakly-interacting supersymmetric particles, as well as discover at least one of the super-symmetric Higgs bosons.

In this way, the LHC should provide a breakthrough in our understanding of the Problem of Mass. If supersymmetric particles exist, it should also provide some detailed measurements of their spectroscopy, that could provide some hints concerning GUTs and perhaps a Theory of Everything, contributing to our understanding of unification. The LHC will also make possible measurements of mesons and baryons containing bottom quarks that are more detailed than those possible with the SLAC and KEK *B* factories, continuing their assault on the Problem of Flavor.

The LHC machine and detectors are being constructed as a global project, with significant contributions from the United States, Japan, Russia, Canada, China, and Israel as well as the European member states of CERN. Indeed, the American contingents of experimentalists are the largest in ATLAS and CMS. It seems likely that any future major accelerator project will be realized as a similar global collaboration.

This is the context in which several projects for linear e^+e^- colliders are currently being prepared for proposal to governments around the world. Although we cannot be sure what the LHC may (or may not) discover, we can already make some educated guesses as to the stones it will leave unturned. Although it can discover the Higgs boson, the LHC cannot measure many of its couplings to other particles and verify that it does its job of providing their masses. Although it can discover some supersymmetric particles, the LHC probably cannot discover all the predicted weakly-interacting ones. It is also limited in its possible mass measurements, and hence in the possible tests of unification it can make. Also, the LHC may well be unable to discover the heavier Higgs bosons expected in supersymmetric theories. There is plenty of scope for an accelerator with complementary capabilities to complete our picture of physics in the range of energies up to about 1000 GeV.

The fact that present electroweak data favor a relatively light Higgs boson offer the enticing prospects of copious production at a next-generation e^+e^- collider.

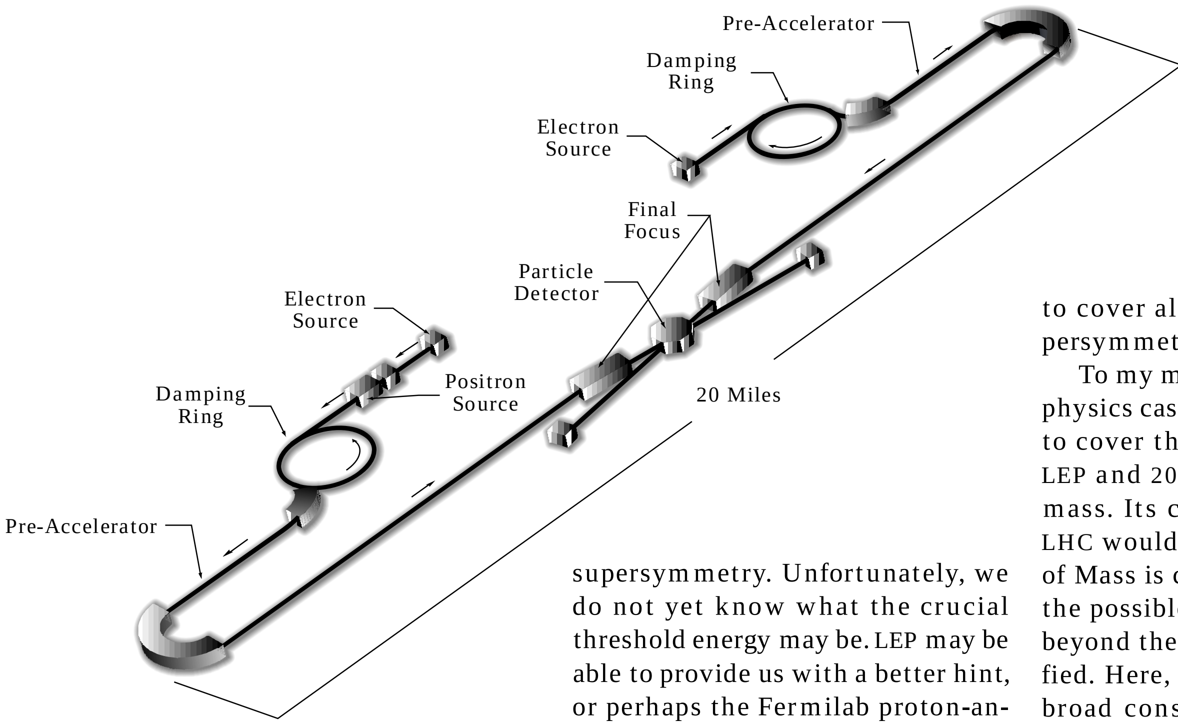
LEPTON-ANTILEPTON collisions offer a cleaner experimental environment than proton-proton collisions, and one in which all types of particles—both weakly- and strongly-interacting—are produced democratically. An example of the complementarity of e^+e^- and hadron-hadron collisions has been provided in the 100 GeV range. Although the *Z* was discovered in proton-antiproton collisions, its detailed study has been possible only in e^+e^- collisions. These and other prior experiences convince us that future e^+e^- colliders are likely to broaden and deepen considerably our understanding of physics beyond the Standard Model.

The first established landmark beyond the energy reach of LEP is the top quark-antiquark threshold around 350 GeV in the center of mass. Detailed measurements of properties of the top quark will be possible, and spectroscopic measurements may enable us to derive indirectly some properties of the Higgs boson.

Moreover, the fact that present electroweak data favor a relatively light Higgs boson offer the enticing prospects of copious production at a next-generation e^+e^- collider. This would enable its couplings to the *Z* boson, the bottom quark, the tau lepton and perhaps two photons to be compared in detail with the predictions of the Standard Model or its supersymmetric extension. A linear e^+e^- collider would certainly advance our knowledge of the Higgs boson far beyond its initial discovery.

Another guaranteed physics topic is W^+W^- production, which will enable the gauge theory predictions for W^+W^-photon and W^+W^-Z couplings to be tested with far higher precision than is currently possible at LEP or Fermilab. It may also be possible to measure the *W* mass more precisely, as well as many other properties.

IN ADDITION to this (more or less) guaranteed physics agenda, there is the prospect of a threshold for new physics beyond the Standard Model, perhaps that for



Artist's conception of a TeV linear electron-positron collider. The individual elements are not drawn to scale.

supersymmetry. Unfortunately, we do not yet know what the crucial threshold energy may be. LEP may be able to provide us with a better hint, or perhaps the Fermilab proton-antiproton collider, but we may well have to wait for the LHC before knowing better where (for example) supersymmetry may appear.

Once one is above the threshold, a linear e^+e^- collider will enable unparalleled measurements to be made. The masses of supersymmetric particles can be measured with high precision, and their spins determined. All the heavier weakly-interacting supersymmetric particles can be studied, as well as the heavier supersymmetric Higgs bosons. Many consistency checks on the supersymmetric theory will be possible, and many checks of unification ideas will be made. Although the LHC should be able to discover supersymmetry, and may be able to make many spectroscopic measurements, a linear e^+e^- collider will be needed to complete the job.

In view of the uncertainty in the supersymmetric threshold energy, flexibility in the energy of such a collider is essential. We have already seen that a center-of-mass energy of 350 GeV provides an interesting initial program of top physics and perhaps the Higgs boson. However, the machine should be able to reach much higher energies eventually: ideally up to 1000 GeV per beam so as to cover all the likely range of supersymmetric particle masses.

To my mind, there is a compelling physics case for a linear e^+e^- collider to cover the energy range between LEP and 2000 GeV in the center of mass. Its complementarity to the LHC would ensure that the Problem of Mass is completely resolved, and the possible path to related physics beyond the Standard Model is clarified. Here, personal prejudice and a broad consensus among theorists have guided me to concentrate on supersymmetry, but the linear collider would surely have similar benefits for the study of composite Higgs models. During the coming few years, as projects for linear colliders are finalized around the world, this physics case must be conveyed convincingly to the world particle-physics community, and beyond. It seems to me clear that any such project would need to be constructed on a fully international basis, building on previous experiences with HERA and the LHC. In this connection, the present cooperations between different laboratories around the world, notably between SLAC and KEK, are excellent first steps. On the other hand, it is also good that several alternative technologies be researched and developed in parallel, so that the optimal choice may be made when the time comes. Since several different options are being studied, one might wonder whether more than one project could be proposed successfully. In my view, it would be difficult to convince the physics community of the need for more than one linear collider in the energy range below 2000 GeV in the center of mass, but politics is another story.

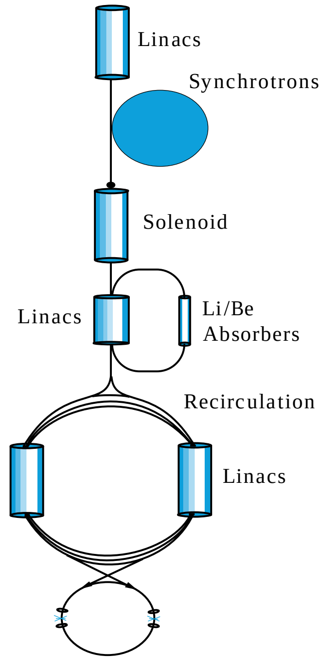
SINCE THE PERIODS between the first discussions and the realizations of new accelerators are long and lengthening—LEP was first studied in 1975 and entered operation in 1989, a gap of fourteen years, the LHC was first mentioned in 1978 and first studied in 1984—it is not too soon to consider what might come after the LHC and the linear collider. Several ideas are circulating in the United States and in Europe, many of which will require years of research and development before a specific project can be formulated.

One of these is a higher-energy linear e^+e^- collider, operating in the range of 2000 to 5000 GeV in the center of mass. This would probably require a different technology from those currently envisaged, operating at a high accelerating frequency. CERN has been doing research and development on such a technology, based on the compact linear collider (CLIC) two-beam concept, as part of the world-wide linear collider effort. I trust that the CLIC work will continue, with the aim of demonstrating its feasibility for a post-LHC project.

Another concept for colliding leptons at several thousand GeV in the center of mass is a muon collider. There are major issues connected, for example, with source intensity, beam cooling and decay radiation, that will need to be tackled in an extensive research and development program. Aspects of this idea are now being discussed actively in the U.S. and it is desirable that other regions of the world also contribute to this effort. A lot of work is necessary before the feasibility of such a muon collider can be demonstrated, so there is plenty of work to go around! It is attractive to consider a demonstration project, analogous to the SLC as "proof of principle" for linear e^+e^- colliders. From the physics point of view, an interesting option could be a Higgs factory, which would exploit the larger coupling of the Higgs boson to muons, and the possibility of a reduced energy spread compared to an e^+e^- collider.

Finally, let me mention the ideas that are being discussed for a possible proton-proton collider with beams in the 50–100 $\times 10^3$ GeV range. The key technical issues here are the reduction of tunneling and equipment costs by an order of magnitude below those of the LHC.

IT IS TOO EARLY to say which of these possible options for accelerators beyond the LHC and the linear e^+e^- collider might be most interesting. The answer depends on the unknown physics that these prior machines will uncover. However, we can already be sure that both the LHC and a linear collider capable of reaching up to about 2000 GeV in the center of mass will be very exciting physics projects. Now that the LHC is under construction, I hope that all the world's particle physicists will work together to gain approval and construct such a linear e^+e^- collider. Physics needs it!



Possible layout of a muon collider.

Future Collider Parameters

Collider	Particles	$E_{c.m.}$ (GeV)	Luminosity ($cm^{-2}s^{-1}$)	Starting Date
PEP II	e^+e^-	10	3×10^{33}	1999
KEK-B	e^+e^-	10	10^{34}	1999
LHC	pp	14,000	10^{34}	2005
LHC	ep	1300	10^{32}	?

A Physicist in the World of Vi

by WILLIAM ATWOOD

A physicist applies well-known principles to the construction of violins and in the process comes to a better understanding of techniques for shaping their sound.

THE DEVELOPMENT AND HISTORY of the violin covers much the same time frame as contemporary physics. While Newton was developing his classical mechanics and formulating how the planets move in the sky, Antonio Stradivari was creating some of the world's greatest and most treasured violins in Cremona, Italy. This instrument had evolved from the viol family about one hundred years earlier and was given its final form by another Italian maker, Gasparo da Salo, and his student Giovanni Maggini. Like the modern-day electric guitar, this evolution was driven by the need for a louder sound to accommodate public functions. It was an instrument of the masses, unsophisticated and brash. The viol remained the refined favorite of the royal courts.

Salo's new design was seen and copied by a Cremonese maker, Andrea Amati, whose sons continued their father's lead and perfected the violin's design. The question of whether Stradivari was an apprentice in the Amati shop rests on a single label in one of his violins attributing its style to Amati. What we know for sure is that both lived in Cremona in the second half of the 1600s and that Stradivari began making violins in the Amati style. He soon branched out, however, and went on to a long and illustrious career during which he produced over a thousand instruments.

By 1750 the violin had gained wide acceptance. Indeed, most famous composers of the day produced works that featured this instrument. It was their demands that led to some last modifications. The definition of pitch was raised and the neck (and hence the string length) was lengthened, allowing

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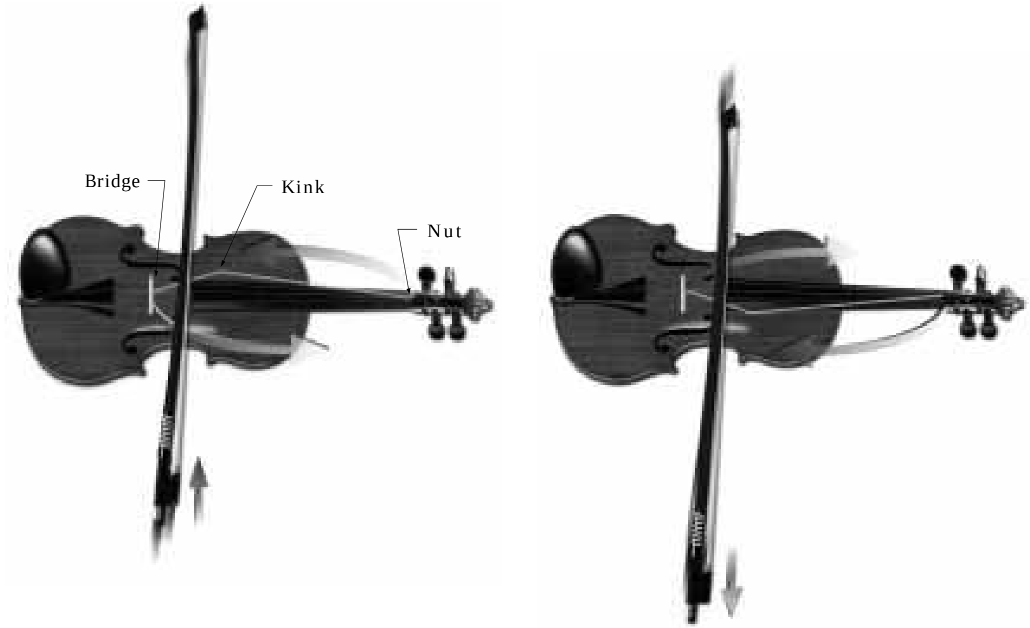
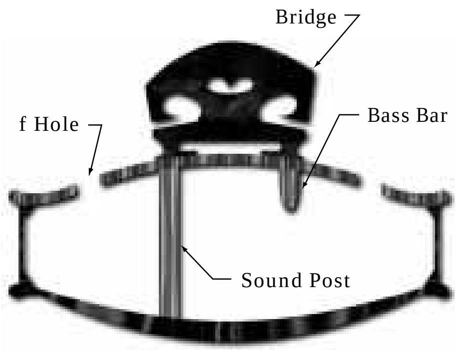


An example of Helmholtz string motion where a kink appears to rotate around the string, alternately bouncing off the bridge and the nut.

a greater range in notes. Unfortunately, the original technique of simply nailing and gluing the neck onto the body proved too weak to counterbalance the pull of the strings. In response, in the late 1700s the maker Mantegazza developed a method of mortising the neck into the sides and top block for added strength. The modern violin had been born. However, violins made during the next two centuries failed to achieve the success of the earlier Italian makers, leading many to believe that a secret or secrets had somehow been lost.

Yet, progress was made during this period in understanding how bowed instruments produced their characteristic sound. The German physicist Hermann von Helmholtz is credited with explaining bowed string motion. The conditions of fixed length and tension and of continuous sound (periodicity) mean that the only frequencies present could be integral multiples of the fundamental (that is, the lowest allowed vibration mode). Musically this means that for a given note, its octave, the fifth above that, the next octave, the third above that, and so on are part of the sound. Helmholtz recognized that the timbre of the violin sound must be very rich in these harmonics, and he became curious as to how the moving string produced them. The string, he determined, does not just wag back and forth upon being agitated by the bow. Rather a sharp kink makes round-trips between the bridge and the nut, flipping over at each reflection (see the above illustration). As the kink passes by the contact point of the bow on its way to the bridge, it first releases the grip of the bow hairs on the string and then an instant later grabs them again. The result is a continuous plucking action often referred to as "slip stick." The kink travels in an apparent



A cross section of the violin at the bridge.

clockwise direction for "down" bows and counter clockwise for "up" bows. As a result, when drawn across a level string, the bow tends to skate towards the bridge on an up-bow and towards the fingerboard on a down-bow.

Following the Second World War, a renaissance in violin making began with luthiers bent on recapturing the precision and artistry of former years and a new interest from scientists about how the violin works. Several new schools dedicated to the construction of stringed instruments now exist in the United States. And there are organizations with refereed journals focused mainly on the science of the instrument. In this stimulating environment, a young and enthusiastic group of makers is arguably producing violins that may in fact rival the old Italian instruments.

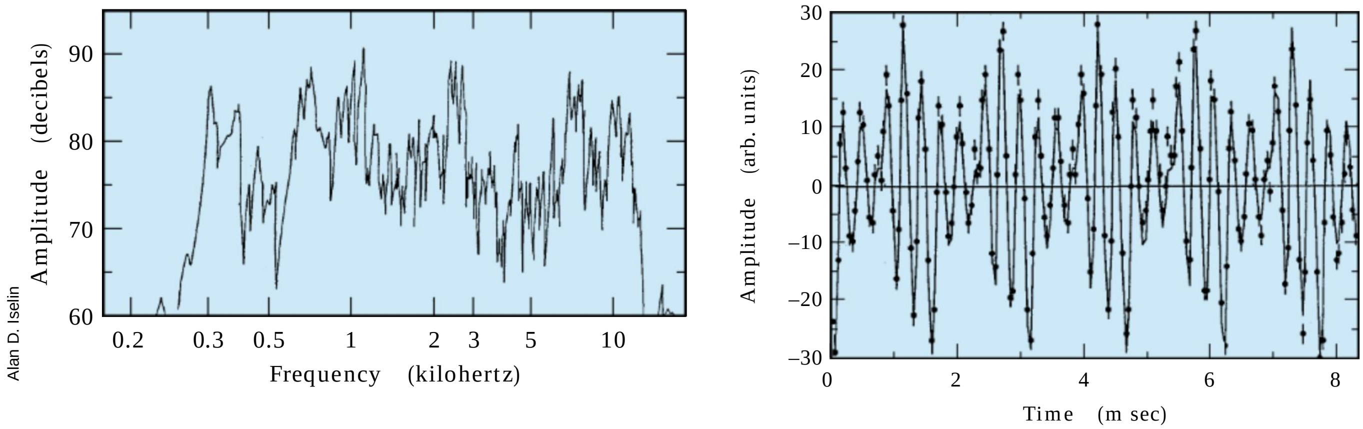
FUNDAMENTALS OF A WORKING VIOLIN

The illustration at the top of the page is a cross section of the violin at the location of the bridge. The body is essentially an empty, thin-walled box. The top plate, or "belly," that produces most of the sound is typically two to three millimeters thick while the back is similar in thickness around the edges but can be five millimeters or more near the center. The walls are approximately one millimeter thick except for the linings that reinforce the glue joint to the belly and the back. Under the foot of the bridge corresponding to the E string, a small dowel mechanically couples the top to the back. Under the other foot is a longitudinal stiffener called the bass bar. Both of these components not only have a profound influence on the sound but also are structurally critical as the downward force of the bridge owing to the tension of four strings is about twenty pounds.

The violin body is basically a resonator driven by the vibrations from the bridge induced by the aforementioned Helmholtz-type string motion. However, only those frequencies that approximately match the natural resonances of this system will result in any appreciable sound.

The frequency response can be measured by vibrating the bridge with a pure sine wave (no harmonics). This is accomplished by attaching a lightweight transducer to the bridge and sweeping the drive frequency while keeping the amplitude constant. The relative sound output can then be recorded (see left illustration on the opposite page). While the data in this plot are in detail complex, some overall features are easily understood. First there is a forest of resonances extending from about two hundred hertz up to about fifteen kilohertz. Although there are many sharp gaps, most notes generally sit sufficiently close to a resonance to be excited. The lower end of this range is quite curious: the bottom few notes on a violin (G at 196 Hz and G# at 208 Hz) fall off the edge of the response envelope. Somehow our ears are able to infer these notes from the higher harmonics they contain. Also, investing money in home audio equipment with responses above fifteen kilohertz won't make the violins sound better!

Each note on a violin has its own particular sound. The fundamental as well as its harmonics will fall in various places in the response



These two plots illustrate aspects of violin sound. On the left is the frequency response of a Guarnerius del Gesù violin. A dense "forest" of resonances covers the frequency band from approximately 220 Hz to 15 kHz. On the right, the waveform of a single note (E) played on a violin is shown. The pronounced rapid oscillations are due to the fifth and sixth harmonics, while the slow, overall envelope modulation is at fundamental frequency (660 Hz). (Courtesy Carleen Hutchins)

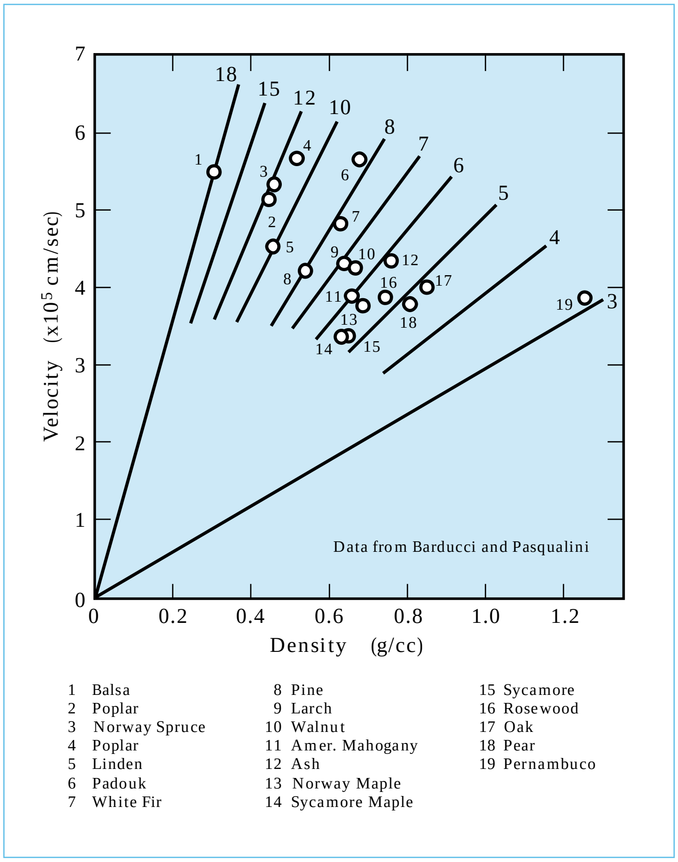
spectrum and be amplified accordingly. In the right-hand figure above the waveform of the sound of a violin being bowed on the open E string is shown fit to a harmonic series. For this note the fifth and sixth harmonics are three and two times larger than the fundamental. The apparent rapid oscillations are the result of these harmonics, slowly interfering with each other while the (slow) overall envelope modulation is at the fundamental frequency of 660 Hz. Yes, we hear E, but we also very quickly identify the note as coming from a violin. It is the presence of these harmonics that identifies the source of the sound for us. If we repeated the above exercise for other notes, similar results will be obtained; however, the harmonic content (which harmonics are important) will change.

To achieve good projection, one wants positive pressure waves to be produced simultaneously from the top and back of a violin. You might visualize this condition as the violin alternately swelling and shrinking. Jack Fry, another noted physicist, pointed out in the 1970s that the arrangement of sound post and bass bar inside a violin resulted in this type of radiator. But certainly not all of the resonances can be so simple. Various places on the surfaces of the two plates will be moving in and out of phase and hence form higher pole radiator patterns. For each note, various radiation patterns will emphasize various harmonics when measured at different locations. As Gabriel Weinreich recently observed, "perhaps this is part of the intriguing aspect of the violin sound. . .it seems to dance and sparkle, coming all at once from some place and at the same time from no place in particular!"

APPLYING PHYSICS TO MAKING VIOLINS

A physicist's approach to learning about a new physical system often starts with fundamentals such as how big, how small, what controls the overall behavior, etc. John Schelleng pursued this type of dimensional analysis and found an overall figure of merit for violin wood: the ratio of the density to the velocity of sound. One can intuitively understand this ratio is important by observing that if, for instance, the top weighs a lot, it will require a hard shove to get it moving. But, just being light is not sufficient. We want the entire top plate to move—not just the local area under the feet of the bridge. Hence, the stiffness is also of prime importance.

It is interesting to plot the density versus velocity for various types of wood (see illustration next page). Along straight lines emanating from



The velocity of sound versus the density is plotted for different types of wood. This ratio (velocity/density) is a good figure of merit for acoustical quality of a material, high values being best. The data points indicate typical values for a particular wood; however, a variation of 30 percent in both sound velocity and density is not uncommon.

the origin the ratio is constant: large values being "good." Balsa wood seems to be the material of choice! Of course we're not even suggesting this option since, as already mentioned, the static loading of twenty pounds from the downward force of the bridge would tax the strength of balsa. Not far behind, however, is spruce. This is the material of choice in practically all musical instruments that have a wooden sound board (pianos, guitars, harps, violins) and for the same reason (high stiffness per unit weight) was the material of choice for aircraft before aluminum was readily available (Howard Hughes' famous Spruce Goose).

However only the top of a violin is made of spruce. The rest is usually made from curly maple, a high figured hard wood. The above chart reveals that this material wasn't chosen on the basis of sound production but rather for its beauty and perhaps, more fundamentally, its strength (the axial pull of the four strings combine to over sixty pounds). It also matters how the wood is cut from the tree. Detailed experiments have revealed that the velocity of sound decreases as the alignment of the fibers deviates from being parallel to the surface. In fact not only does the sound velocity go down, but the rate at which vibrations are damped-out increases. This becomes a noticeable effect for deviations as small as a few degrees. Traditional makers have always preferred wood split from logs rather than sawed. Now we know why.

A modern mechanical engineer might view the violin as an example of thin shell technology. Violin plates begin as thick pieces of wood. The final cross-sectional shape shown in the figure on page 22 is arrived at through a two step-carving process. First the outside is shaped. The curve (called the arching pattern) is usually copied from one of the old Italian masters. While these arching patterns may appear the same to the untrained eye, there can be a wide variety in the maximum height and how full the curve is. "Height" is a measure of how much the center section puffs up from the plane of the sides (usually 14–16 mm). The back of the instrument can have a different height than the front. The "fullness" of the arch refers to how far this puff extends toward the edges. "Swoopy" arches start their descent closer to the center. Full arches (descent closer to the edge) tend to be stronger and more resistance to long-term deformation owing to the static loading produced by the strings.

After shaping the outside, the inside is scooped out. Here's where the fun begins! How thick should the wood be and what thickness pattern should be used. This is complicated and still only poorly understood. Since the material is wood and therefore anisotropic with variations in physical properties from piece to piece, any recipes based purely on dimensions will fail. Somehow one must use the vibrational properties of the individual plate to guide the thinning (or graduating) process.

Traditionally makers have use a technique based on "tap" tones. Holding the free plate off center in the upper area with the thumb and

forefinger and tapping near the center with the other hand produces a distinct pitch that can be perceived as the plate nears its final thicknesses. Keeping the same holding point and tapping at the center near the bottom reveals yet another pitch. When old Italian instruments are disassembled for repair, their tap tones are often examined. In particular, a famous maker/restorer named Simone Sacconi, who worked on many of the existing Stradivari violins, reported that all had tap tones between F and F#, strongly suggesting that this was part of the equation that evolved in Italy three hundred years ago.

Carleen Hutchins, who studied under Sacconi, developed a new method for measuring tap tone while making instruments. She showed that they corresponded to eigenmodes of the free plate, which can be seen clearly using the technique of Chladni patterns. Hutchins suspended a violin plate horizontally above a loud speaker connected to an audio signal generator. When black glitter is sprinkled on the surface, at resonance, the patterns shown in the drawing on the right appear. The nodal lines are the places where the plate is not moving and hence the glitter tends to accumulate. The traditional tap-tone technique required holding the plate on a nodal line and tapping at an anti-node. This simple process is in wide use today.

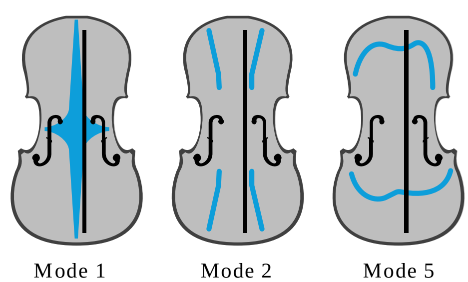
Tuning the tap tones has now been reduced to a "science," but free violin plates are not an end in themselves. The plates are eventually glued to the sides, changing the boundary conditions from free to somewhere between hinged and clamped. Investigations into the vibration modes for plates both free and clamped have shown that the thickness of wood near the edges of a plate is critical in determining the eigenmodes for the clamped condition but has little effect on the free plate. Wood removal near the center controls the latter.

SETUP

Completing the violin after graduating the plates involves a great deal of precision woodworking and then comes the varnishing. The next phase in which physics can play a part is in the "setup." This includes shaping the fingerboard and bridge, fitting the sound post, adjusting the tailpiece, and so on. Much of how the final product will sound is determined at this stage.

The bridge of the instrument conveys the vibrations from the strings

The three main eigenmodes for a free violin top plate are shown below. The frequencies associated with these modes are approximately an octave apart.



Both Helmholtz and Drell seem fascinated with bowed string motion. Here Drell is engaged in a real time analysis of the bow's slip-stick interaction with the string.

to the top. In some sense it may be viewed as an audio filter. Its traditional and intricate shape was most likely arrived at through trial and error with an eye on the esthetics and an ear on the resulting sound. Observations of which bridges sound good suggested an experiment using similar tuning techniques to those used for free plates. A high-power audio tweeter and audio generator revealed that the best bridges had a simple bending eigenmode pitched at high F (2800 Hz).

After a final fitting with strings, the vibrational modes of the complete instrument can be investigated and adjusted. When the lowest air cavity resonance and the lowest body-bending mode have similar frequencies, the violin comes alive. While there has been little quantitative work to detail what sound a mode-matched instrument makes as opposed to an unmatched one, both players and listeners prefer the former. The frequency of the air cavity mode is determined mostly by the volume of the box and the size and shape of the f holes. Little can be done to alter this "bottle" note and indeed one finds little variation from instrument to instrument. A closely related note can be found by humming into an f hole and finding the pitch at which the body resonates (usually between C to C#). The first bending mode has two transverse nodal lines running across the widest sections of the upper and lower areas. The pitch of this eigenmode can be determined by holding the instrument upside down at the widest point in the area next to the tailpiece and lightly tapping on the scroll. It turns out that the fingerboard can be modified to vary this note. By adding weight at the end near the bridge or by thinning the fingerboard in the area where it joins the neck, the note can be lowered.

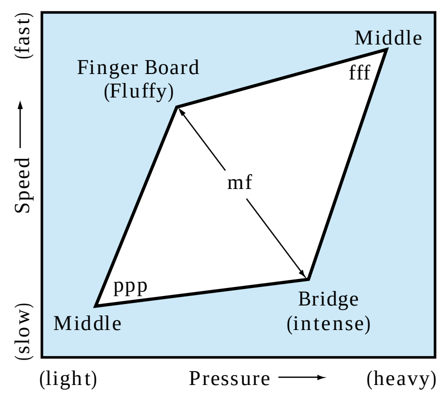
The tailpiece also plays an important role. The relative lengths of the strings between the bridge and the nut at the far end of the fingerboard and between the bridge and the tailpiece should be six to one. This length can be approximately tuned using a ruler; however, a knowledge of harmonics gives a simpler and more accurate technique. A string which is one-sixth the full length will sound a note at the fifth above the second octave. Given that violins are tuned in fifths means that by plucking the string behind the bridge and comparing it to the note of the next higher string plucked in front of the bridge an octave should be heard. The pitch of the note behind the bridge may be adjusted by changing the length of the cord (called the tail-gut) fastening the tailpiece to the end-pin of the instrument. But the tail-piece also has resonances which have been found to have optimal values. Changing the tail-gut length changes these pitches as well. The only remaining free parameter is to adjust the weight of the tailpiece: heavier lowers its resonance pitches while lighter raises them.

PLAYING

One of the appealing attributes of the violin is the variety of sounds. This sound palette is manipulated through the bowing technique. The main variables are the speed of the bow, the downward pressure on the string, and the point of contact on the string with the bow hair. (To a lesser extent the amount of hair which is in contact with the string also plays a role.) All this occurs essentially in a three-dimensional space. A two-dimensional projection of this bowing space is illustrated in the figure on the right. The softest sound is obtained with a slow, light bow halfway between the fingerboard and the bridge while the loudest notes are produced with a fast, heavy bow at a similar contact point. The most variation in speed and pressure can be found in the middle of the range. Here too is the largest range of possible contact points. Harsh, biting sounds may be produced with the bow near the bridge, while soft, fluffy sounds are obtained with the contact point nearer the fingerboard. By adjusting these three variables, the violinist is changing not only the music dynamic but also the timbre of the sound.

An obvious question is what happens if you go outside the "allowed" region in the bowing space? You produce either an annoying scratching sound or an equally unpleasant whistling. The edges of this space are given by the boundaries for the Helmholtz-type "slip stick" action as discussed in the introduction. Preferred violins have a large bowing space. These instruments allow the concert artist the greatest range in sound color and dynamic. A corollary is that cheap violins have a small bowing space. This explains how the beginning student, with little bow control, may sound atrocious on a $200 fiddle provided by the parents while the teacher (with a great deal of bow control) can sound reasonably good!

Physics or physics training does not give any intrinsic advantage to violin making. Music is essentially esthetic and producing an instrument necessitates highly developed woodworking skills. However, physics can be a tool for understanding what parameters of the instrument control various aspects of its sound, thereby providing a guide for shaping it. It can also inspire experiments with narrow focus aimed at resolving specific issues in violin making. And, finally, good experimental science, specifically the recording of detailed notes on each instrument, can pay off handsomely when trying to determine what works or doesn't work.



The bowing space for the violin is illustrated above. The speed that the bow is drawn across the string together with the down pressure changes the musical dynamic (shown inside the white diamond). The point of contact between the bow and string changes the "color" of sound and has its largest range in the middle (shown outside the white diamond).

A Possible Future

Microfabrication for Millimeter W

by HEINO HENKE & ROBERT SIEMANN

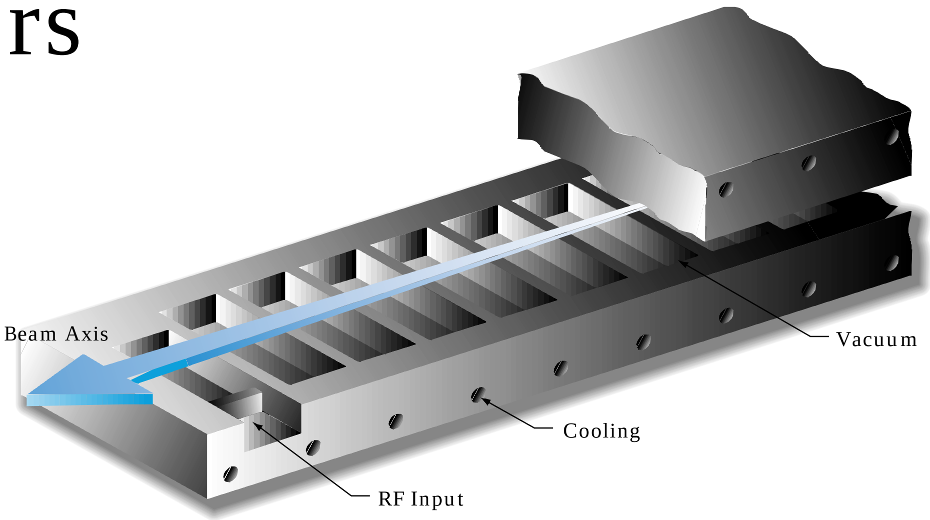
Large and small scales are intertwined throughout physics. Huge accelerators produce beams for atomic, nuclear, and particle science. In an interesting, new twist, microfabrication could be the key to the accelerators of the future.

INSTRUMENTS AND THEIR CAPABILITIES often determine the frontiers of science. The particle physics frontier is the highest achievable energy, and there has been exponential growth in energy because of breakthrough concepts, inventions and discoveries, and application of new technologies to particle acceleration. The Fermilab Tevatron is an example—there the earlier breakthrough concepts of strong focusing and colliding beams are combined with superconductivity, one of the great discoveries of modern physics, and the application of superconducting technology to accelerator magnets.

Synchrotron radiation sources are having profound impact on a wide range of sciences from condensed matter physics to protein crystallography. Those synchrotron radiation sources were made possible by the invention of klystrons which are efficient high power radio-frequency (rf) amplifiers, strong focusing, and sophisticated magnet technology.

What concepts, discoveries, and/or technologies will determine the future frontiers of accelerator-based sciences? In the near term the CERN Large Hadron Collider and the NLC—or ILC—the international linear collider being designed jointly by KEK and SLAC, depend on extensions of magnet and rf technologies. Superconducting rf is becoming more and more important for high current applications in nuclear and particle physics, for single-pass coherent light sources and for linear colliders with the TESLA project at DESY. However, none of these are breakthroughs—and without breakthroughs the costs of accelerators are becoming formidable.

ve Accelerators



A muffin-tin accelerator structure.

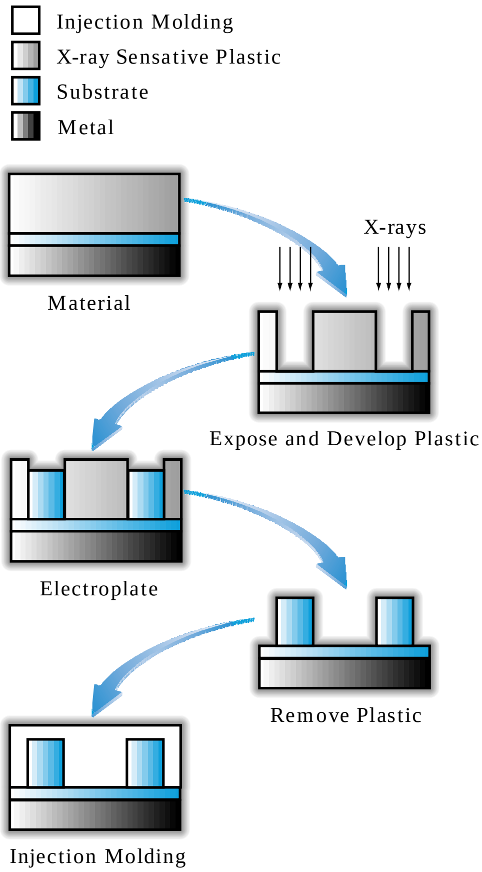
Our interest has been in exploring new technologies which are riskier but have breakthrough potential. One such technology is micromachining that makes it possible to think about millimeter-wavelength accelerators that have millimeter-size features. These accelerators have potential for both particle physics and synchrotron radiation, but they cannot be realized from straightforward extrapolations of long wavelength accelerators like the classic SLAC 10-cm wavelength, S-band linac. We do not imply that there are not formidable problems; rather we see an opportunity if the problems posed can be solved either through technological developments or conceptual insights.

MICROFABRICATION

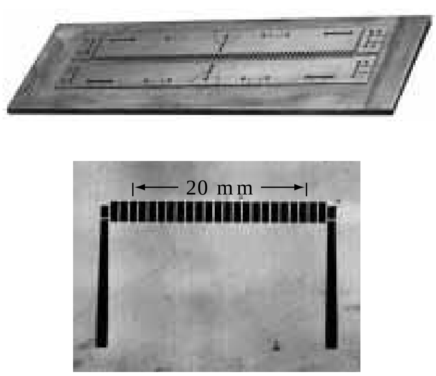
In the last few decades microfabrication has made impressive progress. High speed milling machines and shaped diamonds allow 2–5 μm precision. Tiny high pressure water jets and laser beams provide fast and precise cutting. Pressureless wire electrodischarge machining reaches the accuracy limits (below 1 μm) of the best measuring devices. Ion etching and lithographic techniques, the work horses of the semiconductor industry, have been modified for mechanical manufacturing. The youngest offspring is shape deposition manufacturing where metallic powder is injected into the melt-pool of a scanning laser focus.

Traditionally linac structures are cylindrical, but a more open structure is needed for adequate vacuum pumping as dimensions shrink to the millimeter scale. The planar, muffin-tin geometry shown in the illustration above seems to be the best candidate. It consists of two separate halves with a wide gap in between that provides sufficient pumping conductance. This simple two-dimensional structure has mechanical tolerances in the 1 to 3 μm range. These tolerances and the planar geometry make wire electrodischarge machining and deep X-ray lithography with the German acronym LIGA that stands for Lithographie, Galvanoformung, Abformung meaning lithography, electroforming, and molding.

Electrodischarge machining (EDM) is a relatively cheap and conceptually simple process, and the tolerances achieved are remarkable. The workpiece and an electrode are immersed in a dielectric fluid, and a high voltage pulse produces an arc between the electrode and workpiece which erodes the surface. Since the electrode is eroded as well, it has to be replaced, and a wire electrodischarge machine uses a thin, spooled



Simplified lithography process.



A 94 GHz muffin-tin accelerator fabricated with LIGA by Argonne National Laboratory scientists (top) and the center of a 25-cell, 91 GHz accelerator machined with EDM at Ron Witherspoon, Inc. (bottom).

tungsten wire that cuts through the workpiece like a fretsaw. The cutting speed strongly depends on the thickness of the workpiece and the desired surface finish. Today, the best machines cut 1 mm per minute in a 1 mm thick copper sheet with submicron precision and 1 μm surface roughness. A nearly optical surface roughness of 0.1 μm is possible at lower speeds.

LIGA has been developed primarily at Karlsruhe, Germany, for fabricating microstructures with extreme height to width ratios. In a first step (Lithographie) a thick resist layer of plastic is irradiated through a mask by synchrotron radiation (see illustration on the left). By developing the exposed resist layer, a template is produced which can be filled with a metal by electroforming (Galvanoformung). After stripping the unexposed resist a metallic microstructure is obtained. A third and fourth step allow for mass fabrication. A negative of the microstructure is obtained by injection molding of thermoplastic material (Abformung). This negative serves as a template for subsequent electroforming processes. Many structures can thus be made from the original master.

Today LIGA is an advanced technology. Many components such as acceleration sensors, microturbines, electrostatic motors, and linear actuators are being manufactured by industry. The achievable accuracy is around 1 μm or better, and structure depths of 600 to 800 μm are possible. This is exactly what is needed for a muffin-tin structure.

Both EDM and LIGA have the promise of meeting the tolerances required for accelerator applications, and they have relative advantages and disadvantages that could prove crucial. Two drawbacks of EDM are that the structure must be constructed in layers and diffusion bonded together and that the process is not well suited for mass production. LIGA is expensive for prototype fabrication since it requires an X-ray mask which typically must be manufactured in three steps, a synchrotron radiation beam line for a many-hour exposure of the plastic resist, and facilities for developing the resist and electroforming the structure which proves to require care and experience. It should be much cheaper when producing large numbers of identical structures because molding can be used. Open questions depend on the application and relate to surface quality and the suitability of electroformed copper for high gradients.

mm-WAVE POWER SOURCES

In many ways the availability of high power sources is critical for accelerator development, and at the present time such sources for mm wavelengths are scarce. The only real high power sources are gyrotron oscillators with up to 1 MW power, and they are large and expensive. Modifying them to be amplifiers reduces the power rating and efficiency dramatically. The Naval Research Laboratory, working in collaboration with CPI and Litton, is developing a 100 kW gyrotron amplifier. Another candidate for high power at short wavelengths is the ubitron, but it is better suited as an oscillator, and significant development is needed for it to be a high power amplifier.

Microfabrication offers a new and different approach to a mm-wave power source—a large number of small, simple, and cheap tubes with a minimal power distribution network in contrast to the usual configuration of a small number of high power tubes with a complex distribution circuit. A mm-wave klystron should be able to generate up to 150 kW and would have good efficiency. Using permanent magnet focusing, developed at SLAC for X-band (3 cm wavelength), such klystrons would meet the goal of being small, simple, and cheap.

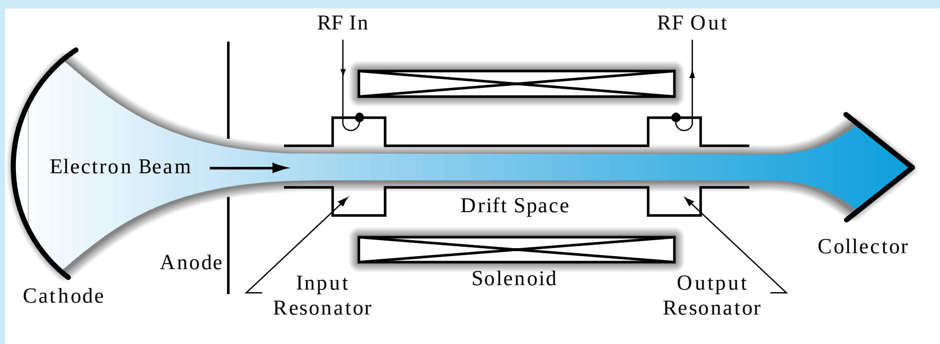
A 150 kW klystron would be an ideal power source for compact, low energy accelerators, but high energy accelerators require substantially more power. Possible ways to achieve that are discussed in the high energy accelerator section.

COMPACT, LOW ENERGY ACCELERATORS

An advantage of mm-wave accelerators is the small, planar geometry. Complex structures can be realized with lithography on a single support with no extra fabrication costs, and the designer's skill—rather than cost or space—becomes the limiting factor. This leads to new ideas for compact, low energy accelerators that could include a small, light linac for medical applications, a space-based accelerator, or a compact synchrotron radiation source.

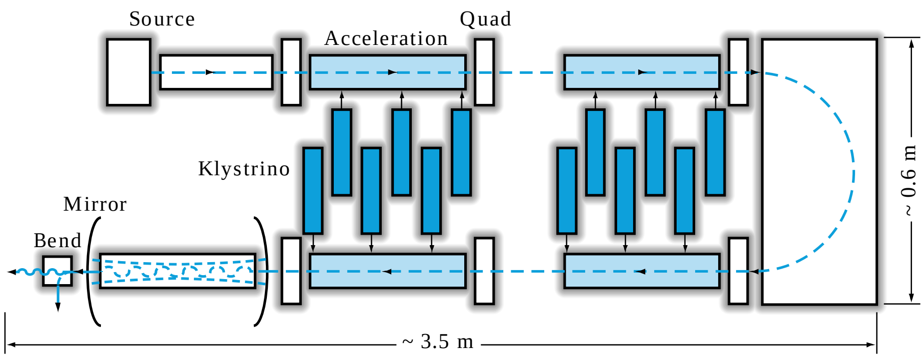
Some applications require standing wave cavities where forward and backward traveling waves conspire such that the accelerating gradients have alternating signs from cell-to-cell. It isn't possible to feed many cells of a standing-wave cavity from a single input coupler, and these structures are also sensitive to fabrication errors. The solution is to use specially designed coupling cells that do not contribute to the acceleration but increase and symmetrize the coupling. Such a structure is normally complicated and expensive, but with lithography one gets it for free.

Klystron Amplifier Principle

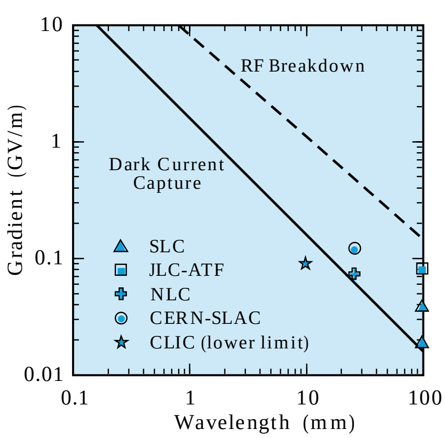


Basic klystron arrangement.

THE PRINCIPLE OF THE KLYSTRON AMPLIFIER is shown in the figure above. A heated cathode emits a continuous electron beam of relatively low density. The beam is accelerated and at the same time focused by an electrostatic anode before entering an input resonator that is fed by a low power input signal. There the beam receives a velocity modulation that depends on the input signal. This dependence makes the klystron an amplifier, as opposed to an oscillator. This is critical because it allows control of the multiple klystrons needed in an accelerator. After a drift space, where the beam is focused magnetically, the originally continuous beam is bunched which corresponds to a large rf current. It enters the output resonator where it loses energy to an external load (usually the accelerator). The leftover beam with a strongly reduced kinetic energy is dumped into the collector. The klystron as described would be fully operational, but it would have low efficiency and low gain. High power klystrons have additional idling resonators between the input and output resonators to improve the bunching process.



A compact, tunable radiation source. The linac is composed of 16 cm long rf structures with 140 cells each. Every structure is powered by a 100 kW klystron.



Gradient limits from dark current capture and rf breakdown together with achieved gradients.

A mm-wave rf undulator is under development at Argonne National Laboratory's Advanced Photon Source. When a relativistic electron beam travels through an rf undulator, it is subjected to transverse forces from the rf fields, and it radiates coherent, quasi-monochromatic radiation similar to that from a conventional magnetic undulator. At around 100 GHz the undulator period can be as short as 1 mm. This is substantially smaller than possible with a magnetic undulator, and the electron beam energy can be lowered for the same photon energy.

Scientists at the Technische Universitaet in Berlin are interested in low energy accelerators with gradients between 1 and 10 MV/m, and the main concerns are small size, low weight, and low cost. One example is a compact 50 MeV linac that could power either an undulator or a free electron laser and generate tunable radiation down to 50 nm wavelength. The figure at the top of the page shows some details. The klystron and beam focusing device are integrated such that the whole accelerator fits on one table apart from power supplies. For the future it would be attractive to have integrated machine modules. That means a certain number of rf structures together with a klystron, beam monitors, and magnetic focusing devices are mounted on a single support and all fabricated lithographically. A module then needs only connections to the power supplies, vacuum pumps, and low power electronics to become a working accelerator.

PARTICLE PHYSICS

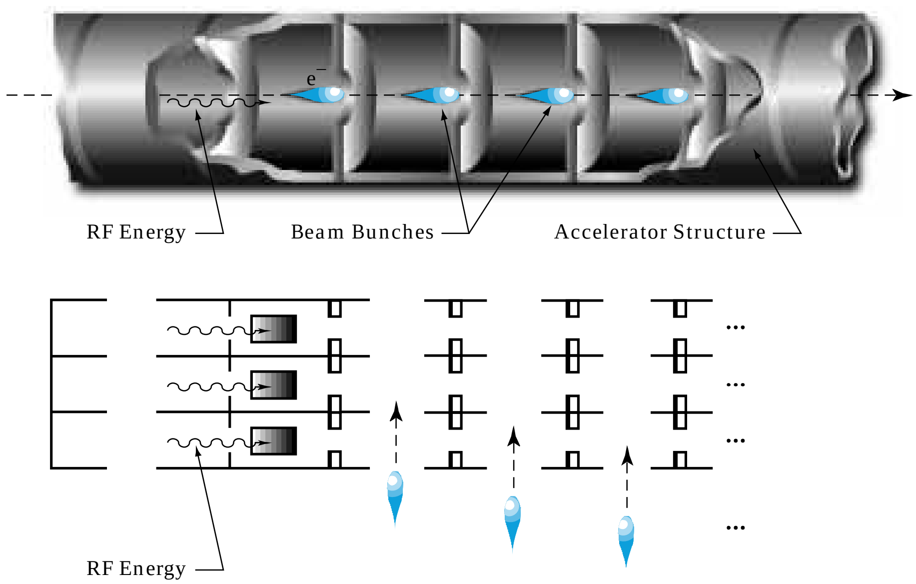
The potential advantage of mm-wavelength accelerators for particle physics is a higher accelerating gradient. It is known from experience with 1–10 cm wavelengths that gradients are limited by dark current capture, which is the acceleration of field emitted electrons to relativistic energies, and by rf breakdown which is a complex phenomenon involving field-emitted electrons, X rays, ions, rf fields, and surface conditions. The maximum gradients from these phenomena scale approximately inversely with wavelength as shown in the figure on the left.

These wavelength dependencies give mm-wavelengths breakthrough potential, but short wavelength, high gradient accelerators require much more than a straightforward extrapolation from longer wavelengths. Our colleague Dave Whittum of SLAC has a lighthearted, but also serious, transparency that summarizes the situation were such an extrapolation made, "The power source doesn't exist; the accelerator would melt in one shot; the beam density dilution is unacceptably large; the beams destroy themselves during the collision; and the collider would require a dedicated power plant." This is fertile ground for imaginative solutions.

Begin with pulsed temperature rise and associated destructive effects. Radio-frequency losses are concentrated on the surface, and the resultant heat does not diffuse significantly into the metal during the rf pulse. The surface volume is repeatedly stressed beyond the yield strength and eventually the surface could fail. This has been seen with 40°C temperature rise in high power laser mirrors where surface quality is critical.

Forty degrees is far below the temperature rise for interesting accelerating gradients, but there is anecdotal evidence that pulsed temperature rises well above that do not cause problems for rf cavities. An experiment using power from the X-band klystrons is in progress to study this systematically. The first run had the tentative result that rf properties of copper were not affected after 10^7 pulses with over 100°C pulsed temperature rise. This apparatus will also allow testing of materials with higher yield strength and testing of ideas to reduce pulsed temperature rise such as a thin diamond coating that is transparent to rf but serves as a heat sink thereby reducing the temperature rise by up to a factor of three.

Beam-induced fields, or wakefields, reduce the beam phase space density. If the reduction is sufficiently large, it is impossible to focus tightly at the interaction point. Wakefield effects are proportional to charge, offset of the trajectory from the center of the aperture, and vary inversely with the cube of the wavelength. The latter is obviously bad for short wavelengths. The best way to reduce wakefields is to precisely align the beam and accelerator.



The conventional and matrix accelerator approaches to multiple bunch acceleration.

Experiments with a prototype X-band structure have demonstrated that wakefields can be detected and are precise position monitors. A wakefield-derived signal can be used as the input to a feedback loop that maintains alignment. Such feedback loops could provide the wakefield control that is needed for millimeter wavelengths. To the degree they do not, the charge will have to be lowered.

But, since luminosity depends on the beam charge as well as phase space density, this is also bad. It is possible to get high luminosity with low charge individual bunches by accelerating a large number of bunches, one behind the other, with a long rf pulse (see the illustration at the top of the page). This cannot be extended to millimeter wavelengths because the pulsed temperature rise is proportional to the square root of the rf pulse length. Dave Whittum has conceived of a simple, elegant way to accelerate multiple bunches: let the rf and beam travel in perpendicular rather than parallel directions. This "matrix accelerator," shown on the previous page, accelerates multiple bunches while keeping the rf in a single cell for only a short time. Like many conceptual breakthroughs, this solves a problem but introduces new ones which, hopefully, are easier to solve. Parallel beams must be combined; the rf structure remains to be designed and shown to be practical (lithography could be the key); and a source for the required high power rf needs to be invented.

To put a scale to the rf power, the gradient depends on the square root of the rf power, and a muffin-tin structure requires 40 MW input power for 200 MV/m gradient. If such powers are possible they require both a power source beyond the 150 kW klystron we discussed and pulse compression. A high power ubitron may be the right power source. We will know better when the klystron development is further along. With a ubitron one could reach multimegawatt power levels for several microseconds. This must be compressed down to a few nanoseconds to have the appropriate peak power and pulse length for the matrix accelerator. Pulse compression is routine; it is used in the SLAC Linear Collider and is part of the NLC rf system. The new aspects are the degree of compression and the almost certain need for active switching of high power rf which has been demonstrated at moderate powers.

SOME OF WHAT we have written about is futuristic; some is not. Some will come to pass, and some will not. The 150 kW klystron and low energy compact accelerators could be realized in the near term. When they are, they will be direct results of thinking about and applying microfabrication to accelerators.

A millimeter-wavelength, 5 TeV collider is an ambition to study physics at a new energy frontier. It will take originality and creativity to make it possible, and even so it may never be. It is a promising direction, and, as with all basic research, one pursues it for that reason while at the same time realizing that the unexpected is expected.

Part I

Can't You Keep Einstein's Equations Out of My Observatory?

by VIRGINIA TRIMBLE

Fish and guests start

to smell after three days.

—Old Country Saying

ALL SPECIES ARE ADAPTED to the environment that has been around for a while (some admittedly better than others). This includes *Homo scientificuss* and its subspecies *H. scientificuss* astronomische. Thus even we, committed though we are to Progress and Advancement, tend to resist changes in our surroundings. Such resistance is by no means completely foolish. Most of the inventions patented never worked very well; most new ideas are wrong; and most of the people who tell you how to improve your research want to start without the second law of thermodynamics. Hence, betting against neat, new things is nearly always the winning strategy. But when you lose, you lose spectacularly, and virtually all the achievements of astronomy over the last few centuries can be traced pretty directly to changes in technology, demographics, or theoretical constructs.

New widgets include amplifiers, detectors, telescopes, launch vehicles, computers, and much else. The community constantly recruits not just new people but new kinds of people. And I hope at some future time to explore how we have (often reluctantly) incorporated these. Today (Wednesday), however, the focus is on interchanges of ideas between astronomy and other parts of science. An oral presentation of some of the material, at the 150th anniversary meeting of the American Association for the Advancement of Science,

was originally entitled "How physics came to visit and tried to take over the house," but I soon realized that the real situation is a good deal more complex. Notice, however, that even the name of the society includes "defend our territory" words as well as "reach out" words.

Both the domains of thought and the specific ideas within each of the following sections are as chronological as I could make them (which isn't very). And the approach is relentlessly Whiggish, making use of highly collimated hindsight, emphasizing contributions that we perceive as moving the subjects forward, and drawing distinctions between disciplines that probably did not exist when some of the ideas were being formulated.

NEWTONIAN GRAVITATION

Sir Isaac, in assembling his theory of universal gravitation, made direct use of astronomical observations of the motions of the moon and planets. He promptly returned the favor by providing the first estimates of the mass of the sun: 29.7 earth masses in 1687 and 226.512 earth masses in 1713. Notice he shared the fondness of modern, calculator-owning freshpersons for carrying around more (in)significant figures than necessary! The correct value is actually 332.9 earth masses, but nearly all of Newton's error the first time out arose from using a distance to the sun that was not the best available even then, not from the content of his theory. Astronomers soon fell in with the idea, and improved "solar masses" came from Lalande (a cataloguer of stars) in 1774 and Encke (who found an interesting comet) in 1831. Estimates for the masses of Jupiter and the other planets with satellites followed and gradually improved in parallel as the distance scale for our solar system improved.

The next step obviously was masses for other stars, but this required recognizing that many of them orbit in gravitationally bound pairs. John Michell, the English polymath, had said so on statistical grounds as early as 1767. He was perhaps more ignored than disbelieved, and the recognition of the reality of bound systems had to wait for William Herschel's efforts to measure stellar parallax. He thought that close star pairs in the sky were accidental projections. But, after more than twenty years of looking for changes in separation angles due to the motion of the Earth (parallax), he concluded that the real motions were orbital and the pairs not accidents.

Masses for stars followed slowly (most visual binaries have orbit periods of many years, and you need a distance estimate as well). And, meanwhile, Herschel held to the view that only solid bodies could exert gravitational forces and therefore the sun, though perhaps gaseous on its surface, must have a cooler, solid interior, of which we catch glimpses through the sunspots. He also expected that interior to be inhabited, and was by no means alone in either opinion. A gaseous (and uninhabitable) sun had to await advances in spectroscopy and thermodynamics, which brought their own controversies and misunderstandings.

CERES, STATISTICS, AND THE PERSONAL EQUATION

The discovery of Ceres in 1801 (by Giuseppe Piazzi, a Sicilian astronomer) was very nearly followed by the undiscovery of Ceres, when it disappeared behind the sun with too little of its orbit observed to permit calculating where it should reappear. Carl Friedrich Gauss came to the mathematical rescue, with an improved method of orbit computation that made use of what we now call the method of least squares to incorporate discordant data. And to this day, the least squares method is the only bit of statistics the average astronomer has heard of. We nearly all think Gauss invented the idea, which is also only approximately true.

The personal equation is a statistical idea that arose within astronomy and has not, so far as I know, ever seemed applicable anywhere else in science, though it turns up occasionally in detective fiction. It arose from the method by which positions of stars in the sky used to be measured. Start with a telescope that is free to swing in only one direction, perpendicular to the horizon, in the north-south ("meridian") direction. Then the precise time a star passes through the center of your field

of view provides its east-west position (right ascension) and the distance above the horizon at which it passes provides the north-south position (declination), at least after a bit of additional arithmatic. Standard operating procedure involved two astronomers, one with a list of stars to be observed, an accurate clock, and a pencil to keep records, and the other with his eye to the telescope, to say "now" each time the next star passed the cross-hairs in his eyepiece. Feel free to replace the pencil by a pen in this narrative and "now" by "nunc" or some other one-syllable equivalent.

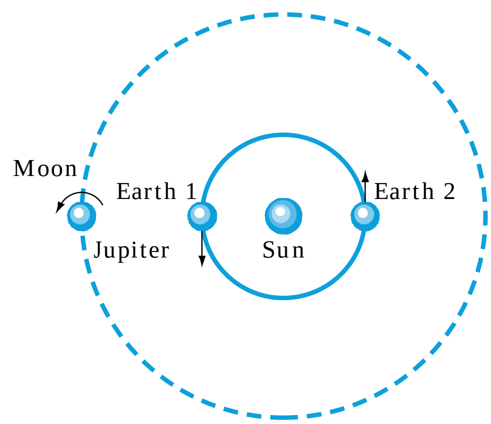
Comparison of positions obtained by different astronomers at different observatories soon showed that the identity of the record-keeper didn't matter very much, but the identity of the now-sayer did, especially for the east-west coordinate. In the words of a 1926 text: "Even the best observers habitually note the passage of a star across the fixed wires of the reticule slightly too late or too early, by an amount which is different for each observer. This *personal equation* is an extremely troublesome error, because it varies with the observer's physical condition and also with the nature and brightness of the object. Faint stars are almost always observed too late in comparison with bright ones . . . " Thus Prof. Apple's right ascensions for a given set of stars could easily be systematically larger or smaller than Dr. Berry's by several seconds of arc, enough to matter in many applications of positional astronomy.

In the end, the way around the problem of the personal equation was that of Rutherford, "Don't do better statistics. Do a better experiment." Other measurable quantities that, over the years, have revealed systematic offsets from one observer to another include stellar and galactic brightnesses, Doppler shifts, and classifications of stars and galaxies. And increased automation and mechanization, from photoelectric detectors to automated neural networks, have gradually made results more repeatable and impersonal (which is not quite the same as more correct). There must surely be similar cases in other sciences (the average number of species assigned to a newly-discovered genus?), and examples would be appreciated.

THE SPEED OF LIGHT, MAXWELL'S EQUATIONS, AND THE REST MASS OF THE PHOTON

Ole or Olaus* Roemer (1644–1710) actually participated in 1672 in one of the sets of observations that put the sun much further away than the distance Newton chose to use in the first edition of *Principia*, but astronomers know him as the first person to measure a reliably-finite value for the speed of light. Even before universal gravitation, Roemer had enough confidence in the uniformity of nature to suppose that the orbit periods of Jupiter's moons were constant from year to year. He thus attributed late and early sightings of their eclipses by Jupiter to light having more or less distance to travel,

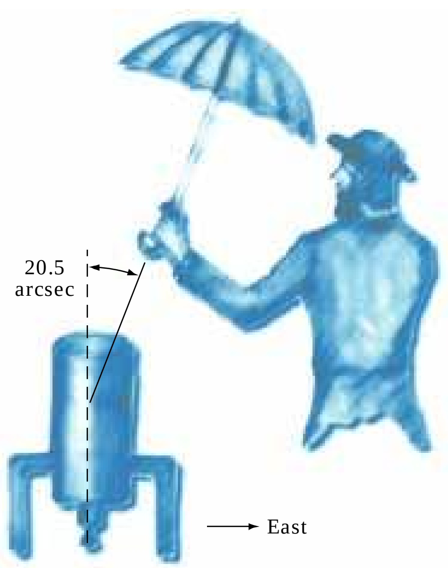
**Or any of several other spellings, but it doesn't matter which you choose because the sound is one of those Danish phonemes not available to the rest of us. It is, as Victor Borge said, a long way from "ugghrl" to "thhhh." Incidentally, Roemer also invented the transit circle and meridian circle instruments whose use revealed the "personal equation" of the previous section, and the mercury thermometer, whose use was germane to the discovery of the "mechanical equivalent of heat" of the next section.*



Each of the four Galilean moons of Jupiter is eclipsed once every 1.7 to 16.7 days. When the Earth is at E_1, we see the eclipses occurring too early by about 8.5 minutes. You can't quite observe Jupiter when we are at E_2 (the sun is in the way), but the eclipses will seem to be occurring somewhat too late any time we are in that part of the orbit. The Earth's orbit is really only one-fifth the size of Jupiter's, not about one-third as shown.

depending on the relative orbital positions of the planets. The data available to him slightly over-estimated the range of early-to-late and slightly underestimated our distance from the sun, leading to a number for c within ten percent of the current best value (which can never be improved again, since it is now a definition!).

Halley (who also had a comet) updated Roemer's numbers in 1694, and uninterest flowered for another generation. Then James Bradley found the same sort of number by a very different method. Bradley (like Herschel) was looking for stellar parallax, but discovered the oddly-named aberration of starlight. Think of photons as raindrops descending upon a moving observer. The angle you have to tilt your umbrella (telescope) forward to catch the drops (photons) is the ratio of your speed to the speed of light. Bradley saw this 20.5 arcsec tilt in 1725, had understood it by 1729, and, armed with a better idea of the size of the solar system and the Earth's speed in it, improved on Roemer's number for c.



An astronomer unclear on the concept of aberration of starlight attempts to shield his telescope from photons by holding an umbrella over it at an angle of 20.5 arcsec to the vertical. 20.5 arcsec = 10^{-4} radians = v_e/c, where v_e is the Earth's orbital velocity.

Notice that sufficiently accurate versions of Roemer's and Bradley's observations could, in principle, take the place of the Michelson-Morley experiment, because one can carry them out with the Earth's orbital velocity making various angles with the incoming light. In fact, the physicists finally took over, and the first laboratory value of c came from Fizeau in 1849. It was about as good as the astronomical ones, but improved methods, from Cornu, Foucault, and (of course) Albert Abraham Michelson quickly took the lead in providing additional significant figures (1850+) and, eventually, evidence for non-dependence on the motion of source or observer (1887+).

Meanwhile, back at the Cambridge ranch, James Clerk Maxwell was busy writing down the four equations, knowledge of which is frequently taken as the minimum requirement for calling yourself a physicist. Constancy of c is built in, along with the absence of magnetic monopoles. Astronomers quickly came to terms with the former; the latter you may well wonder about. We quite blithely ascribe polarization of radio (etc.) radiation to synchrotron emission in magnetic fields strung out for kiloparsecs and more along the arms of spiral galaxies and the jets and lobes of quasars. If an experimental physicist wise in the ways of laboratory plasmas comes along and asks where are (or at least were) the electric currents that sustain (or at least produced) these magnetic fields, the answer is quite often, "eh?" On the other hand, you get back the curious fact that the rest mass of the photon must be less than 10^{-47} to 10^{-57} g, in order for fields to persist over large scales from Jupiter to a galaxy.

The corresponding limit on the mass of the graviton (from the existence of gravitationally-bound superclusters of galaxies at least 100 million parsecs across) is about 10^{-63} g. Both are considerably smaller than laboratory limits, and occasional free spirits have taken them as real, non-zero masses.

WHAT MAKES THE SUN SHINE?

The source of solar and stellar energy was the most important unsolved problem in astronomy/astrophysics

for half a century or more, 1880±10 to 1938±2. Of course, it couldn't be a problem until conservation of energy was recognized as **a** universal phenomenon, which it was not by Galileo or Newton or even William Herschel (though he speculated on the role of chemical interactions in the atmosphere above the solid(!) surface of the sun).

Energy conservation became part of laboratory physics sometime between 1798, when Benjamin Thompson (Count Rumford) reported to the Royal Society (London) on "Experimental inquiry concerning the source of heat excited by friction" and 1849, when James Joule reported to them "On the mechanical equivalent of heat." The German physician Julius Robert Mayer is generally credited with the first proposal of energy conservation in full generality in 1841. By this time, the Earth was already at least tens of millions of years old, according to Hutton, Lyell, and other uniformitarian* geologists who had thought about how long it must have taken to build up sedimentary rocks, make the oceans salty, and so forth. Chemical reactions, which might sustain the sun for a few thousand years, were, therefore, never serious contenders, except on Archbishop Ussher's chronology, and, even then, the end would be at hand.

In fact, Mayer soon proposed a solution to the solar problem he had identified. It was gravitational potential energy, liberated by infalling meteors. Independently, the Scots engineer John James Waterston recognized the problem of solar energy in 1843 and proposed as a solution the continuous contraction of the sun, following on from its origin in a Kantian rotating nebula. Mayer's paper was rejected by the Academy in Paris, Waterston's by the Royal Society in London.

A contracting star can live about GM^2/RL years on gravitational potential energy, a number that should obviously be called the Mayer-Waterston time scale, but is actually called the Kelvin-Helmholtz time scale, for those who put forward the same ideas in about 1854 (Kelvin with meteors initially, Helmholtz with overall contraction). Both had encountered Waterston's work, in a two-page abstract arising from an 1853 meeting of the British Association for the Advancement of Science and containing both the meteoric and the contraction possibilities. Kelvin and Helmholtz were, therefore, presumably at least guided by their predecessors, but they had better credentials, better press agents, and better formal mathematics at their disposal.

Uniformitarianism is not, at least in intention, a religion, but just the notion that we see in continuous operation now all the important processes that have shaped the Earth. The opposite is catastrophism, and, as usual, the truth is somewhere in between.

There followed a brief era of good feeling, in which most geologists squeezed hard on their layers of sediment to compress them within 20–40 million years, though a few refused, including T. C. Chamberlin of Chicago (whom we recognize as the first half of the Chamberlin-Moulton hypothesis for the origin of the solar system, once a popular competitor to Kantian contraction). Chamberlin and Kelvin faced off at the 1900 meeting of the American Association for the Advancement of Science, neither, of course, changing his mind.

In any case, the solar boat was soon set further rocking, this time by the biologists, who had taken the ideas of Darwin to heart (and head). They insisted on at least 10^9 years for evolution from slime molds to Kelvins. The physicists clambered back aboard starting in 1905, when Rutherford and Boltwood (independently) reported rock ages of many gigayears after considering the decay of uranium and thorium to lead. The numbers settled down around $3–4\times10^9$ yr in 1913, when Soddy weighed in with the concept of isotopes.

A PAIR OF JEANS (Sir James)

You might think at this point that we are well poised to romp home with Eddington, Bethe, and all to hydrogen fusion as the primary source of stellar energy. But real events took several detours, which it would not be fair to blame entirely on James Jeans.* Jeans was, however, the person who most strongly insisted that the real age of stars and stellar systems was more like $10^{12–13}$

After all, it was Eddington who declared that the sun was made mostly of iron, silicon, and oxygen, so that radiation pressure and gas pressure were equal at its center.

years than 10^{9-10}. This "long chronology" arose when Jeans tried to account for the relaxed appearance of stars, clusters, and (after 1925) galaxies and for the observed distribution of the shapes of orbits of binary stars. The binary star case, at least, was an example of the theorem that most mistakes are made before you ever put pencil to paper. Jeans postulated that all binaries had formed with circular orbits, which were gradually distorted into ellipses by encounters with other stars. Stars are VERY far apart, and so this will take a VERY long time: in fact, just about the time Jeans calculated. Like most British astronomers of his and later generations, he had a background in formal mathematics, and did not often make mistakes after putting pencil to paper. Modern observations of binary systems of different ages show, however, that they form with eccentric orbits that are gradually circularized by tidal interactions between the stars.

Meanwhile, however, Jeans' time scale led him to require a source of stellar energy in which mass was completely annihilated and 100 percent of mc^2 made available. That physics knew of no process to do this was not, to him, a fatal objection. Indeed, at the time, experimental physics was not very clear on how to liberate any form. of "subatomic energy" except through spontaneous radioactivity.

There were even times when the long chronology seemed to be winning. Rutherford's definitive 1929 statement on the age of earth rocks from uranium decay (3.4 Gyr) includes the remark that this number proves the sun must be making uranium more or less currently, since the total solar age is 100 times longer. Similar remarks appear in other papers and books by both physicists and astronomers up until about 1935 when the "short" time scale of a few $\times 10^9$ yr triumphed. This seems to have come from approximate agreement among (a) the age of the Earth, (b) the expansion time scale of the Universe, and (c) the time stars could live on nuclear transformations without annihilation. Even Jeans capitulated, in the last, 1944, edition of one of his popular books.

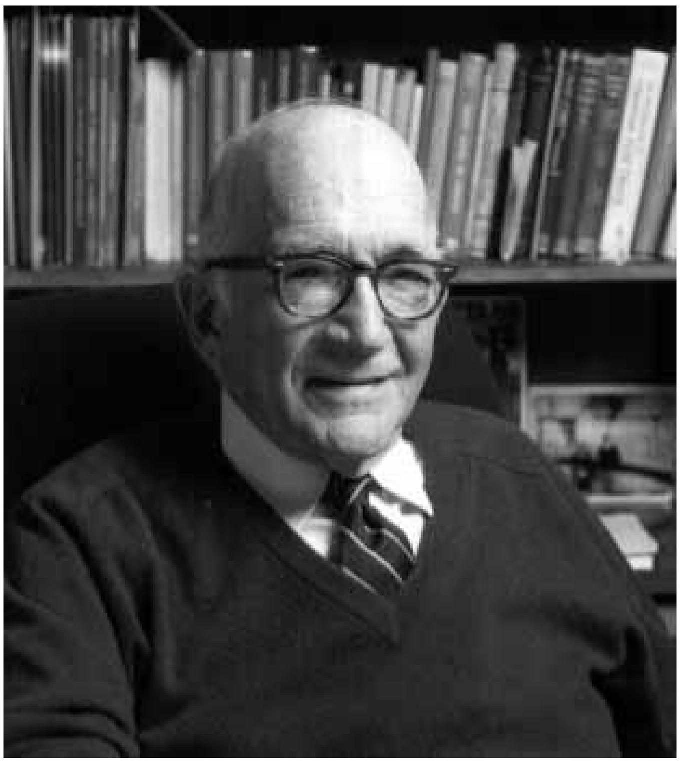
E. A. Milne, whom we have not met before in these pages, was an equal muddier of the waters of stellar physics in the 1920s and 30s, and his ideas are even harder for the modern reader to appreciate than those of Eddington and Jeans. Leon Mestel has made the intriguing suggestion that it would all have been sorted out much sooner if Karl Schwarzshild had survived the Great War and had been available to bang their recalcitrant heads together.

BACK ON TRACK

A second seeming detour was actually the path back to the main highway. This was the association of the stellar energy problem with that of the origin of the elements. Heroes of the tale include Aston with his mass spectrograph (1922+), Atkinson and Houtermans with cyclic nuclear reactions that both built up heavy elements and released a few MeV per nucleon (1929), and Gamow, Condon, and Gurney with barrier penetration. They and others are hymned at greater length in an earlier *Beam Line* (Spring 1994, Vol. 24, No. 1), ending with two choruses by Hans Bethe, the first dated 1939 (when he wrote down most of the details of the hydrogen fusion reactions that we now call the CN cycle and proton-proton chain), the second dated about 1990 (when he added his voice to the choir proclaiming that the solar neutrino deficit is a problem in weak interaction physics, not in astronomy or argon chemistry).

Nucleosynthesis (the formation of complex atoms from simple ones) and the production of energy in stars are now solved problem, though much input was required from nuclear physics and from the area of spectroscopy, to which we will turn in Part II in the Winter issue.

CONTRIBUTORS



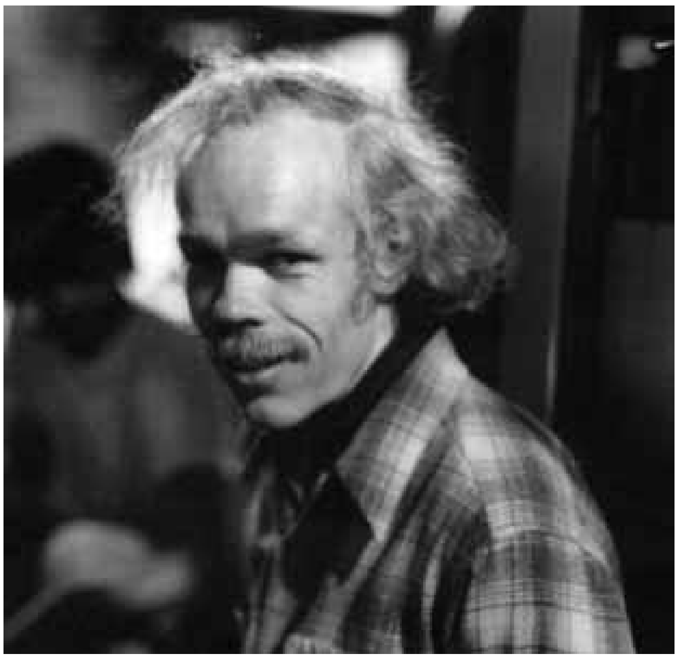
Sharon Kinder-Geiger

SID DRELL was educated at Princeton and the University of Illinois. After short stints on the faculty at Stanford and MIT he returned to Stanford as a physics professor in 1956. When it was created in 1963, he transferred to SLAC where he served as head of theoretical physics from 1969 to 1986, and Deputy Director.

An advisor to the U.S. government on technical issues of national security and arms control for many years, he is currently a member of the President's Foreign Intelligence Advisory Board and chairs the council overseeing the Livermore, Los Alamos, and Berkeley National Laboratories for the UC president. He is a member of the National Academy of Sciences, was President of the American Physical Society in 1986, and was head of the High Energy Physics Advisory Panel from 1974 to 1983. Among his awards was a MacArthur Fellowship in 1984. He is an avid amateur string quartet player.

Educated in England, JOHN ELLIS obtained his BA and PhD from Cambridge University. After one year each at SLAC and the California Institute of Technology as research associate, he settled at CERN in 1973, where he led the Division of Theoretical Physics for six years, and is currently a senior staff member. He is the author of over 500 scientific articles, mainly in particle physics and related areas of astrophysics and cosmology.

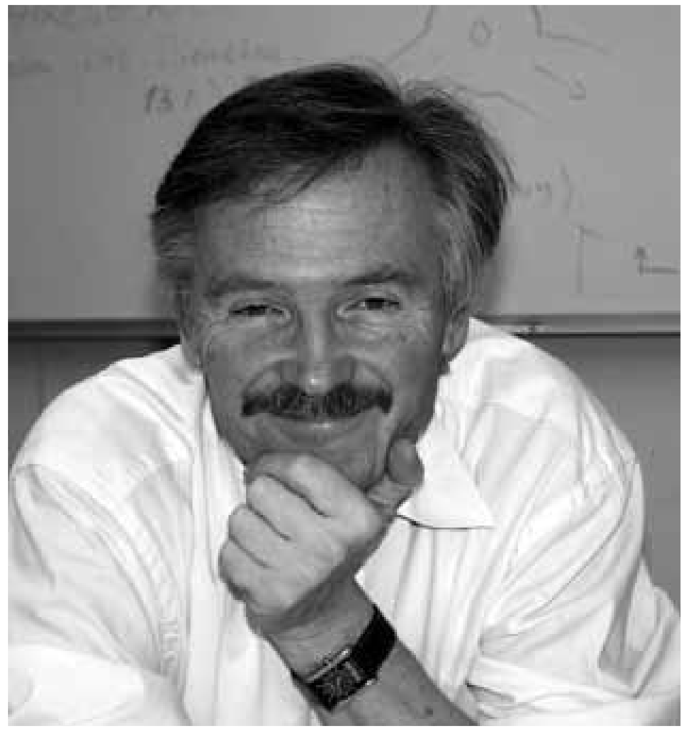
Most of his research work has been on the possible experimental consequences and tests of new theoretical ideas such as gauge theories of the strong and electroweak interactions, grand unified theories, supersymmetry, and string theory. He was awarded the Maxwell Medal of the Institute of Physics in 1983 and was elected a Fellow of the Royal Society in 1985. The University of Southampton awarded him an Honorary Doctorate in 1994, and he has held visiting appointments at Berkeley, Cambridge, Oxford, Melbourne, and Stanford.



Rainer Pitthan

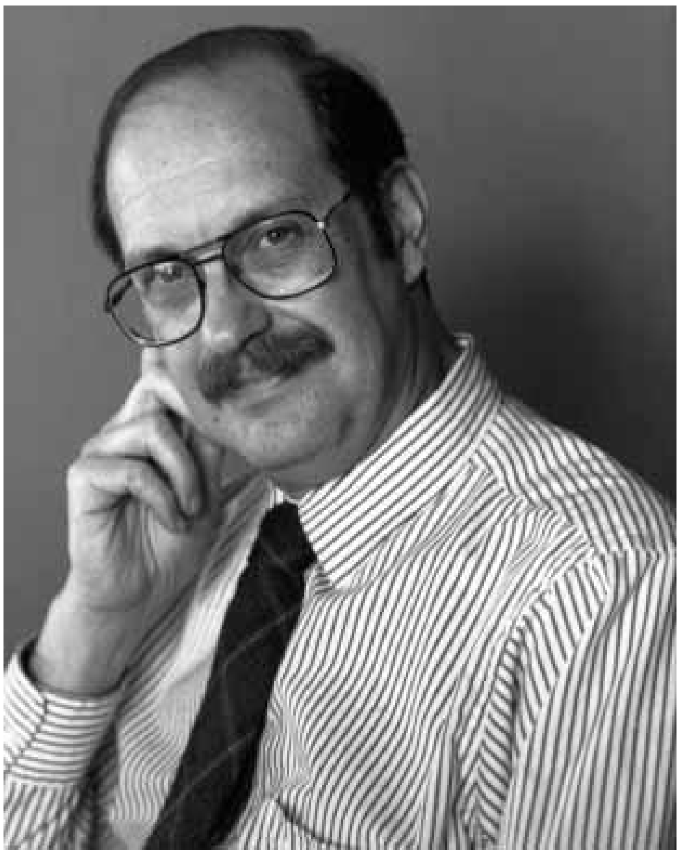
BILL ATWOOD has been a physicist at SLAC since 1975. He received his BS from the California Institute of Technology and PhD from Stanford University. He has co-authored over 130 articles in the field of particle physics.

Raised in the greater Boston area, he began playing the piano when he was six years old and the violin at eight. Because his father was a part-time furniture maker who specialized in copies of French Provincials, he received early training in wood working. He became interested in violin making in 1976 and began making instruments in 1983. He has made more than forty violins and violas which are being lovingly played by Bay Area musicians. Recently a young violinist from the University of California, Santa Cruz, won a concerto competition on an Atwood violin.



HEINO HENKE is Professor and head of the Electrical Engineering department of the Technische Universitaet of Berlin. He was educated at the Technische Hochschule in Darmstadt where he also received his doctoral degree. Beginning his career in biocybernetics, he later switched to accelerator physics when he became a staff member at CERN in 1977. Recently, he spent six months at SLAC working on millimeter-wave radio-frequency structures. His research interests include rf technology, electromagnetic fields, beam-coupling impedances, new accelerating techniques, and linear colliders.

During a research stay in the radio-frequency group of the Advanced Photon Source at Argonne National Laboratory the idea was born to combine modern microtechnology with accelerator physics. Since then, millimeter-wave radiofrequency structures are one the main research topics in his Berlin group.



ROBERT SIEMANN is a Professor at SLAC and Head of Accelerator Research Department B. He was educated at Brown and Cornell Universities and spent seventeen years on the faculty at Cornell before coming to SLAC in 1991. He has worked on a number of high energy accelerators, including CESR, the Tevatron, and the SLAC Linear Collider, and has written extensively on accelerator theory, experiment, and technology. His present interests are advanced accelerator techniques, and he is involved in development of mm-wave accelerators and experiments on laser and plasma based acceleration. He is the Founding Editor of the newly established journal *Physical Review Special Topics—Accelerator and Beams*.

VIRGINIA TRIMBLE continues to oscillate among appointments at two universities (University of California Irvine and University of Maryland), vice presidencies, and council memberships and such in at least six professional organizations, an assortment of editorial boards, peer review panels, and lectures. She is currently feeling a bit frazzled but hopes it doesn't show too much in this quarter's column.