FERMILAB–CONF–02/048–T

Why We Are Here

Chris Quigg*
Fermi National Accelerator Laboratory
P.O. Box 500, Batavia IL 60510 USA

(Dated: March 12, 2002)

Opening talk at *Snowmass 2001:* a summer study on the future of particle physics.

I. WELCOME!

On behalf of the American Physical Society, the Division of Particles and Fields, and our partners in the Division of Physics of Beams, it is my great pleasure to welcome you to *Snowmass 2001:* a summer study on the future of particle physics. We are off to a wonderful start, with more than 1000 participants already registered. Thanks in part to generous support from the National Science Foundation, the DPB, and the DPF, more than eighty students have come to Snowmass. The students form part of a contingent of more than 200 of our colleagues who are, by their own admission—no questions asked!—"young."

I'd also like to add a special welcome, and hearty thanks, to the more than 150 participants who have come to Snowmass from outside the United States. We look forward to drawing on your expertise and your perspectives as we try to shape the future of our subject. It is plain to everyone, I hope, that international cooperation will make many more futures possible: the choices we make can be when and where, not just yes to this and no to that.

The exceptional infrastructure and stimulating ambience we find here at Snowmass would not have been possible without the enthusiastic support—material and otherwise—of our sponsors. Our funding agencies, the United States Department of Energy, the National Science Foundation, and NASA, have provided resources and encouragement. The APS Division of Particles & Fields and Division of Physics of Beams made substantial contributions to the outreach effort—which also benefited from very considerable donations from private foundations that wish to remain anonymous—and to student support. Another of our professional organizations, the Nuclear and Plasma Sciencies Society of the IEEE, has organized and financed an all-star "technology emphasis" that will run throughout the three weeks of Snowmass 2001. We're also extremely grateful for financial support and the show of solidarity from ten laboratories engaged in particle physics research in the United States: Argonne National Lab, Berkeley Lab, Brookhaven National Lab / Brookhaven Science Associates, Cornell University / LNS / Wilson Synchrotron Lab, Fermilab / Universities Research Association, Jefferson Laboratory / SURA, Lawrence Livermore National Laboratory, Los Alamos National Laboratory, Oak Ridge National Lab / Spallation Neutron Source, Stanford Linear Accelerator Center / Stanford University. We thank all these institutions for their commitment to particle physics, and to Snowmass 2001, and we salute the organizations behind our great laboratories for their stewardship.

The stimulating and inclusive program of activities we will enjoy during the next three weeks is the creation of the Snowmass Organizing Committee. It has been a joy to work with these thoughtful, creative people: they have given enormous amounts of time, energy, and goodwill to the task of building a framework in which we can explore the whole range of scientific opportunities and confront many kinds of issues that will influence the conduct of particle physics in the future. Because he cannot be here to see the fruits of his labors, I especially want to call attention to the energetic leadership and great wisdom of Ron Davidson, the DPB co-chair of the organizing committee. Ron brought wonderful ideas and a confident serenity to the committee's work. Ron and I join in thanking our colleagues Sally Dawson (BNL), Paul Grannis (Stony Brook), David Gross (ITP/UCSB), Joe Lykken (Fermilab), Hitoshi Murayama (Berkeley), René Ong (UCLA), Natalie Roe (LBNL), Heidi Schellman (Northwestern), and Maria Spiropulu (Chicago) from the DPF side; and Alex Chao (SLAC), Alex Dragt (Maryland), Gerry Dugan (Cornell), Norbert Holtkamp (SNS), Chan Joshi (UCLA), Thomas Roser (BNL), Ron Ruth (SLAC), John Seeman (SLAC), and Jim Strait (Fermilab) from the DPB side.

As we begin our adventure, I want also to thank the local organizing committee, whose work you will see all around you over the next three weeks, and our many working-group convenors from around the world, who have responded so enthusiastically to their charges and will give definition to our work together.

II. THE STATE OF PARTICLE PHYSICS

The physics curriculum in the 1898–99 University of Chicago catalogue begins with a very triumphalist Victorian preface [1]:

> "While it is never safe to affirm that the future of the Physical Sciences has no marvels in store even more astonishing than

---

[*] E-mail: `quigg@fnal.gov`; Fermilab is operated by Universities Research Association Inc. under Contract No. DE-AC02-76CH03000 with the United States Department of Energy.

> those of the past, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice .... An eminent physicist has remarked that the future truths of Physical Science are to be looked for in the sixth place of decimals."

As the ink was drying on these earnest words, Röntgen discovered x rays and published the epoch-making radiograph of his wife's hand, Becquerel and the Curies explored radioactivity, Thomson discovered the electron and showed that the "uncuttable" atom had parts, and Planck noted that anomalies in the *first* place of the decimals required a wholesale revision of the physicist's conception of the laws of Nature.

We have the benefit of a century of additional experience and insight, but we are not nearly so confident as our illustrious Victorian ancestors were that we have uncovered "most of the grand underlying principles." Indeed, while we celebrate the insights codified in the *standard model of particle physics* and look forward to resolving its puzzles, we are increasingly conscious of how little of the physical universe we have experienced and explored. Future truths are still to be found in precision measurements, but the century we are leaving has repeatedly shown that Nature's marvels are not limited by our imagination. Exploration can yield surprises that completely change what we think about—and how we think.

A. A Decade of Discovery Ahead

Over the next decade, we look forward to an avalanche of experimental results that have the potential to change our view of the fundamental particles and their interactions in very dramatic ways. A special preoccupation for me is the search and study of the Higgs boson; this is really shorthand for a thorough exploration of the 1-TeV scale, which will elucidate the mechanism of electroweak symmetry breaking. We can also expect wonderful progress in flavor physics: the detailed study of CP violation in the B system, dramatically increased sensitivity in the exploration of rare decays of K and D mesons, and pinning down the nature of neutrino oscillations. Maybe we will at last see a CP-violating permanent electric dipole moment of the neutron. Run II of the Tevatron will give us our first opportunity to regard the top quark as a tool, and not only as an object of desire. Although the interpretation of heavy-ion collisions at RHIC and the LHC promises to be challenging, the heavy-ion colliders offer a real chance to discover new phases of matter and enrich our understanding of QCD.

On many fronts, we are taking dramatic steps in energy and sensitivity that will help us *explore:* extra dimensions, new dynamics, supersymmetry, and new kinds of forces and constituents might show themselves. (I'm conflicted about whether I'd like to see them all at once, or in easy-to-understand installments!)

Experiments that use natural sources also hold great promise for the decade ahead. We suspect that the detection of proton decay is only a few orders of magnitude away in sensitivity. Astronomical observations should help to tell us what kinds of matter and energy make up the universe. The areas already under development—if not exploitation—include gravity wave detectors, neutrino telescopes, cosmic microwave background measurements, cosmic-ray observatories, γ-ray astronomy, and large-scale optical surveys. Indeed, the whole complex of experiments and observations we call astro/cosmo/particle physics should enjoy a golden age.

Here at Snowmass, we will have the opportunity to consider many imaginative ideas for instruments and experiments that lie beyond our current horizon. Although theoretical speculation and synthesis is valuable and necessary, we cannot advance without new observations. The experimental clues needed to answer today's central questions can come from experiments at high-energy accelerators, experiments at low-energy accelerators and nuclear reactors, experiments with found beams, and deductions from astrophysical measurements. Past experience, our intuition, and the current state of theory all point to an indispensable role for accelerator experiments.

The opportunities for accelerator science and technology are multifaceted and challenging, and offer rich rewards for particle physics.

One line of attack consists in refining known technologies to accelerate and collide the traditional projectiles—electrons, protons, and their antiparticles—pushing the frontiers of energy, sensitivity, and precise control. The new instruments might include brighter proton sources; very-high-luminosity e^+e^- "factories" for B, τ / charm, ϕ, ...; a Tevatron "Tripler" based on high-field magnets; cost-effective hadron colliders beyond the LHC at CERN, represented by the Super-LHC and Very Large Hadron Collider initiatives; and e^+e^- linear colliders.

A second approach entails the development of exotic acceleration technologies for standard particles: electrons, protons, and their antiparticles. We don't yet know what instruments might result from research into new acceleration methods, but it is easy to imagine dramatic new possibilities for particle physics, condensed matter physics, applied science, medical diagnostics and therapies, and manufacturing, as well as a multitude of security applications. A teach-in on July 5 will explore opportunities to become involved in research on advanced acceleration methods.

A third path involves the exploration of exotic particles for accelerators and colliders to expand the experimenter's armamentarium. Muon storage rings for neutrino factories, $\mu^+\mu^-$ colliders and $\gamma\gamma$ colliders are all under active investigation, and each of these would bring remarkable new possibilities for experiment.

Finally, let us note the continuing importance of enabling technologies: developing or domesticating new materials, new construction methods, new instrumentation, and new active controls. I call your attention to the IEEE Nuclear and Plasma Sciences Society's program of Technology Short Courses and Lunchtime Lectures, beginning on July 5.

To a very great extent, the progress of particle physics has been paced by progress in accelerator science and technology. A renewed commitment to accelerator research and development will ensure a vigorous intellectual life for accelerator science and lead to important new tools for particle physics and beyond.

B. Creating the Future

Now, the decade of discovery won't happen automatically. Many of our goals are difficult, and timely success is in doubt for many experiments. We must push hard to prepare the instruments, and get to the answers.

The glorious future of new machines and new experiments that lies beyond the established program also won't happen by itself. We have, I think, come to the collective realization that we must do more to prepare alternative futures by creating a rich and organic program of accelerator research.

We're also challenged by our success: *the scope of our science has grown, but funding has not.* Within our own extended family and beyond, we must do more to convey the urgency and importance of the new scientific opportunities, and fashion a program that we can carry out that includes the right measure of scale diversity to ensure a healthy intellectual ecosystem.

Many individuals in our community, all the major laboratories, and many university departments work energetically—and effectively—in outreach and educational activities, conveying the excitement and substance of science—and particle physics—to students, and to the general public. But we are not doing everything we might to promote scientific literacy, to inspire the next generation of scientists, engineers, and technologists, and to report to our patrons—our fellow citizens in government and in the general public—our hopes and dreams, our triumphs and challenges. We can communicate much more effectively the wonders of our science. It is in our interest to do so, and it is our obligation to those who support our work.

To show how seriously we take the need to present our science to others, we have expanded the usual technical program of a Snowmass summer study to include a vigorous and diverse program of outreach and educational activities—right here in the Roaring Fork Valley. The presence of Quarknet teachers, the Science Weekend extravaganza on the Snowmass Mall next weekend, and a schedule of public lectures in Aspen, Snowmass, and Carbondale are only part of the story. I hope that many of you will participate in the outreach program, and that all of you will take time to see what your colleagues are doing and to think about what more you might be doing at home.

I also call your attention to the Communications workshops scheduled this week and next, and to the "Working with Governments" forum near the end of the summer study. To add to the literature that presents the achievements and aspirations of particle physics to a broad audience, the Division of Particles and Fields is preparing an illustrated thematic survey entitled *Quarks Unbound*.

C. What We Need to Know

Plans that proceed from broad scientific goals to specific questions and then to instruments and technology development have been used to excellent effect by the National Cancer Institute and by NASA. In organizing my thoughts about our future, I find it useful to consider the agenda of particle physics today under a few broad rubrics.

Elementarity. Are the quarks and leptons structureless, or will we find that they are composite particles with internal structures that help us understand the properties of the individual quarks and leptons?

Symmetry. One of the most powerful lessons of the modern synthesis of particle physics is that (local) symmetries prescribe interactions. Our investigation of symmetry must address the question of which gauge symmetries exist (and, eventually, why). We have learned to seek symmetry in the laws of Nature, not necessarily in the consequences of those laws. Accordingly, we must understand how the symmetries are hidden from us in the world we inhabit. For the moment, the most urgent problem in particle physics is to complete our understanding of electroweak symmetry breaking by exploring the 1-TeV scale. This is the business of the experiments at LEP2, the Tevatron Collider, and the Large Hadron Collider.

Unity. In the sense of developing explanations that apply not to one individual phenomenon in isolation, but to many phenomena in common, unity is central to all of physics, and indeed to all of science. At this moment in particle physics, our quest for unity takes several forms.

First, we have the fascinating possibility of gauge coupling unification, the idea that all the interactions we encounter have a common origin and thus a common strength at suitably high energy.

Second, there is the imperative of anomaly freedom in the electroweak theory, which urges us to treat quarks and leptons together, not as completely independent species. Both of these ideas are embodied, of course, in unified theories of the strong, weak, and electromagnetic interactions, which imply the existence of still other forces—to complete the grander gauge group of the unified theory—including interactions that change quarks into leptons.

The third aspect of unity is the idea that the traditional distinction between force particles and constituents might give way to a unified understanding of all the particles. The gluons of QCD carry color charge, so we can imagine quarkless hadronic matter in the form of glueballs. Beyond that breaking down of the wall between messengers and constituents, supersymmetry relates fermions and bosons.

Finally, we desire a reconciliation between the pervasive outsider, gravity, and the forces that prevail in the quantum world of our everyday laboratory experience.

Identity. We do not understand the physics that sets quark masses and mixings. Although we are testing the idea that the phase in the quark-mixing matrix lies behind the observed CP violation, we do not know what determines that phase. The accumulating evidence for neutrino oscillations presents us with a new embodiment of these puzzles in the lepton sector. At bottom, the question of identity is very simple to state: What makes an electron and electron, and a top quark a top quark?

Topography. "What is the dimensionality of spacetime?" tests our preconceptions and unspoken assumptions. It is given immediacy by recent theoretical work. For its internal consistency, string theory requires an additional six or seven space dimensions, beyond the $3 + 1$ dimensions of everyday experience. Until recently it has been presumed that the extra dimensions must be compactified on the Planck scale, with a stupendously small compactification radius $R \simeq M_{\text{Planck}}^{-1} = 1.6 \times 10^{-35}$ m. Part of the vision of string theory is that what goes on in even such tiny curled-up dimensions does affect the everyday world: excitations of the Calabi–Yau manifolds determine the fermion spectrum.

We have recognized recently that Planck-scale compactification is not—according to what we can establish—obligatory, and that current experiment and observation admit the possibility of dimensions not navigated by the strong, weak, and electromagnetic interactions that are almost palpably large. A whole range of new experiments will help us explore the fabric of space and time, in ways we didn't expect just a few years ago.

III. SOME GOALS FOR SNOWMASS 2001

All of you have arrived in Snowmass with ambitious plans for the next three weeks. The charges to the twenty-seven working groups give a detailed picture of the Organizing Committee's vision of what we can achieve, but here are the high-level goals I hope we can attain together:

▷ Survey our aspirations for particle physics over 30 years.

▷ Assess the current state of development of accelerator protoprojects and advanced accelerator research, and understand the investment we must make (financial and human capital) to bring the most promising lines to maturity. Here at Snowmass, the Division of Physics of Beams will be completing a very broad look at our opportunities and needs for Accelerator R&D.

▷ Look beyond our immediate goals for measurements and searches to contemplate the shape of a more complete, more ambitious theoretical framework. How should theoretical vision shape our experimental goals?

▷ Examine the importance of scale diversity for a healthy and productive future.

▷ Educate ourselves about the full range of possibilities before us. We must know enough to judge critically, to improve the arguments, to articulate our goals effectively. We're very grateful to the senior scientists who have agreed to serve as HMOs—high-minded outsiders—in experimental working groups E1 – E6. Their assignment is to act as friendly skeptics, probing and strengthening arguments, and—by the example of their time and effort—to lead many others to view Snowmass 2001 as a forum for engaging with the ideas and aspirations of others.

▷ Listen carefully to our young colleagues, who will help create our common futures. We'll have a formal opportunity to hear from the next generation at the Young Physicists Forum on July 17, so mark that date on your calendar.

▷ Take advantage of opportunities to interact with the HEPAP Subpanel. Technical work carried out at Snowmass will undergird the recommendations the subpanel makes.

▷ Consider the international dimensions of what we hope to achieve. It will be a particular pleasure to welcome the international lab directors, and I call your attention to an evening discussion this week on the issues of a global accelerator network. During the final week of Snowmass 2001, we'll have reports from the European and Japanese high-energy physics planning Committees

I believe we must articulate a comprehensive vision of particle physics (and the sciences it touches) to make our case effectively to ourselves, to other scientists, and to society at large. At the same time, we have a special responsibility to examine the prospects for the most ambitious accelerators, which are major drivers of our scientific progress. If we judge the science to be rich, and if we can make the cost and technical risk attractive, we will want to pursue all the leading possibilities: linear colliders, hadron colliders reaching far beyond the TeV scale, muon storage ring, and muon collider. The vision we present should include the scientific promise of all these instruments, and a strategy for deciding what, where, and when that includes the organic R&D investment we will need to evolve the right set of instruments to serve our science.

▷ Thanks to the work of many people, the moment is upon us to probe, shape, and judge the idea of a linear collider as a possible next big step for particle physics. *Evaluating a linear collider and working to define a scientifically rich, technically sound, fiscally responsible plan is a homework problem for the entire community.* Everyone must come to an informed judgment.

Beyond the technical issues, please think about how to make our dreams happen. Creating a future is not accomplished when we draw our individual conclusions or read the subpanel recommendations.

Welcome to Snowmass 2001! Your passion, energy, creativity, and commitment will change the world.

[1] S. B. Treiman. *The Odd Quantum.* Princeton University Press, Princeton, 1999.