The Lesson of Quantum Theory, edited by J. de Boer, E. Dal and O. Ulfbeck

Does Elementary Particle Physics Have a Future?

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How pleased I am—having spent 27 months of the past 27 years in Denmark—to return after a lamentable lapse of 21 years! And how honored to speak at this celebration of the genius of Niels Bohr! What a marvelous century it has been, and how wonderful Copenhagen truly is!

The tantalizing discovery of the Balmer series took place the year before Bohr's birth, 101 years ago. Last year, the Nobel Prize was awarded to an Italian and a Dutchman for their discovery of the intermediate bosons W and Z—the penultimate predictions of today's theory. It was a fitting climax to the century of quantum mechanics, the century we now celebrate as Niels Bohr's.

Physicists deal with an incredible range of distances, from the inconceivably small Planck scale of 10^{-33} cm to the incomprehensible size of the visible universe, 61 powers of ten larger. Arranged sequentially upon this cosmic ruler are the many disciplines of science: particle, nuclear, and atomic physics, then chemistry, biology and geology, and finally astronomy and cosmology. All these fields are ultimately quantum mechanical, and quantum mechanics began with Bohr.

Typically, Bohr began not at the beginning of the ruler nor at the end, but at the muddle in the middle, the atom. He saw that the classical rules could not describe Rutherford's nuclear atom so he invented new ones which did. His rules evolved into a theory which explained the mysteries of the atom and the successes of the periodic theory. But, quantum mechanics is a greedy master which admits of no competition. Quantum rules must rule the whole ruler.

The quantum atom led to the quantum nucleus, which is built up of nucleons held quantum-mechanically together. Nucleons themselves are built up of quarks, whose quantum nature is demonstrated by the discovery of dozens of "stationary states" of the proton. The smaller things are, the more they are quantum mechanical. For this reason, it took physicists decades to recognize these states for what they are.

For objects the size of a mountain and larger, gravity is the dominant force and the relevance of quantum mechanics seems to fade. But, stars shine because of a

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complex interplay of all of the forces of nature in an essentially quantum-mechanical game. Quantum mechanically as well, the hot early universe of ten billion years ago cooked up many of the light nuclei now about us. Things work out so well that we know that the laws of quantum mechanics were the same then as they are now. Truly, *plus ça change*, *plus c'est la même chose*.

In the earliest moments of the formation of our universe, things were so very hot that particle physics and its bestiary of quarks and leptons reigned supreme throughout the universe. The rule has curled up upon itself—it is no ruler at all but a snake swallowing its own tail, Ouroboros. The physics of the microworld is the physics of the entire cosmos. The large and the small are one. Our earthly accelerators are at once microscopes of supernal resolution, and miniature replicas of the greatest accelerator of them all, the entire universe.

The unification of the small and the large is twofold. Gravity itself must be a quantum-mechanical theory: Bohr and Einstein must ultimately be reconciled. Quantum gravity is dominant only at times so early that the temperature of the universe approaches the Planck mass, or conversely, at the very smallest distance scale of the Planck–Compton wavelength. Once again the snake swallows its tail, but at energies and at distances far removed from any conceivable laboratory but the universe itself.

Bohr's modest domain of atomic sizes has been expanded and expanded by the arrogant reductionism of today's physical scientist to cover the whole shebang. Mysteries still confront us at the largest distance scales: What is the dark matter of the universe? Why do globular clusters seem to be older than the universe itself? How did the galaxies form? And so on. But, we have made remarkable progress at the smaller scales. We have what appears to be a correct, complete and consistent theory which describes all the known phenomena of the microworld: quantum chromodynamics and the electroweak theory.

On vacation upon the lovely isle of Jamaica I discovered that Jamaicans know but one variety of cheese. It is used on pizzas, in sandwiches, and in omelets. It is called standard cheese. So it is in particle physics. We have only one theory that works, and a very good theory it is. Quantum chromodynamics and the electroweak theory comprise our standard model of particle physics. It is used in cosmology, astrophysics, nuclear physics, and in particle physics. There is really no choice.

No known phenomena suggest structure beyond the standard model. No measured quantity contradicts the standard model. There are no internal contradictions, and there are no loose ends. Yet, the standard model appears to no one to be a satisfying conclusion to the search of the particle physicist. For one thing, quantum chromodynamics (QCD) is not yet as predictive as one might hope. It has not yielded the observed mass spectrum of the hadrons. Presumably, this is a computational question which will be resolved by further study and future development of computer systems. There are more fundamental puzzles outstanding.

Why is the gauge group what it is? Why are there three families of fundamental fermions? Is there a Higgs boson, or what? Aren't seventeen basic particles and seventeen arbitrarily tunable parameters far too many? How about a quantum theory of gravity?

Have you noticed that we have made the great leap sidewise? Once upon a time, we particle physicists could honestly claim to be studying the ultimate structure of ordinary matter, from atoms, to their nuclei, to the garden varieties of quarks. Things began to change about 40 years ago with the discovery of the muon. I. Rabi, like other New York physicists fond of Chinese food, is said to have said upon hearing about the muon, "Who ordered that?" It is among the very few questions that Rabi's student, Julian Schwinger, hasn't answered. Schwinger's own student, yours truly, doesn't yet know.

Today the muon has been joined by the tau lepton, strangeness, charm, top and bottom. We no longer study the structure of the atom, for two thirds of our particles have nothing whatever to do with mundane matter. They are exotica found only at large accelerators or in the debris of cosmic-ray collisions. They have no more to do with "ordinary physics" than do elements numbers 108 and 109 have to do with "ordinary chemistry". We seem to be following an endlessly difficult and expensive side issue. Have we lost the thread of relevance?

We have not at all lost our way. It is just that we are not yet there. The standard theory cannot be our final answer just because it cannot justify the great leap sidewise. Like my Harvard predecessor, Percy Bridgeman, I (and, surely, we) have a quite unjustifiable faith in ultimate simplicity, in the existence of a one and true theory: unjustifiable but always justified by the remarkable progress in our discipline. Once again, think of the century and the man we now honor. Today's side issue will one day be central. The muon will find its essential place in the sun, along with its curious brethren.

Yes, my children, elementary-particle physics does have a future. Yet, today it is threatened, and its exposure may be greater than ever before.

The greatest danger is the possibility of rejection by our parent society: because of its seeming irrelevance, and because of its considerable cost. The American high-energy physics community is uncharacteristically unanimous in its desire and perceived need for a large hadron collider. Budgets are tight, and the multi-billion-dollar SSC has not yet been funded. Will it ever be? The answer is not obvious. That such a machine is essential to the American technological renaissance is hardly an incontrovertible argument. Perhaps, as R.R. Wilson has argued, large accelerators are today's analog to the great cathedrals. This too is not a convincing argument, for we are not any more prone to monument building. We might try to argue that particle physics builds sound minds in strong bodies. Those lured into intellectual combat by the challenge of particle physics often make their mark in other and more useful fields, like Wally Gilbert's discovery of the repressor, or Allen Cormack's contribution to the CAT scanner, or Luis Alvarez, or George Charpak, or Max Delbrück, and so on. Ultimately our arguments should be more self-contained. We must, and I hope we will, support particle physics for its own sake. We must convince our governments of the importance of pursuing pure science, of our obligation to understand the world we are born into. The American people understand this kind of argument far better than do its elected representatives.

The high cost of particle physics is not an exclusively American problem. Not all the European countries enthusiastically support fundamental physics. Denmark, the land of Niels Bohr, is certainly not one of the big spenders, not even on a per capita basis. And, England has produced the Kendrew Commission report which seriously puts forth the suggestion of a fatal 25% cut in the entire CERN budget. Fortunately, it is unlikely that things will come to such a sorry pass, since the entire British contribution to CERN is a mere 16%, and not all of Europe is so disenchanted with big science. France has discovered, after all, that it *does* have need for its savants.

A second threat to our discipline is the recent divorce between particle experiment and particle theory. Perhaps it all began with quantum chromodynamics, an apparently correct theory underlying the quark structure of nucleons and the nuclear force itself. It is not merely *a* theory, but within a certain reasonable context, it is *the* unique theory. In principle, QCD offers a complete description and explanation of nuclear physics and of particle physics at accessible energies. While most questions are computationally impossible to answer fully, the theory has had very many qualitative (and, a few quantitative) confirmations. It is almost certainly "correct". QCD is not the threat I have in mind. It has *not* produced a divorce between experiment and theory—indeed, it has led to closer coordination and cooperation between experimenters and theorists. Yet, it has planted a seed that has blossomed elsewhere. It suggests and affirms the belief that elegance and uniqueness can be criteria for truth. I believe in these criteria. But, they must be reinforced by experiment. I agree with Lord Kelvin when he says,

> "When you can measure what you are speaking about and express it in numbers, you know something about it. And when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind. It may be the beginning of knowledge, but you have scarcely in your thought advanced to the stage of a science."

By this criterion, too, QCD is a science. But, can the same be said of the superstring and its ilk?

Quantum mechanics is contagious, and gravity must be framed within its context. Some of my theorist friends feel that they have come upon the unique quantum theory of gravity: a supersymmetric system of strings formulated within a ten-dimensional space–time. Most of the physics of the superstring lies at forever inaccessible energies, up around the Planck mass. Within its context, the theory is unique. It may even be finite and self-consistent. It seems capable of describing the low-energy phenomena which we can observe in the laboratory, but it is hard to prove that it really does. In principle, it predicts what particles exist. In principle, the number of tunable parameters is reduced to zero. In practice, however, it has made no verifiable prediction at all, and it may not do so for decades to come. The string theorist has turned towards an inner harmony. But, can it be argued that elegance, uniqueness, and beauty define truth? Has mathematics supplanted and transcended experiment which has become irrelevant? Will the mundane problems which I call physics, but which they call phenomenology, simply come out in the wash in some distant tomorrow? Is further experimental endeavor not only difficult and expensive, but *unnecessary* and *irrelevant*? Perhaps I have overstated the case made by string theorists in defense of their new version of medieval theology where angels are replaced by Calabi–Yau manifolds. The threat, however, is clear. For the first time ever, it is possible to see how our noble search could come to an end, and how Faith could replace Science once more. Personally, I am optimistic. String

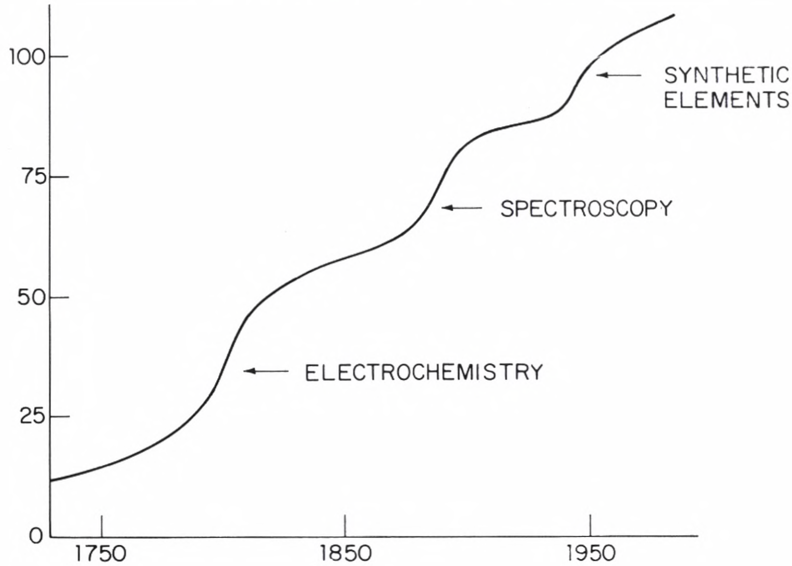

Fig. 1. The first population explosion. The growth of the number of known chemical elements over the past three centuries. The curve is roughly linear, with superimposed spurts of discovery resulting from technological developments. Elements 108 and 109 were produced in the 1980s. Will there be more?

theory may well dominate the next fifty years of fundamental theory, but only in the sense that Kaluza–Klein theory has dominated the past fifty years. Perhaps we should turn our attention to the past for guidance about the future.

The search for the ultimate constituents of matter has had a cyclic history passing from chaos, to order, to a new level of structure, and to chaos once more. We have passed through four such cycles in recent history: atoms, their nuclei, hadrons, and now quarks and leptons.

Recall the search for new chemical elements, which precedes Lavoisier and continues to this day. Figure 1 shows the growth of the number of known species with time. The most recent additions, Nos. 108 and 109, were produced and observed in Germany in the 1980s. The curve increases almost linearly over two-and-a-half centuries, showing spurts of discovery due to the new technologies of electrochemistry, spectroscopy, and more recently, artificial synthesis.

Midway in the history of the discovery of chemical elements, the Periodic Table was devised. The systematic order thus revealed was fully explained in terms of electrons, nuclei, and the quantum rules of Bohr. Nuclei were soon identified as composite structures themselves. The discovery of isotopes showed that there are far more nuclear species than there are chemical elements. Indeed, fig. 2 shows a plot of the 399 nuclear species whose half-life exceeds one year. (My colleague, Roy Glauber, had guessed 400.) Clearly, there are far too many nuclides for them to be elementary. The integral values of Z discovered by Moseley, and the almost integral values of A, represent the second level of order. Structure awaited the discovery of the neutron in the year of my birth.

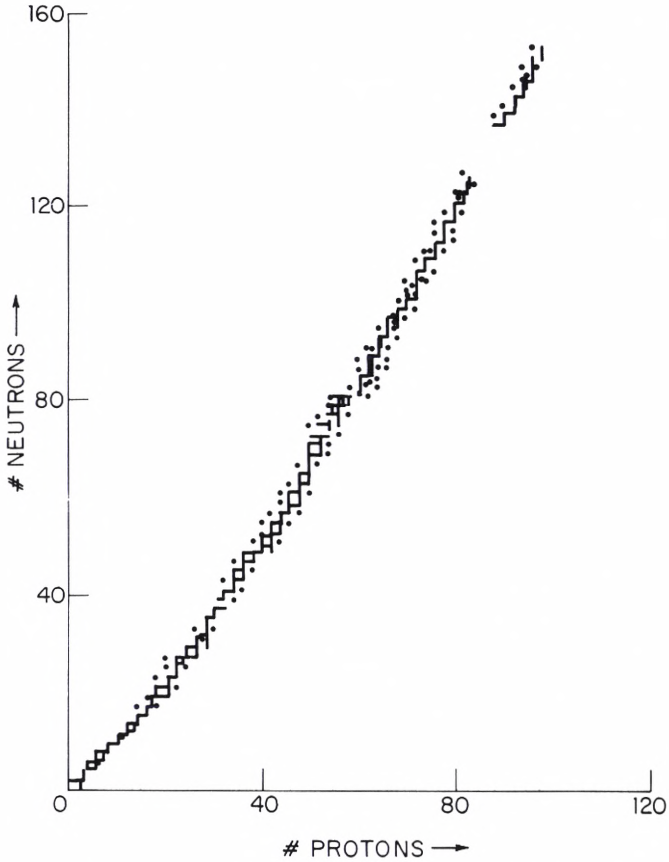

Fig. 2. The second population explosion. There are precisely 399 nuclear species whose half-lives exceed one year. Most of them lie on a connected "peninsula", but some, including uranium, form an isolated "island of stability". Is there a second such island awaiting discovery?

Things were beginning to look simple. In the late 1940s, Gamow wrote that the elementary particles were only three in number: nucleons, electrons, and neutrinos. These were the pointlike particles that were the basic building blocks of all matter. Alas, it was not to be so simple. Pions and strange particles were discovered. As large accelerators were deployed, there was a virtual population explosion among the nuclear particles, or hadrons. Figure 3 shows the time-evolution of the number of known hadron types. The curve rises approximately linearly from a few to more than a hundred in a time interval of only several decades. The curve is reminiscent of the explosion of known atomic species, but compressed in time by a factor of ten. As before, wiggles in the curve correspond to developments in experimental technology: bubble chambers and electron–positron colliders.

Once again, midway in the evolution of the curve, a new level of order is discovered: the eightfold way of Gell-Mann and Ne′eman. New particles with specific properties were predicted by the theory. The discovery of the famous Ω^-

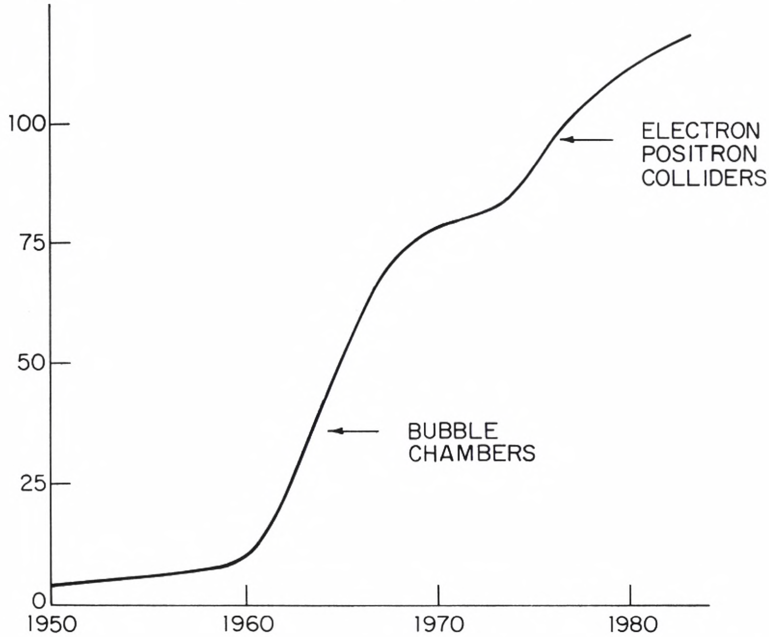

Fig. 3. The third population explosion. The growth of the number of known hadron multiplets over the past three decades. Again, the curve is roughly linear with spurts due to the deployment of new instruments. There are now about as many hadrons as there are chemical elements. Soon there will be more.

particle played the same role in this third cycle that the finding of scandium, germanium and gallium did for the first. Gell-Mann was no more mad than Mendeleev. For a third time, the appearance of systematic order led to the revelation of a new layer of structure. The hadrons were found to have all the attributes of composite systems: they were shown to be made up of quarks.

Today's candidate elementary particles have endured a quieter population explosion than their predecessors. The time evolution of the number of known quark species, leptons, and force particles is shown in fig. 4. There are, in our standard theory, just 17 building blocks, and 14 have been unambiguously discovered in the laboratory. Indirect evidence for the existence of the tau neutrino is compelling. Muted reports of the discovery of the top quark appear regularly in *Physics Today* and other journals of record. By far, the most important of the missing but predicted particles is the Higgs boson. If it is sufficiently light, it will be discovered at LEP. If its mass lies in an intermediate mass window, it will show up at the SSC or the LHC. The Higgs boson is the last great confirmation of the standard model that awaits discovery.

While the most recent population explosion, or descent into chaos, has been gentle, it is of a profoundly new and disturbing aspect. In earlier cycles, we were studying the nature of matter, quite ordinary matter such as is found on earth. Of our fundamental fermions, this is true for just two varieties of quarks, electrons, and perhaps the electron neutrino. None of the other quarks and leptons have a relevant

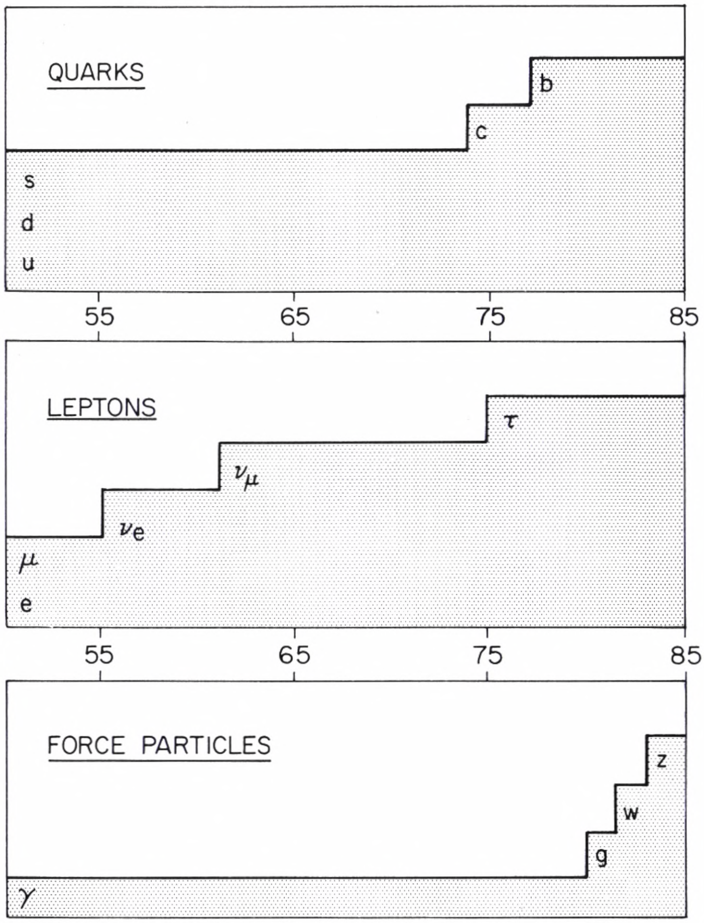

Fig. 4. Today's population explosion of fundamental particles. According to our canonical theory, only one particle of each type remains to be found. The top quark and the tau neutrino are relatively easy. The search for the Higgs boson is the outstanding challenge to defenders of the standard model. Will it be found? Will something unexpected be found?

role to play in the standard model. This great leap sidewise indicates that great progress remains to be made. The one-and-true theory that we seek is perhaps not the best, but it is surely the *only* possible world, and in it each and every particle will have an essential (an discernible!) *raison d'etre*.

The next level of order is surely revealed by today's periodic table of quarks and leptons, fig. 5. The fundamental fermions form families, each with the same recurrent pattern of strong and electroweak properties. The pattern is explained in terms of grand unification, but the reason for the seemingly superfluous replication of families remains obscure.

From the time of Bohr to the present time particle physics has progressed enormously. Many, indeed most, of the problems of yesteryear have been solved. To a large extent, our success is the result of experimental discovery. Generally, the

-1	-1/3	0	2/3
e	d	ν_e	u
μ 1938	s 1947	ν_μ 1961	c 1974
τ 1975	b 1976	ν_τ	t

MATTER PARTICLES

γ	g 1980	W 1983	Z 1984	HIGGS

FORCE PARTICLES

Fig. 5. The periodic table of quarks and leptons and the supplemental list of force particles. Two decades ago, the table suggested the possible existence of the charmed quark. Today, it hints at a deeper level of fundamental structure. Each row or family is a minimal anomaly-free unit. Why are there three families, or, are there more than three? Mendeleev's earlier table left no room for inert gases. Have we made such an omission again?

essential empirical basis to our theory is the result of patient, even plodding, endeavor. It is the accumulated knowledge due to very many scientists, all too few of whom will be remembered for a particularly startling or significant discovery. It has been a history, though, which is punctuated by dramatic and surprising events. Things like the unanticipated discoveries of X-rays or of radioactivity have taken place with remarkable frequency.

For example, in the 1930s, the neutron was discovered. So were the deuteron the positron, and the muon. And, let us not forget nuclear fission.

The 1940s saw the taming of the nuclear force, if such may be said of the use of the atomic bomb. The Lamb shift was measured, and it led to the surprising development of a consistent relativistic quantum theory. It was the decade of pions, and of the strange particle.

Parity-violation surprised almost everybody in the 1950s. So did the ever stranger properties of strange particles: associated production and neutral kaon behavior. Neutrinos were actually "seen" in the laboratory, and the first excited state of the nucleon was detected. An accelerator powerful enough to produce antimatter was commissioned. The discovery of the antiproton was a great accomplishment but hardly a surprise.

Schwinger was not surprised by the discovery of a second neutrino species in the 1960s, but most of us were. The population explosion of new hadron species grew out of hand. As if by magic, these new particles filled out complete supermultiplets of the SU(3) symmetry scheme. Deep inelastic electron scattering produced convincing evidence for the existence of pointlike "partons" in the proton, which, like the atom, turned out not to be a plum pudding after all. And who predicted that time-reversal symmetry would be violated?

So it went in the 1970s. A kooky theory purporting to unify weak interactions and electromagnetism was shown to be renormalizable, and wary experimenters were amazed to find that its neutral currents were for real. The discovery of the J/ψ particle was a big surprise, and even the existence of charmed particles surprised some of us. As if that were not enough, there were the tau lepton, the upsilon particle and its associated beauty particles.

The present decade is only half over. It has been a remarkably quiet decade so far. It was a surprise that CERN was able to mount a search for intermediate vector bosons in a timely and effective fashion. Any demonstration of international cooperation is surprising. However, the existence of W and Z bosons (like the existence of the antiproton) cannot really be thought of as something unexpected.

The lack of a fundamental but unanticipated discovery in this decade has not been for want of trying. Quite a number of surprises were *reported* during the 1980s. The trouble is that none of them seem really to be there. Perhaps a list of such non-discoveries will suffice:

(1) The magnetic monopole,
(2) Neutrino oscillations,
(3, 4) Neutrino masses (twice: Russian and Canadian),
(5) The zeta particle,
(6) No-neutrino double beta decay,
(7) Muons from Cygnus X-3 (still alive?),
(8) Proton decay,
(9) Forbidden decays like $\mu \to e\gamma$,
(10) Inexplicable "wrong sign dileptons",
(11) Free quarks,
(12) Anomalons,
(13) About a half-dozen varieties of anomalous events seen at the CERN collider and purporting to show that there is new physics beyond the standard model. None of these effects is established. Those who have done so much to confirm the standard model have not, as yet, succeeded in demolishing it. Don't they wish!

What is the meaning of this almost incredible list of failed but noble efforts? Has the era of great surprises in particle physics come to an end? Have we exhausted nature's bag of tricks? Do we already have enough clues in hand to build a theory of everything? Or, have we set into effect a self-sustaining prophecy wherein no new discoveries will lead to no new machines, and a guarantee that there can be no discovery tomorrow? These are dangerous times for particle physics.

There are two approaches to our current dilemma, the possession of a theory which is on the one hand too successful, but on the other, clearly incomplete. There is the pedestrian and the grandiose: the upwards path from mere experiment to theory, and the downwards path of pure positive thinking: the way of Bohr, and the way of Einstein. I think that there *is* a lesson to be learned from the past. Bohr's route has proven itself to be successful beyond any reasonable expectation. Einstein's path—the search for a complete and unified theory *now*—has proven to be a dismal failure.

Some day, if our species lives so long, Einstein's dream may be fulfilled. Of course there *is* a connection between gravitation and the other forces of nature. Michael Faraday, like Einstein, and like all of us, believed in the existence of such a relation. Unlike Einstein, he was a follower of the upwards path. Towards the end of his life, on July 19, 1850, after an unsuccessful search for an experimentally verifiable connection between the forces, he wrote:

> "Here end my trials for the present. The results are negative. They do not shake my strong feeling of the existence of a relation between gravity and electricity, though they give no proof that such a relation exists."

The Theory of Everything will come in its time if we let it. I am convinced that we still have a lot to learn about the phenomena of nature. One reason that Einstein failed in his quest is that he simply didn't know enough physics. Particle physics has not necessarily come to the end of the road. Astonishing experimental discoveries certainly remain to be made. If, and only if, we look, shall we find. The question is one of perseverance. Shall the scientific traditions established in the Renaissance survive in today's bizarrely materialistic society? The Way is clear, but what of the Will?

Discussion, session chairman H. Bethe

Bjørnholm: Could you comment on the question of the two additional families of quarks? Are they an indication of a substructure of the quarks?

Glashow: Let's remember that the Periodic System was discovered simultaneously by Mendeleev and Meyer and that they came to opposite conclusions. To Mendeleev the superfluous repetition of the Periodic System suggested that the atom had a substructure, while to Meyer it indicated the elementarity of the atom.
We have seen a very similar superfluous repetition in the case of the three identical families of quarks and leptons. To some people this suggests another layer of structure, and yet at the highest energies studied, there is absolutely no evidence of any structure. For molecules, atoms, nuclei and even the proton and neutron there is a vast number of energy levels that can be studied, and of course the appearance of energy levels is the key indication of the existence of an underlying structure. There is absolutely no indication of excited states of electrons, muons and quarks. These are not excited states which decay into each other, but are simply systems with different quantum numbers.

Casimir: When you say that most questions of yesteryear have been solved, it seems to me that you have still many adjustable parameters which you cannot explain.

That signals an incomplete theory. Why the fine-structure constant is $\frac{1}{137}$ is an unsolved problem. I am surprised how well the theory works despite the fact that it is incomplete. Just think of how many of the predicted particles, both in the 1960s and 1970s have turned out actually to exist.

Glashow: I fully agree with you. The existence of so many adjustable parameters, of which the fine-structure constant is just one, is terrible. It is one of the primary goals of the ultimate theory to resolve this ancient problem. Also I agree with you that the succesful predictions of new hadrons in the 1960s, the charmed quark and the W and Z can be viewed as surprising.

Rabi: Often new discoveries are made by closer examination or an increase in accuracy of measurements of already known results. This is not possible any more. Often I listen to talks in experimental high-energy physics and I ask the speaker, "have you published this?". He answers, "of course I have", but really he has not. Only conclusions are published, not experimental data that can be subjected to criticism. Moreover the experiments are too expensive to be repeated. The experimental physicists are reduced to technicians testing some theoretical predictions. I am afraid that what we see today sometimes may be artifacts of the theory.

Glashow: Professor Rabi, you are absolutely right. The only solution is that you stop attending all these symposia and get back to your laboratory.