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by SIDNEY DRELL

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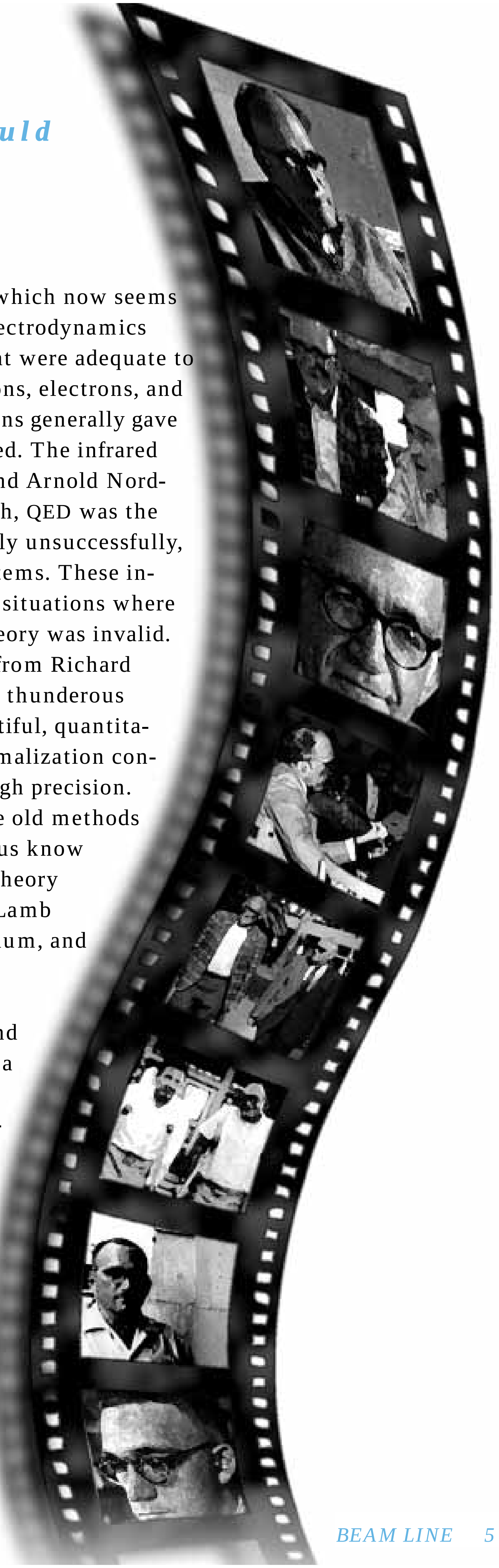
MY CAREER IN RESEARCH as a theoretical physicist dates back to fifty years ago shortly after the end of World War II. And it has been the best of times. Back then, it was a dream time to have been a graduate student! There was no need to worry about a job, unless for some strange reason you felt that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

"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

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

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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 nonpartisan 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 nongovernmental 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 nonproliferation 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


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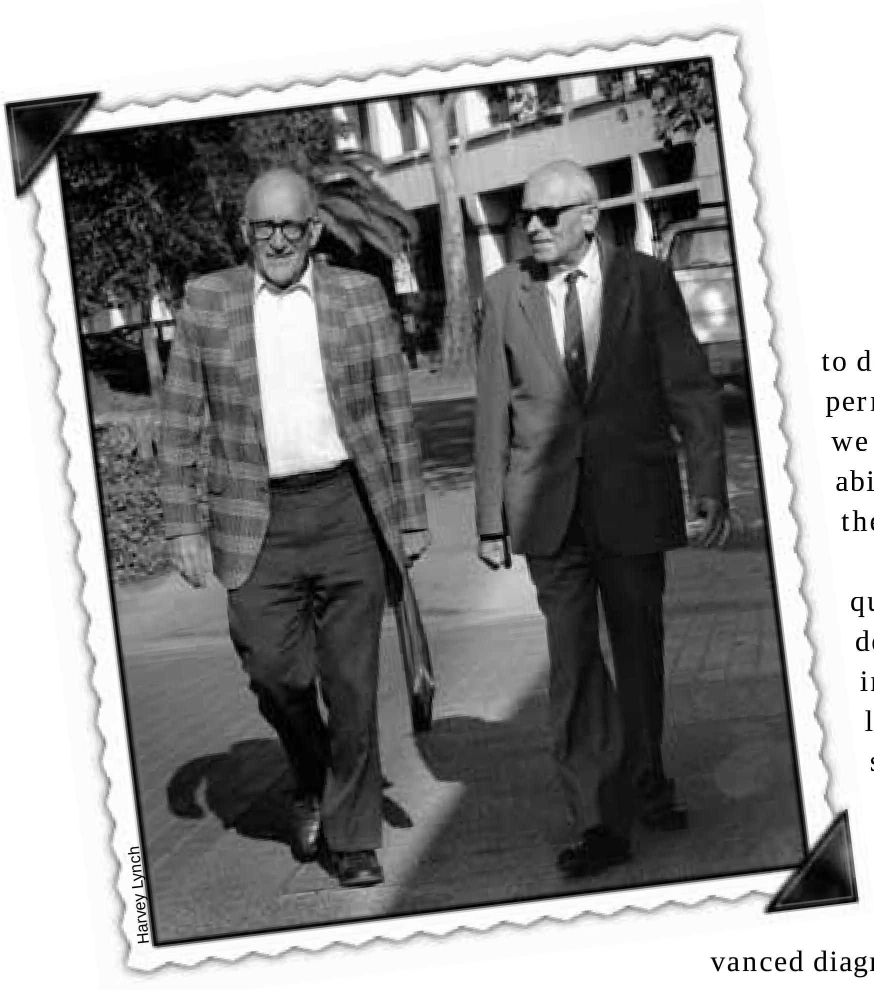

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to determine what would have to be included as permitted activities in a negotiated CTBT, so that we could retain confidence in the safety and reliability of our enduring nuclear warheads into the future.

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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