Physics Focus and Fiscal Forces

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Two items are reproduced herein: my ‘Outlook’ talk, an amended version of which was presented at the 1991 joint Lepton–Photon and EPS Conference in Geneva, and an Open Letter addressed to HEPAP. One is addressed primarily to the European high–energy physics community, the other to the American. A common theme of these presentations is a plea for the rational allocation of the limited funds society provides for high–energy physics research. If my ‘loose cannon’ remarks may seem irresponsible to some of my colleagues, my silence would be more so.
1. OUTLOOK

Just a few months after CERN’s triumphant observations of $Z^0$ and $W^\pm$ bosons, Harvard celebrated the centenary of its Jefferson Physical Laboratory. Carlo Rubbia, then one of our most illustrious faculty members, described these discoveries and more: he spoke with gusto of certain curious events — monojets — which could not be explained with the standard model of particle physics. We have learned a great deal since then. There are no inexplicable monojets and there has not been one demonstrable failure of the standard model. QCD and the electroweak theory reign supreme. They offer a complete, correct and consistent description of all known elementary–particle phenomena. Rubbia is now the director–general of a laboratory that seems forever destined to confirm the predictions of what we must now call the standard theory. His punishment is fit as a modern–day myth.

Some physicists look upon the current situation as a triumph of human ingenuity, since many of the most puzzling problems of the past are solved. The rich and complex spectroscopy of hadrons is ‘understood’ at last in terms of the energy levels of a system of two or three interacting quarks, much as the spectroscopy of nuclei is understood in terms of constituent nucleons and that of atoms in terms of electrons. (The quotes are needed since we cannot as yet perform as precise calculations with QCD as we might wish to do.) Similarly, all of the wealth of weak–interaction phenomena is resolved in terms of the gauge interactions of three intermediate vector bosons, whose properties have been found to agree with the predictions of the electroweak theory.

Many questions lie beyond the ken of the standard theory. They remain to be answered and suggest the existence of a more powerful theory. Why are there three fermion families? Why do particles have the masses and mixings they do? Why did nature choose the gauge group $SU(3) \times SU(2) \times U(1)$? Look to history, say the optimists. The secrets of the outer atom were exposed at energies of a few eV. Those of the inner atom by X ray studies at energies of a few KeV. The nucleus was explored at MeV energies and evidence for quarks appeared at some GeV. Each great breakthrough required a leap in energy by a factor of 1000. Thus, the next great discovery to be made — the nature of electroweak symmetry breaking and the origin of mass — will be revealed in the multi–TeV domain: at the next great hadron collider.

Other physicists interpret the manifold empirical successes of the standard theory as a tragedy marking the end of an era of exciting discovery. Aside from one missing quark and
the Higgs boson, nothing is left to discover at the high–energy frontier. All that remains
is the measurement of the next decimal place. Although we have heard this refrain before,
the pessimists also consult the history of our discipline. They point to the great surprising
discoveries of the past century, which until recently have sprung upon us every few years:

| Decade | Discoveries                                                                 |
|--------|-----------------------------------------------------------------------------|
| 1890s  | X rays, rare gases, radioactivity and the electron.                          |
| 1900s  | Planck’s $h$, half–lives, photoelectric effect, special relativity.         |
| 1910s  | Cosmic rays, nuclei, the Bohr atom and the bending of starlight.             |
| 1920s  | Hubble shift, quantum theory, spin and the Pauli principle.                  |
| 1930s  | Pions, muons, neutrons, fission, continuous beta spectra.                    |
| 1940s  | Positrons, strange particles, the Lamb shift and the atomic bomb.           |
| 1950s  | Parity violation, pion–nucleon resonance, neutrinos, $V - A$.                |
| 1960s  | Muon neutrinos, scaling behavior, CP violation, hadrons aplenty.             |
| 1970s  | $J/\Psi$, neutral currents, tau leptons, charm and beauty quarks.           |
| 1980s  | SN1987a, voids and walls of the universe.                                   |

While new and wonderful things are being found out about the universe, there hasn’t been
a big surprise in our discipline since the establishment of the third family of quarks and
leptons in 1977. True, $W$ and $Z$ were first produced and detected in the 1980s, but their
reality came as no surprise. Not for well over a century has there been such an insipid
interregnum. So be it. While optimists glory in the success of the standard theory and
pessimists bemoan their fate, there remains much to be done:

- Let us not be hasty in assuming its absolute validity: the standard theory must be
  tested as best we can. With a million $Z^0$ events in hand, the theory works just fine. Will
  agreement persist when we have ten times that number? Will the deduced value of the
  muon’s $g - 2$ survive the next experimental onslaught? Is the Kobayashi–Maskawa matrix
  truly unitary? And so on.

- We must learn to exploit the standard model better. Theorists and experimenters
  must work together to devise more and better ways to extract the consequences of a
  recalcitrant theory and confront them with experiment.

- We must hope for more surprises and search for them. Nature is not the enemy;
  complacency is. No physicist should be so arrogant as to believe that her bag of tricks is
  exhausted, lest the great desert of particle physics become a self–fulfilling prophecy.

- We must worry the weak points of the standard theory. CP violation is surely
  among them. Another generation of experiments is needed to pin down the fundamental
CP–violation parameters in $K^0$ decay. The search for a neutron electric dipole moment must go on. Most importantly, the world needs one (and only one!) $B$ factory powerful enough to explore the question of CP violation in $B$ decay.

- We must extend the burgeoning non–accelerator frontier and not forget that much of what we know about elementary particles once came, and will come again, from disciplines far removed from accelerator–based high–energy physics:

(a) The existence of an alleged 17 KeV neutrino is flatly inconsistent with the standard theory. Does it exist or does it not? Discussions at this meeting were inconclusive. Many experiments, some of them solid, see evidence for it. Many experiments, some of them solid, claim to exclude it. Here is an exciting dilemma!

(b) Does resolution of the solar–neutrino problem demand the existence of mixed and massive neutrinos? These effects lie beyond the minimal 17–parameter standard theory. However, the case for MSW oscillations as a cure to the problem is far from iron clad. Morrison, for example, is led to conclude that there is no solar neutrino problem. Decisive results from the gallium experiments are awaited with fervor, as are those from future experiments now being planned.

(c) Astronomers have come to the dreadful conclusion that they cannot detect anything beyond the gravitational influences of the dominant form of matter in the universe. The dark matter may be non–baryonic and unconventional. If so, its identification and investigation lies within the realm of high–energy physics.

The near–term future of elementary–particle physics, largely if not exclusively, depends on those large accelerators that are now or soon operating: the fixed target facilities at Fermilab, Brookhaven’s kaon beam, the Tevatron collider, LEP, HERA and the mini $B$ factory at Cornell. I have not the gall to write a guide for the experimenter. Our discipline is open–ended, in the sense that the new and profitable directions are almost certainly not those that we anticipate. Surprises may lie in wait for experimenters at all of these facilities.

For the long term, our community is almost unanimous in its belief that the next great accelerator must be a hadron collider. SSC is approved and partially funded while an LHC proposal is soon to be put to the CERN council. At the risk of alienating many friends and colleagues, let me consider this issue.†

† The following remarks on the issue of one too many supercolliders were not a part of my oral presentation at the conference.
Giant accelerators, their enormous detectors, and their continuing operations are enormously expensive enterprises. We all agree that the world needs one great hadron collider, but two such machines makes no sense at all. The world’s governments, if they knew what we know, should not fund the construction and instrumentation of both machines. There are too many other interesting and important things to do! Although well-intentioned arguments have been presented for each accelerator from each side of the Atlantic, the course that is set seems bound for disaster.

Should we prove ingenious (and irresponsible) enough to convince our governments to begin building the two comparable behemoths, these endeavors are likely to starve ongoing research programs in all of physics, and especially in high-energy physics. Money is more-or-less conserved: CERN needs extra support (from member or non-member) nations to build LHC expeditiously, and SSC may not be built without substantial external support. The majority of our colleagues — those who do physics in real time and choose not to live at the very fringe of the high-energy frontier — will need and deserve funds that are almost certainly to be preempted by our preoccupation with bigness.

If and when the twin supercolliders begin to do real physics research — which may not happen until the next millennium — there may no longer exist enough of a community of experienced and dedicated high-energy physicists to use them. It is not for reasons of national pride that I believe that SSC is the preferred machine if we have the vision, the scientific integrity, and especially, the fiscal responsibility to build only one.

- There are technical obstacles in the path of the construction and exploitation of LHC. It requires novel two-in-one magnets operating at nearly ten tesla. Can reliable and adequate magnets can be industrially produced at reasonable cost? The physics reach of the LHC is extended by exchanging a compromised collision energy for an intense luminosity, but can appropriate detectors be designed and built to make use of such luminosities?
- There are operational difficulties associated with the construction of LHC. LEP has made a glorious start in its ambitious and exciting program. It has both a higher-luminosity and a higher-energy phase to look forward to. It would be tragic for physics if LHC deployment would hamper, delay or constrain these essential projects.
- There is a serious scientific risk associated with the exploitation of LHC. Its collision energy is severely constrained by its need to fit within the LEP tunnel. What if the new physics is accessible at 40 TeV but not at 20 TeV?
My dream is of a ‘new world order’ of cooperation rather than competition for which high-energy physics can lead the way. Let SSC evolve into a truly international project: in funding, instrumentation and administration. CERN is an enormously successful and enviable model. Let SSC fly the flags of all interested and committed nations and be administered by a representative council much as CERN is. Let it be a machine in America but of the world. My dream is certainly naive, unrealistic and all but impossible to realize. Until recently, the DOE has insisted that SSC be an essentially American enterprise. CERN needs a new initiative to ensure its continued vitality. Nonetheless, I cherish the hope — for the sake of our discipline — that my dream may come true.

Much has been said at this conference about grand unification. Strong, weak and electromagnetic forces are each mediated by gauge bosons. The hypothesis that there is a simple underlying gauge group is irresistibly attractive. In minimal $SU(5)$, the curious charges of quarks and leptons are forced upon us and each family corresponds to an anomaly–free representation. Neutrinos are automatically massless and neutral. The observed disparity in strength between strong and electric forces sets the symmetry–breaking scale, makes protons practically stable, and evaluates the weak mixing angle. The trouble is in the details: protons are not stable enough and $\sin^2 \theta$ comes out a bit too small.

There are many ways in which minimal $SU(5)$ may be modified so as to patch up the problems, fit the data, and save the notion of grand unification. The First Fix: A decade ago, several theorists pointed out that supersymmetrized $SU(5)$ could do the trick [1]. The Second Fix: Both empirical problems could be dealt with by adding to the standard theory one or more split $SU(5)$ fermion multiplets [2]. The Third Fix: Perhaps there is an intermediate stage of symmetry breaking involving a semisimple gauge group lying between the unifying group and the group of the standard theory. An interesting example of such a hierarchy is:

$$ O(10) \to SU(3) \times SU(2) \times SU(2) \to SU(3) \times SU(2) \times U(1). $$

The intermediate mass scale can be chosen to fit the low–energy values of $\alpha$, $\alpha_s$ and $\cos \theta$ [3]. For this case, the unification scale is comparable to the Planck energy (neat, but no observable proton decay), and left–right symmetry is restored at roughly a million TeV (useful to generate sensible neutrino masses, but not for new phenomena at LHC or SSC).

Now that LEP data has provided us with an accurate determination of $\alpha_s$, many workers have refocussed on the rescue of grand unification and the intriguing possibility
that lots of new particles lie almost within reach. The results of their analyses are remarkable (and have received lots of attention in the semi-popular press).

Although the failure of minimal $SU(5)$ is now established beyond any possible doubt, its supersymmetrized version is still very much alive. Furthermore, the super-partners of known particles may lie $\sim 1$ TeV and be accessible to the next great hadron collider; and proton decay may be detectable at super-Kamioka. A careful and current analysis of the data is dramatically summarized in figure (1).† However, the second fix (which does not please supersymmetry fans, but involves many fewer hypothetical and perhaps accessible particles) is also in the running, as figure (2) demonstrates. The first and second fixes suggest — the input data are not precise enough as yet to use a stronger verb — the possible existence of new physics at soon-to-be accessible energies. However, the third fix also preserves grand unification with an intermediate mass scale lying well beyond experimental reach. And, there may be other fixes as well. We shall learn more about the various possibilities as data improve, and we cannot emphasize too strongly the importance of those ‘pedestrian’ experiments, at LEP or HERA, that serve to measure the value of the strong coupling constant.

In summary, let me quote a countryman, ‘The game ain’t over ’til it’s over. Particle physics is very much alive, both at and away from the highest energies, both at and away from accelerators. Our colleagues who lean toward astrophysics and cosmology have given us a solar neutrino headache and various costly medicines to cure it, gravitational lenses, an indirect confirmation of gravitational radiation and a hoped-for (but unfunded) search for such waves, supernova neutrinos, a weird large-scale structure to the universe coupled with a perfectly smooth era of recombination, inexplicable burster phenomena, the possibility of unconventional dark matter, and many more surprises and dilemmas to come. Our low-energy colleagues give us monopole constraints, axion searches, an alleged 17 KeV neutrino, an exorcism of the fifth force, a precision test of the electroweak theory, and again, more to come.

At the accelerator frontier, there are many important experiments to do and surprises to uncover. Indeed, there is far more to do than we can easily afford, especially with the burden of building and instrumenting supercolliders in two continents. The Fermilab collider must be upgraded so that it may do its best to find the top, which will be a

† The figures are omitted from this preprint. All three fixes work flawlessly.
primary background to experiments at larger hadron colliders. Fixed–target experiments are needed to measure $\epsilon'$ and to search for rare decays and neutrino oscillations. HERA may find leptoquarks, but it will certainly lead to precision tests of QCD. LEP, in its present and future avatars, may yield wonderfully inexplicable events, but it offers the best tests of the electroweak theory and the best constraints on its variations. A $B$ factory is essential if we are to clarify our understanding of quark mixing and CP violation, while smaller factories which are dedicated to physics at 1 GeV and at 3 GeV have important rôles to play as well. Finally, and in my view most importantly, we must learn how to build a powerful linear electron–positron collider with which to study the TeV domain in a relatively clean environment.

Many of our best theorists are all wrapped up in strings and conformal field theory. They believe that a new mathematical framework is needed that lies beyond tried and trusted quantum field theory. They are almost certainly right in their belief: the most puzzling ‘why questions’ cannot even be posed in the standard theory or any of its conventional modifications. However, their early optimism is gone: most string theorists no longer believe that a theory of everything lurks around the corner. They should not be discouraged. The new theory will come in its time, and today’s theorists may lead us to it kicking and screaming. In the meanwhile, particle physics remains, as it traditionally has been, primarily an exciting and rewarding experimental science.

2. AN OPEN LETTER TO HEPAP

Esteemed colleagues: Some years ago, I became a member of HEPAP. For all I know, I may yet be. I attended the first two meetings during my tenure, and no others. The reasons include time pressure and my own irresponsibility, but the primary cause for my delinquency is a strong impression that HEPAP (unlike the CERN Science Policy Committee, in which I served honorably for six years) does not adequately address the needs of the community. Due to the open nature of its meetings, a constituency that reflects the conflicting self–interests of our several national laboratories, the obligation to follow a firmly orchestrated agenda, the absence of prior discussions and the lack of executive sessions, difficult issues are rarely faced and sad truths are withheld or sloughed over. I am led to write this letter — rather than play–act at HEPAP — because of my love for and concern about the discipline of accelerator–based high–energy experimental physics in the United States.
I apologize for this hurried and distraught communication, but I am alarmed by the precipitous decline of a discipline that Americans created and, until recently, dominated. There is not reason nor sense for continuing American domination of the field, but we should at least remain as major players. We spend a lot of money on HE physics, but seem to get too little physics done per dollar spent. If we are to argue, as I think we should, for more funding — or even for continuing funding at present levels — we must demonstrate more of a sense of fiscal responsibility than we have in the past. At the price of alienating many colleagues and friends, let me say the unsayable:

- The SLD detector at SLAC has been a sink–hole for funds that could have been spent productively: it is a second detector for a machine that will probably never do useful physics. (CERN has analyzed a million, going on ten million, $Z^0$ events. SLC has got a few hundred, and won’t do all that better, polarization or no. Face it chums: it’s a brilliant demonstration of a new accelerator technology, but will never be much of a research tool.) Years ago, when the SLD project was initiated, its proponents may have had sound arguments for it. Very soon, however, it became clear to much of the community that the device was pointless. However, once the great bureaucratic ship sets its sails, no agent on Earth can correct its course. Physicists lucky enough to be on board cling blindly to their challenging tasks no matter that the cruise is to nowhere. For all I know, money is being spent on this useless toy even as I write.

- When the Soudan proton decay detector was first proposed, it seemed a good idea. If the larger IMB and Kamioka could detect proton decay, a smaller but more highly instrumented device would have been an invaluable facility. As it turned out many years ago, there was no such signal. It became clear that the Soudan initiative was pointless, as far as its original primary goal is concerned. No matter. The ship plows on.

- Although PEP was in many ways a better instrument than PETRA, its timing was a disaster. Aside from a few physics gems, it came on line far too late to succeed as a forefront accelerator. As my father taught me, if a job’s worth doing, it’s worth doing right... and when it needs doing!

- Years ago, it seemed a good idea to provide the Fermilab collider with a better and bigger (and very expensive) new detector. But a second detector is useful only when there is the potential to exploit the accelerator at which it is to be used. However, we have hardly begun to exploit the CDF detector. With collider runs as far apart as they are, who needs another detector? It’s just one more uncontrollable spigot or useless ship. All
that money could have been used to shore up the decaying university–based infrastructure upon which all of physics research depends.

Enough spilt milk and rotten eggs! High–energy physics in America is threatened by dilution (too many sacred laboratories), by dispersion (too many directions, with little foresight and no corrective capacity) and by the reluctance of our community and its sponsors to perform difficult but necessary triage. In the future, we must focus more tightly and painfully, or abandon ship.

For sound scientific and technical reasons, we all agree that a proton super–collider is our first priority. However, SSC will not come on line for physics for a decade. If things continue as they are, there aren’t going to be any American experimenters around who are ready, willing and able to use it. All other priorities are irrelevant and immaterial unless we properly attend to numero uno!

As I talk to my experimentalist friends, I find that almost all groups – from the brilliant to the merely sound – are being squeezed out of the business of high–energy physics. Hardly any American graduate students care to board our sinking ships. Yet, if we are to build SSC, we must keep HE physics, especially hadron–collider physics, healthy in the meantime — and there’s lots of good physics remaining to do at the World’s Highest Energy but Rarely Running Accelerator. The training ground for a large hadron collider is a large hadron collider, and we’ve got the only one. We’ve got to ‘find the top’ because it’s likely to be the background to really interesting SSC events. Can anyone doubt that part and parcel of the SSC initiative must be the maximum exploitation of the Fermilab collider. This means that we must have more collider runs sooner and a Main Injector too!

One last word about the SSC, whose balance sheet I’ve never quite understood. It seems that we are depending on massive foreign support for the construction of both the machine and its enormous detectors. We don’t have commitments for such support, and we may never do if Europe commits to the LHC and Japan joins their action. Furthermore, it is international lunacy to build and instrument both behemoths. Arguments based on physics goals and technical obstacles convince me that SSC is the better machine to build: LHC is just too small, too hard to build and too hard to instrument. Is the federal government finally reaching the sensible conclusion that SSC must be truly internationalized, not only in funding but in management, so that it becomes a facility in America but of and for the World? The search for the secrets of matter and the universe is both glorious and costly. Surely, we must cooperate rather than compete — it’s the way to go, and perhaps the only way.
Acknowledgements

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