

Nature's Greatest Puzzles

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1. QUESTIONS GREAT AND SMALL

It is a pleasure to be part of the SLAC Summer Institute again, not simply because it is one of the great traditions in our field, but because this is a moment of great promise for particle physics. I look forward to exploring many opportunities with you over the course of our two weeks together. My first task in talking about Nature's Greatest Puzzles, the title of this year's Summer Institute, is to deconstruct the premise a little bit.

1.1. The Nature of Scientific Questions

"About 500 years ago man's curiosity took a special turn toward detailed experimentation with matter," wrote Viki Weisskopf [1]. "It was the beginning of science as we know it today. Instead of reaching directly at the whole truth, at an explanation for the entire universe, its creation and present form, science tried to acquire partial truths in small measure, about some definable and reasonably separable groups of phenomena.

"Science developed only when men began to restrain themselves not to ask general questions, such as: What is matter made of? How was the Universe created? What is the essence of life? They asked limited questions, such as: How does an object fall? How does water flow in a tube? etc. Instead of asking general questions and receiving limited answers, they asked limited questions and found general answers."

An important part of what we might do in these two weeks together is to think about how we actually construct science, how we construct understanding, and how we present the acts of doing science to other people. Galileo, the icon of the moment when we humans found the courage to reject authority and learned to interrogate nature by doing experiments, expressed his approach in this way [2]:

Io stimo più il trovar un vero, benchè di cosa leggiera, ch'l disputar lungamente delle massime questioni senza conseguir verità nissuna.¹

We have built up science over these past five hundred years not so much by focusing on the majestic questions as by thinking about small questions that we have a chance to answer, and then trying to weave the answers to those questions together into an understanding that will give us insight into the largest questions.

By focusing on "small things," with an eye to their larger implications, Galileo achieved far more than the philosophers and theologians who surrounded him in Florence and Venice, and who, by their authority, asserted answers to the "greatest questions." A great shame of the race of physics professors is that going through Galileo's motions, *without* an eye to their larger implications, too often constitutes freshman physics lab. We owe it to our students to explain *why* we require them to reenact Galileo's investigations, how we seek to weave the answers to small questions into broader understanding, and what science really is. There is a glorious story here, and we need to convey that glorious story to our students and to the public at large. We owe no less to the future of our science!

I don't underestimate the value of grand themes as organizing principles and motivational devices, but I want to emphasize the need to balance the grandeur and sweep of the Great Questions with our prospects for answering them. At every moment, we

¹I attach more value to finding a fact, even about the slightest thing, than to lengthy disputations about the Greatest Questions that fail to lead to any truth whatever.

must decide which questions to address. Unimagined progress may flow from small questions. Measuring how the conductivity of the atmosphere varies with altitude, Victor Hess discovered the cosmic radiation [3]—one of the wellsprings of particle physics and the subject of Great Puzzle No. 9 at this XXXII SLAC Summer Institute. Hess did not set out to found particle physics, nor even to explore the Great Beyond, but merely to pursue a puzzling observation. So it's entirely possible that by paying close attention to a *well-chosen small thing*, we may be able to change the world.

I am insisting—with Weisskopf and Galileo and many others—on the importance of small questions because their role in the making of science is so poorly understood. Introducing *Time Magazine's* top eighteen (not just ten!) list of America's Best in Science and Medicine, Michael Lemonick wrote in 2001 [4],

“The questions scientists are tackling now are a lot narrower than those that were being asked 100 years ago. . . . As John Horgan pointed out in his controversial 1997 best seller, *The End of Science*, we've already made most of the fundamental discoveries: that the blueprint for most living things is carried in a molecule called DNA; that the universe began with a Big Bang; that atoms are made of protons, electrons and neutrons; that evolution proceeds by natural selection.”

Horgan's assertion that most of the great questions have already been answered is a relatively puerile form of millennial madness. Perhaps this misperception lingers because when we scientists talk about our work we don't always situate our immediate goals within a larger picture that would give an image of what we're trying to learn, what we're trying to understand. But the notion that science's best days are behind us will pass, if it hasn't already.² I'm more troubled by the breezy claim (“more and more about less and less”) that we scientists today address narrower questions than our ancestors did a century ago. This is preposterously false; it has nothing to do with the way science is actually done. Ever since Galileo, what we call science has advanced precisely by asking, and answering, limited questions, seeking small facts, and synthesizing an ever-more-comprehensive understanding of nature. It is vexing to hear this misconception from a distinguished science writer. It is even more vexing because the writer's father was a legendary Princeton physics professor—and a particle physicist. We are failing to communicate that science is, in its essence, weaving together the answers to small questions, and we must do better!

Now let us turn for a moment to the list of “Greatest Puzzles” that will command our attention for these two weeks:

1. Where and what is dark matter?
2. How massive are neutrinos?
3. What are the implications of neutrino mass?
4. What are the origins of mass?
5. Why is there a spectrum of fermion masses?
 6. Why is gravity so weak?
 7. Is Nature supersymmetric?
8. Why is the Universe made of matter and not antimatter?
9. Where do ultrahigh-energy cosmic rays come from?
10. Did the Universe inflate at birth?

To their credit, the organizers have given you ten “Greatest Puzzles” that are not all Great Questions. Some of them are small questions that might grow, in the spirit of Hess's studies of the atmosphere, into great answers. I think it's important to recognize that “top-ten” lists³ are always subjective in some way: they suit a certain moment, a certain purpose, a certain institution, a certain prejudice.

It's also true that the list of “Greatest Puzzles” changes with time. To me, one of the most inspiring things about the progress of science is the way in which questions that were, not so long ago, “metaphysical”—that couldn't be addressed as scientific questions—have become scientific questions. I give you two that in former times were used exclusively to torture graduate students on their qualifying exams:

²Now that Mr. Lemonick has written a biography of the Wilkinson Microwave Anisotropy Probe [5], I trust that he has found at least one counterexample!

³The essential psychosocial capital that lists generate has been examined by Louis Menand [6].

What would happen if the mass of the proton or the mass of the electron changed a little bit?

What would happen if the fine structure constant changed a little bit?

When I was on the receiving end of those questions, I had little patience for them. To tell the truth, I really hated them, because the world wasn't that way, so why think about it? Now that I've lost some of the certainty of youth,⁴ I've come to understand that these were much better questions than my teachers realized.

Let's recast them slightly, as

Why is the proton mass $1836 \times$ the electron mass?

What accounts for the different strengths of the strong, weak, and electromagnetic interactions?

Not so long ago, these were metaphysical questions beyond the reach of science: Masses and coupling strengths were givens. But now we can see how the values of masses and coupling strengths might arise; we recognize these questions as scientific questions. As we'll recall in a few paragraphs, we understand where the proton mass comes from. We have a framework for inquiring into the origin of the electron mass. We know, through renormalization group analysis, that coupling constants evolve with energy; we can make a picture in which the coupling constants have the low-energy values we measure because they evolve from a common value at a high energy—the unification scale. We can imagine how, if the world were a little different, the couplings would have changed. So these turn out to be not such annoying questions—not mere instruments of torture—but questions that we can answer scientifically. Soon, we will be able at least to sketch plausible storylines, if not to tell the full stories. Similar progressions from apparently arbitrary givens to answerable scientific questions appear all over the map of science.

Some questions remain unanswered for so long that we might be tempted to forget that they are questions. One that has been much on my mind of late is, “Why are charged-current weak interactions left-handed?” Nearly everyone in this room was born—or at least born as a physicist—after the 1957 discovery of parity violation in the weak interactions. It's fair to say that, whereas our ancestors were shaken by the asymmetry between left-handed and right-handed particles, we have grown up with it. [I estimate that I have written down more than ten thousand left-handed doublets to this point in my career.] So it would not be astonishing if the question had lost its edge for us. But I hope you will agree that the distinction between left-handed and right-handed particles is one of the most puzzling aspects of the natural world. It suggests the following

Exercise. What other profound questions have been with us for so long that they are less prominent in “top-ten” lists than they deserve to be?

If new questions come within our reach and long-standing questions slip from our consciousness, some formerly Great Questions now seem to us the wrong questions. A famous example, developed in detail by Lincoln Wolfenstein last year [8], is Kepler's quest to understand why the Sun should have exactly six planetary companions in the observed orbits. Kepler sought a symmetry principle that would give order to the universe following the Platonic-Pythagorean tradition. Perhaps, he thought, the six orbits were determined by the five regular solids of geometry, or perhaps by musical harmonies. We now know that the Sun holds in its thrall more than six planets, not to mention the asteroids, periodic comets, and planetini, nor all the moons around Kepler's planets. But that is not why Kepler's problem seems ill-conceived to us; we just do not believe that it should have a simple answer. Neither symmetry principles nor stability criteria make it inevitable that those six planets should orbit our Sun precisely as they do. I think this example holds two lessons for us: First, it is very hard to know in advance which aspects of the physical world will have simple, beautiful, informative explanations, and which we shall have to accept as “complicated.” Second, and here Kepler is a particularly inspiring example, we may learn very great lessons indeed while pursuing challenging questions that—in the end—do not have illuminating answers.

Sometimes we answer a Great Question before we recognize it as a scientific question. A recent example is, “What sets the mass of the proton?” and its corollary, “What accounts for the visible mass of the universe?” Hard on the heels of the discovery of asymptotic freedom,⁵ Quantum Chromodynamics provided the insight: the mass of the proton is given mostly by the kinetic

⁴The scaling laws I derived with Jon Rosner [7] may be seen as an act of penance for my youthful intolerance.

⁵See the 2004 Nobel Lectures by David Gross [9], David Politzer [10], and Frank Wilczek [11].

energy of three extremely light quarks and the energy stored up in the gluon field that confines them in a small space. Almost before most people realized that QCD had made the question answerable, we had in our hands the conceptual answer and an essentially complete *a priori* calculation [12].

1.2. “The Theory of Everything”

I do not have a lot of patience for debates about the problem of knowledge; for the most part, I would rather do science than talk about how to do it. Nevertheless, at this time when we anticipate a great flowering of our subject, we should examine our habits and think a little bit about how other people do science and how they see us.

Two interesting characters, Bob Laughlin and David Pines [13], have published a broadside proclaiming the end of reductionism (“the science of the past”), which they identify with particle physics, and the triumph of emergent behavior, the study of complex adaptive systems (“the physics of the next century”). The idea of emergent behavior, which they advertise as being rich in its applications to condensed matter physics in particular, is that there are phenomena in nature, or regularities, or even very precise laws, that you cannot recognize by starting with the Lagrangian of the Universe. These include situations that arise in many-body problems, but also situations in which a simple perturbation-theory analysis is not sufficient to see what will happen.

My first response to Laughlin & Pines is that they have profoundly misconstrued the way we work. What is quark confinement in QCD, the theory of the strong interactions, if not emergent behavior? You could do perturbation theory for a very long time and not discover the phenomenon of confinement. This notion of emergence is ubiquitous in particle physics. As QCD becomes strongly coupled, new phenomena emerge—not only confinement, but also chiral symmetry breaking and the appearance of Goldstone bosons—that we wouldn’t have anticipated by staring at the Lagrangian. [This is, by the way, one of the reasons that we should force ourselves to pay attention to heavy-ion collisions at high energies; the very lack of simplicity may push us into realms of QCD where we can’t guess the answers by simple analysis.] The “Little Higgs” approach to electroweak symmetry breaking [14] is another example of important features that are not apparent in the Lagrangian in any simple sense. A graceful description of the consequences of these phenomena entails new degrees of freedom and a new effective theory.

Laughlin and Pines advocate the search for “higher organizing principles” (perhaps universal), relatively independent of the fundamental theory. I give them credit for emphasizing that many different underlying theories may lead to identical observational consequences. But they turn a blind eye to the idea that in many important physical settings, the detailed structure and parameters of the Lagrangian are decisive. They campaign as well for the synthesis of principles through experiment, which I also recognize as part of the way we do particle physics. I believe that the best practice of particle physics—of physics in general—embraces both reductionist and emergentist approaches, in the appropriate settings.

Overall, I am left with the impression that Laughlin & Pines are giving a war to which no one should come, because the case for their revolutionary intellectual movement is founded on misperception and false choices.⁶ Perhaps the best way for us to be heard is to listen more closely, try to understand the approaches we have in common, and—occasionally—to use their language to describe what we do. It is important for us to seek the respect and understanding of our colleagues who do other physics, in other ways.

One question of scientific style remains: when we understand a phenomenon as emergent, will that stand as a final verdict, or does emergence represent a stage in our understanding that will be supplanted as we gain control over our theories and the methods by which we elaborate their consequences? And does one perspective or another limit our ability to advance our understanding?

1.3. Some Other Meta-Questions

I would like to bring these introductory remarks to a close by pointing you toward some meta-questions that I hope you will think about during the course of the summer institute. I call them to your attention because some wise people (including wise

⁶It is a delicious irony that string theorists, whose top-down style seems particularly vexing to Laughlin & Pines and their allies, may turn out to be—if landscape ideas take hold or spacetime is emergent—the ultimate emergentists!

people from our own community, and even wise people from Stanford, California) have been pondering them as questions that might be moving toward scientific questions, to which we may hope to find scientific answers.

- ▷ Is this the best of all possible worlds? Dr. Pangloss’s assertion, though burdened with ironical baggage, carries with it the daring suggestion that other worlds are thinkable [15]. According to an enduring dream that has probably infected all of us from time to time, the theory of the world might prove to be so restrictive that things have to turn out the way we observe them. Is this really the way the world works, or not? Are the elements of our standard model—the quarks and leptons and gauge groups and coupling constants—inevitable, at least in a probabilistic sense, or did it just happen this way?⁷
- ▷ Is Nature simple or complex? And if we take the sophisticate’s view that it is both, which aspects will have beautiful “simple” explanations and which explanations will remain complicated?
- ▷ Are Nature’s Laws the same at all times and places? Yes, of course they are, to good approximation *in our experience*. Otherwise science would have had to confront a universe that is in some manner capricious. But *all* times and *all* places is a very strong conclusion, for which we cannot have decisive evidence. Many people have been thinking about multiple universes in which there may be different incarnations of the basic structures.⁸
- ▷ Can one theoretical structure account for “everything,” or should we be content with partial theories useful in different domains? Can we really expect⁹ to have a theory that applies from the lowest energies to the highest, from the smallest distances to the greatest?

All these questions are a bit woolly and may even be undecidable; they could generate a lot of blather and not lead to any telling insights. But we would be mistaken to pretend they are not there. So I urge you to spend a little of your time at the summer institute thinking about what constitutes a scientific explanation.

To work toward your own understanding of the Galilean relationship between small questions and sweeping insights, and to practice presenting the significance of your work to the wider world, please complete the following

Exercise. Explain in a paragraph or two how your current research project relates to Great Questions about Nature or is otherwise irresistibly fascinating. Be prepared to present your answer to a science writer at a SSI social event.

2. ANTICIPATION

2.1. A Decade of Discovery Past

Before I move on to explore some themes that bind together the questions that our organizers have given us (and some other topics), I want to emphasize again that we stand on the threshold of a great flowering of experimental particle physics and of dramatic progress in theory—especially that part of theory that engages with experiment.

We particle physicists are impatient and ambitious people, and so we tend to regard the decade just past as one of consolidation, as opposed to stunning breakthroughs. But an objective look at the headlines of the past ten years gives us a very impressive list of discoveries. It is important that we know this for ourselves, and that we convey our sense of achievement and promise to others.¹⁰

⁷The paradigm of the “string theory landscape” offers a very particular take on this question [16, 17]. The string-theory landscape and anthropic cosmological arguments [18] seem to me to fall in the tradition of Charles Sanders Peirce’s “Design and Chance,” in *The Essential Peirce*, vol. 1 (1867 – 1893), ed. Nathan Houser and Christian Kloesel (University of Indiana Press, Bloomington & Indianapolis, 1992), p. 215. For a brief description of Peirce’s evolutionary cosmology, see Louis Menand, *The Metaphysical Club: A Story of Ideas in America* (Farrar Straus Giroux, New York, 2001), pp. 275–280. For a pithy critique of the anthropic approach, see the short essay by Paul Steinhardt at http://www.edge.org/q2005/q05_print.html#steinhardt.

⁸For one provocative definition of universes, see Ref. [19].

⁹This issue has been joined recently by Freeman Dyson [20] and Brian Greene [21].

¹⁰The citations that follow are to pertinent lectures at this school, rather than to the original literature.

- ▷ The electroweak theory has been elevated from a very promising description to a *law of nature*. It is quite remarkable that in a short time we have gone from a conjectured electroweak theory to one that is established as a real quantum field theory, tested as a quantum field theory at the level of one per mille in many many observables [22]. This achievement is truly the work of many hands; it has involved experiments at the Z^0 pole, the study of e^+e^- , $\bar{p}p$, and νN interactions, and supremely precise measurements such as the determination of $(g - 2)_\mu$ [23].
- ▷ Electroweak experiments have observed what we may reasonably interpret as the influence of the Higgs boson in the vacuum [22, 24, 25].
- ▷ Experiments using neutrinos generated by cosmic-ray interactions in the atmosphere, by nuclear fusion in the Sun, and by nuclear fission in reactors, have established neutrino flavor oscillations: $\nu_\mu \rightarrow \nu_\tau$ and $\nu_e \rightarrow \nu_\mu/\nu_\tau$ [26, 27, 28, 29, 30].
- ▷ Aided by experiments on heavy quarks, studies of Z^0 , investigations of high-energy $\bar{p}p$, νN , and ep collisions, and by developments in lattice field theory, we have made remarkable strides in understanding quantum chromodynamics as the theory of the strong interactions.
- ▷ The top quark, a remarkable apparently elementary fermion with the mass of an osmium atom, was discovered in $\bar{p}p$ collisions [31, 32].
- ▷ Direct \mathcal{CP} violation has been observed in $K \rightarrow \pi\pi$ decay.
- ▷ Experiments at asymmetric-energy $e^+e^- \rightarrow B\bar{B}$ factories have established that B^0 -meson decays do not respect \mathcal{CP} invariance [33].
- ▷ The study of type-Ia supernovae and detailed thermal maps of the cosmic microwave background reveal that we live in an approximately flat universe dominated by dark matter and energy [34, 35, 36, 37, 38].
- ▷ A “three-neutrino” experiment has detected the interactions of tau neutrinos.
- ▷ Many experiments, mainly those at the highest-energy colliders, indicate that quarks and leptons are structureless on the 1-TeV scale.

We have learned an impressive amount in ten years, and I find quite striking the diversity of experimental and observational approaches that have brought us new knowledge, as well as the richness of the interplay between theory and experiment.

Now I want to talk about five themes that weave together the great questions and small that we will be talking about during these two weeks.

3. UNDERSTANDING THE EVERYDAY

The first theme is one on which I am rather confident that we will make enormous progress over the next decade. That is the problem of understanding the everyday, the stuff of the world around us. It pertains to basic questions: Why are there atoms? Why is there chemistry? Why are stable structures possible? And even, knowing the answers to those questions gives us an insight into What makes life possible?

Those are the general questions that we are seeking to answer when we look for the origin of electroweak symmetry breaking. I think that the best way to make the connection is to consider what the world would be like if there were no mechanism, like the Higgs mechanism, for electroweak symmetry breaking. It’s important to look at the problem in this way, because in the public presentations of the aspiration of particle physics we hear too often that the goal of the LHC or a linear collider is to check off the last missing particle of the standard model, this year’s Holy Grail of particle physics, the Higgs boson. *The truth is much less boring than that!* What we’re trying to accomplish is much more exciting, and asking what the world would have been like without the Higgs mechanism is a way of getting at that excitement.

First, it’s clear that quarks and leptons would remain massless, because mass terms are not permitted in our left-handed world if the electroweak symmetry remains manifest.¹¹ We’ve done nothing to QCD, so that would still confine the (massless) color-triplet quarks into color-singlet hadrons, with very little change in the masses of those stable structures. In particular, the nucleon mass would be essentially unchanged, but the proton would outweigh the neutron because the down quark now does not outweigh the up quark, and that change will have its own consequences.

¹¹I assume for this discussion that all the trappings of the Higgs mechanism, including Yukawa couplings for the fermions, are absent.

An interesting, and slightly subtle point is that, even in the absence of a Higgs mechanism, the electroweak symmetry is broken by QCD, precisely by one of the emergent phenomena we have just discussed in §1.2 [39]. As we approach low energy in QCD, confinement occurs and the chiral symmetry that treated the massless left-handed and right-handed quarks as separate objects is broken. The resulting communication between the left-handed and right-handed worlds engenders a breaking of the electroweak symmetry. The trouble is that the scale of electroweak symmetry breaking is measured by the pseudoscalar decay constant of the pion, so the amount of mass acquired by the W and Z is set by f_π , not by what we know to be the electroweak scale: it is off by a factor of 2500.

But the fact is that the electroweak symmetry is broken, so the world without a Higgs mechanism—but with strong-coupling QCD—is a world in which the $SU(2)_L \otimes U(1)_Y$ becomes $U(1)_{em}$. Because the W and Z have masses, the weak-isospin force, which we might have taken to be a confining force in the absence of symmetry breaking, is not confining. Beta decay is very rapid, because the gauge bosons are very light. The lightest nucleus is therefore one neutron; there is no hydrogen atom. There's been some analysis of what would happen to big-bang nucleosynthesis in this world; that work suggests that some light elements such as helium would be created [40, 41, 42, 43].¹² Because the electron is massless, the Bohr radius of the atom is infinite, so there is nothing we would recognize as an atom, there is no chemistry as we know it, there are no stable composite structures like the solids and liquids we know.

I invite you to explore this scenario in even greater detail. [To do so is at least as challenging as trying to understand the world we do live in.] The point is to see how very different the world would be, if it were not for the mechanism of electroweak symmetry breaking whose inner workings we intend to explore and understand in the next decade. What we are really trying to get at, when we look for the source of electroweak symmetry breaking, is why we don't live in a world so different, why we live in the world we do. I think that's a glorious question. It's one of the deepest questions that human beings have ever tried to engage, and *you* will answer this question.

What could the answer be? As far as we can tell, because we have an *effective field theory* description, the agent of electroweak symmetry breaking represents a novel fundamental interaction at an energy of a few hundred GeV. As we parametrize it in the standard electroweak theory, and we contrive the Higgs potential, it is not a gauge force but a completely new kind of interaction. We do not know what that force is.

What could it be? It could be the Higgs mechanism of the standard model, which is built in analogy to the Ginzburg–Landau description of superconductivity. Maybe it is a new gauge force. One very appealing possibility—at least until you get into the details—is that the solution to electroweak symmetry breaking will be like the solution to the model for electroweak symmetry breaking, the superconducting phase transition. The superconducting phase transition is first described by the Ginzburg–Landau phenomenology, but then in reality is explained by the Bardeen–Cooper–Schrieffer theory that comes from the gauge theory of Quantum Electrodynamics. Maybe, then, we will discover a mechanism for electroweak symmetry breaking almost as economical as the QCD mechanism we discussed above. One line that people have investigated again and again is the possibility that there are new constituents still to be discovered that interact by means of forces still to be discovered, and when we learn how to calculate the consequences of that theory we will find our analogue of the BCS theory. It could even be that there is some truly emergent description—at this level—of the electroweak phase transition, a residual force that arises from the strong dynamics among the weak gauge bosons. We know that if we take the mass of the Higgs boson to very large values, beyond a TeV in the Lagrangian of the electroweak theory, the scattering among gauge bosons becomes strong, in the sense that $\pi\pi$ scattering becomes strong on the GeV scale. Resonances form among pairs of gauge bosons, multiple production of gauge bosons becomes commonplace, and that resonant behavior could be what hides the electroweak symmetry. We'll also hear during these two weeks about the possibility that electroweak symmetry breaking is the echo of extra spacetime dimensions. We don't know, and we intend to find out during the next decade which path nature has taken.

One very important step toward understanding the new force is to find the Higgs boson and to learn its properties. I've said before in public, and I say again here, that the Higgs boson will be discovered whether it exists or not. That is a statement with a precise technical meaning. There will be (almost surely) a spin-zero object that has effectively more or less the interactions of the standard-model Higgs boson, whether it is an elementary particle that we put into to the theory or something that emerges

¹²It would be interesting to see this worked out in complete detail.

from the theory. Such an object is required to get good high-energy behavior of the theory.

If something will be found, what is it? How many are there? Is its spin-parity what we expect ($J^{PC} = 0^{++}$) in the electroweak theory? Does it generate mass for the gauge bosons W and Z alone, or does it generate mass for the gauge bosons and the fermions? How does it interact with itself?

There will be a party on the day the Higgs boson is discovered, but it will mark the beginning of a lot of work!

4. THE MEANING OF IDENTITY

The second theme has to do with the cast of characters, the basic constituents of matter, the quarks and leptons. It involves the question, “What makes a top quark a top quark, an electron an electron, a neutrino a neutrino? What distinguishes these objects?” Now, maybe this is a Kepler-style question that we shouldn’t be asking, but it is a tantalizing question in any event.

What do I mean by this more precisely? I mean, what sets the masses and mixings of the quarks and leptons? This has to do with the famous CKM matrix of quark mixings, which our colleagues here and elsewhere are measuring so assiduously. These elements arise, in the standard model, in the course of electroweak symmetry breaking with values set by those famous arbitrary Yukawa couplings, whose values we don’t know except by experiment. What is \mathcal{CP} violation really trying to tell us? One of the things I am most confused about is what discrete symmetries mean, when they are exact and when they are broken. Are parity violation and \mathcal{CP} violation intrinsic defects—or essential features—of the laws of nature, or do they represent spontaneously broken symmetries?

Neutrino oscillations—flavor-changing transitions, more generally—give us a new look at the meaning of identity, because they, too, have to do with fermion masses and identities. Neutrino masses can be generated in the old ways, through Yukawa couplings, and in new ways as well [44], so they may give us a new take on the problem, and add richness to it. We often hear that neutrino mass is evidence for physics beyond the standard model.

I’m here to tell you that *all fermion masses, starting with the electron mass, are evidence for physics beyond the standard model*. The reason in this: while, in the electroweak theory a little box pops up and says, “Write the electron mass here,” nothing in the electroweak theory—either now or at any time in the future—is going to tell us how to calculate that number. It’s not that the calculation is technically challenging, it is that the electroweak theory has nothing to say about fermion mass. All of these masses are profoundly mysterious. Neutrino masses could present an additional mystery, because neutrinos can be their own antiparticle, which means there are other ways of generating neutrino masses. There is a real enigma here, one that we need to get our minds around.

Maybe we haven’t figured out what the pattern is because there is more to see in the pattern [45, 46, 47, 48]. Perhaps it will only become apparent when we take into account the masses of superpartners or other kinds of matter. It’s worth remembering that when Mendele’ev made his periodic table, he constructed it out of the chemical elements that had been discovered by chemists. The chemicals discovered by chemists are the chemicals that have chemistry; and so Mendele’ev didn’t know about helium, neon, argon, krypton, xenon. If you had tried to see the pattern, you would have made real progress filling in the missing elements, but without the noble gases that we now think of as the last column, you wouldn’t have had the clues necessary to build up, in a systematic way, the properties of the elements, or to guess what lies behind the periodic table. Perhaps we need to see something more—an analogue of the noble gases—before we can understand what lies behind the pattern.

I’m less confident that in ten years we will get to the bottom of this theme, because I really think that we are at the stage of developing for ourselves what this question is. We know very well what are the measurements we’d like to make in B physics, charm and strange physics, and neutrino physics—which elements of the mixing matrices we would like to fill in and which relationships we would like to test. But I don’t think we’ve done a satisfactory job yet of constructing what the big question is, and what the properties of the fermions are trying to tell us. I think it is very important that we try to think of the quarks and leptons together, to see what additional insights a common analysis might bring, and to try to understand what the question really is here.

Among the extensions to the standard model that might give us clues into the larger pattern there is, of course, supersymmetry. In common with many extensions to the standard model, supersymmetry brings us dark matter candidates [49]. Supersymmetry

is very highly developed. It has a number of very important consequences if it is true.¹³ First, if the top quark is heavy and a few other things happen in the right way, then supersymmetry predicts the condensation that gives rise to the hiding of electroweak symmetry. It can generate, by the running of masses, the shape of the Higgs potential. It predicts a light Higgs mass, less than some number in the neighborhood of 130, 140, 150 GeV. That's consistent with the current indications from precision electroweak measurements. It predicts cosmological cold dark matter, which seems to be a good thing to have. It might lead to an understanding of the excess [50, 51] of matter over antimatter in the universe [52, 53]. And, in a unified theory, it explains the (relative) values of the standard-model coupling constants. To see that, we have to move on to the next theme.

5. THE UNITY OF QUARKS AND LEPTONS

The quarks have strong interactions, as you all know, and the leptons don't. Could we have a world made only of quarks, or only of leptons? There are many strong reasons for believing that quarks and leptons must have something to do with each other, despite their different behavior under the strong interactions. What do they have in common? They are all spin- $\frac{1}{2}$ particles, structureless at the current limits of resolution. The six quarks match the six leptons. What motivates us to think of a world in which the quarks and leptons are not just unrelated sets that match by chance, but have a deep connection? The simplest way to express it, I think, is to go back to a puzzle of very long standing, why atoms are so very nearly neutral. This is one of the best measured numbers close to zero in all of experimental science: atoms are neutral to one part in 10^{22} .

If there is no connection between quarks and leptons, since quarks make up the proton, then the balance of the proton and electron charge is just a remarkable coincidence. It seems impossible for any thinking person to be satisfied with coincidence as an explanation. Some principle must relate the charges of the quarks and the leptons. What is it? A fancier way of saying it, and more or less equivalent, is that for the electroweak theory to make sense up to arbitrarily high energies, the symmetries on which it is based must survive quantum corrections. The way we say that is that the theory must be free of anomalies—quantum corrections that break the gauge symmetry on which the theory is based. In our left-handed world, that is only possible if weak-isospin pairs of color-triplet quarks accompany weak-isospin pairs of color-singlet leptons. For these reasons, it is nearly irresistible to consider a unified theory that puts quarks and leptons into a single extended family.

Once you've done that, it's a natural implication that protons should decay. Although it's a natural implication, it may not be unavoidable, because we don't know which quarks go with which leptons. If you look at the tables chiseled in marble out in the hallway to celebrate the Nobel Prize of 1976, you will see that the up and down quarks go with the electron and its neutrino. We have no experimental basis for that arrangement, it just reflects the order in which we met the particles. For all we know, the first generation of quarks goes with the third generation of neutrinos. Supersymmetry is interesting in this context because it sets an experimental target that's not so far away—an order of magnitude or two away: Perhaps that target provides enough stimulus—if we can think of how to build a massive, low-background apparatus at finite cost—to go the next order of magnitude or two in sensitivity, perhaps to find evidence for proton decay, which would be the definitive proof of the connection between quarks and leptons.

Coupling constants unify in the unified theory. At some high scale, whose value we might discover in some future theory, all the couplings have a certain value. The differing values we see at low energy for the U(1) associated with weak hypercharge, the SU(2) associated with weak isospin, and the SU(3) associated with color come about because of the different evolution given by the different gauge groups and the spectrum of particles between up there and down here. In this sense we can explain why the strong interactions are strong on a certain scale.

One way of thinking about the masses of the quarks and leptons is to imagine that the pattern just looks weird to us because we are examining the fermion masses at low energies. Masses run with momentum scale in a way analogous to the running of coupling constants. So possibly, if we look at very high energies, we will see a rational pattern that relates one mass to another through Clebsch–Gordan coefficients or some other symmetry factors. There are examples of this. One of the nice

¹³By which I mean, if it is true and relevant on the 1-TeV scale. Supersymmetry might be true and shape physics on the Planck scale but have nothing directly to do with these issues.

fantasy studies for the linear collider is measuring masses of superpartners well enough at low energies to have the courage to extrapolate them over fourteen or fifteen orders of magnitude in energy, to see how they come together [54].

6. GRAVITY REJOINS PARTICLE PHYSICS REJOINS GRAVITY REJOINS ...

We particle physicists have neglected gravity all these years, and for good reason. If we calculate a representative process, kaon decay into a pion plus a graviton for example, it's easy to estimate that the emission of a graviton is suppressed by M_K/M_{Planck} . The Planck mass ($M_{\text{Planck}} \equiv (\hbar c/G_{\text{Newton}})^{1/2} \approx 1.22 \times 10^{19}$ GeV) is a big number because Newton's constant is small in the appropriate units. A dimensional estimate for the branching fraction is $B(K \rightarrow \pi G) \approx (M_K/M_{\text{Planck}})^2 \approx 10^{-38}$. It will be a long time before the single-event sensitivity of any kaon experiment reaches this level! And that's why we have been able to safely neglect gravity most of the time.

All of us have great respect for the theory of gravity, because it was given to us by Einstein and Newton and the gods, whereas we know the people who made the electroweak theory, and so it's natural to think that gravity must be true. But from the experimental point of view, we know very little about gravity at short distances [55]. Down to a few tenths of a millimeter, elegant experiments [56, 57] using torsion oscillators and microcantilevers exclude a deviation from Newton's inverse-square law with strength comparable to gravity's. The techniques and the bounds are very impressive! But at shorter distances, the constraints deteriorate rapidly, so nothing prevents us from considering changes to gravity even on a small but macroscopic scale. Even after this new generation of experiments, we have only tested our understanding of gravity—through the inverse-square law—up to energies of 10 meV (yes, *milli*-electron volts), some fourteen orders of magnitude below the energies at which we have studied QCD and the electroweak theory. That doesn't mean that a deviation from the inverse-square law is just around the corner, but experiment plainly leaves an opening for gravitational surprises. Indeed, it is an open possibility that at *larger* distances than we have observed astronomically gravity might deviate from the inverse-square law. There is a huge field over which gravity might be different from Newton's law, and we wouldn't have discovered it yet.

Now, in spite of the fact that we have had good reason to neglect gravity in our daily calculations of Feynman diagrams, we have also been keenly aware that gravity is not always negligible. In more or less any interacting field theory, and certainly in one like the electroweak theory, where the Higgs field has a nonzero value that fills all of space, all of space has some energy density. In the electroweak theory, that energy density turns out to be really large. If you calculate it, you find that the contribution of the Higgs field's vacuum expectation value to the energy density of the universe is $\rho_H \equiv M_H^2 v^2/8$, where M_H is the Higgs-boson mass and $v \approx 246$ GeV is the scale of electroweak symmetry breaking. A vacuum energy density corresponds to a cosmological constant $\Lambda = (8\pi G_{\text{Newton}}/c^4)\rho_{\text{vac}}$ in Einstein's equations. We've known for a very long time that there is not much of a cosmological constant, that the vacuum energy has to be less than about $\rho_{\text{vac}} \lesssim 10^{-46}$ GeV⁴, a very little number. It corresponds to ≈ 10 MeV/ ℓ or 10^{-29} g cm⁻³. Even in the blackest heart, there is not much dark energy!

But if we use the current lower limit on the Higgs-boson mass, $M_H \gtrsim 114$ GeV, to estimate the vacuum energy in the electroweak theory, we find $\rho_H \gtrsim 10^8$ GeV⁴. That is wrong by no less than fifty-four orders of magnitude! This mismatch has been known for about three decades. That long ago, Tini Veltman was concerned that something fundamental was missing from our conception of the electroweak theory. For many of us, the vacuum energy problem has been a chronic dull headache for all this time.

This raises an interesting point about how science is done, and how science progresses. We could, all of us, have said, "The electroweak theory is wrong, let's put it aside." Think of all that we wouldn't know, if we had followed that course. We can't forget about deep problems like the vacuum energy conflict, but we have to have the sense to put them aside, to defer consideration until the right moment. In the simplest terms, the question is, "Why is empty space so nearly massless?" That is a puzzle that has been with us repeatedly in the history of physics, and it is one that is particularly pointed now. Maybe now should be the time that we return to the vacuum energy problem.

Over the last few years, we have a new wrinkle to the vacuum energy puzzle, the evidence—within a certain framework of analysis—for a nonzero cosmological constant, respecting the bounds cited a moment ago. That discovery recasts the problem in two important ways. First, instead of looking for a principle that would forbid a cosmological constant, perhaps a symmetry principle that would set it exactly to zero, now we have to explain a tiny cosmological constant! Whether we do that in two steps or one step remains to be seen. Second, from the point of view of the dialogue among observation and experiment and theory,

now it looks as if we have access to some new stuff whose properties we can measure. Maybe that will give us the clue that we need to solve this old problem.

We now come to the question of how we separate the electroweak scale from higher scales [58]. This is a realm in which we haven't neglected gravity all along, because we have wanted to think of the electroweak theory as a truly useful effective theory, and we have known that we live in a world in which the electroweak scale isn't the only scale. We have taken note of the Planck scale, and there may be a unification scale for strong, weak, and electromagnetic interactions; for all we know, there are intermediate scales, where flavor properties are determined and masses are set.

We know that the Higgs-boson mass must be less than a TeV, but the scalar mass communicates quantum-mechanically with the other scales that may range all the way up to 10^{19} GeV. How do we keep the Higgs-boson mass from being polluted by the higher scales? That's the essence of the hierarchy problem. We've dealt with this, for twenty-five years or so, by extending the standard model. Maybe the Higgs boson is a composite particle, maybe we have broken supersymmetry that tempers the quadratic divergences in the running of the Higgs-boson mass, maybe Now, because of the observation that we haven't tested gravity up to very high energies, it has become fashionable to turn the question around and ask why the Planck scale is so much bigger than the electroweak scale, rather than why the electroweak scale is so low. In other words, why is gravity so weak?

7. A NEW CONCEPTION OF SPACETIME

That line of investigation has given rise to new thinking, part of it connected with a new conception of spacetime. What is in play here, again, is a question so old that, for a long time, we had forgotten that it was a question: Is spacetime really three-plus-one dimensional? What is our evidence for that? How well do we know that there are not other, extra, dimensions? What must be the character of those extra dimensions, and the character of our ability to investigate them, for them to have escaped our notice?

Could extra dimensions be present? What is their size? What is their shape? What influence do they exert on our world? (Because if they have no effect, it almost doesn't matter that they exist.) Are the extra dimensions where fermion masses are set, or electroweak symmetry is broken, or what? How can we map them? How can we attack the question of extra dimensions experimentally?

I will give you just two examples of new ways of thinking that are stimulated by the notion that additional dimensions have eluded detection. These are both probably wrong, and that hardly matters, because they are mind-expanding.

Perhaps, in contrast to the strong and electroweak gauge forces, gravity can propagate in the extra dimensions—in all dimensions, because it is universal. When we inspect the world on small enough scales, we will see gravity leaking into the extra dimensions. Then by Gauss's law, the gravitational force will not be an inverse-square law, but will be proportional to $1/r^{2+n}$, where n is the number of extra dimensions. That would mean that, as we extrapolate to smaller distances, or higher energies, gravity will not follow the Newtonian form forever, as we conventionally suppose. Below a certain distance scale, it will start evolving more rapidly; its strength will grow faster. Therefore it might join the other forces at a much lower energy than the Planck scale we have traditionally assumed. That could change our perception of the hierarchy problem entirely. That's a way we hadn't thought about the problem before. It has stimulated a lot of research into how we might detect extra dimensions [59, 60].

Perhaps extra dimensions offer a new way to try to understand fermion masses [61]. One of the great challenges—beyond the fact that we don't have a clue how to calculate fermion masses—is that the fermion masses have such wildly different values. In units of $v/\sqrt{2}$, the mass of the top quark is 1, the mass of the electron is a few $\times 10^{-6}$, and so on. How can a reasonable theory generate such big differences? Suppose, for simplicity, that spacetime has one additional dimension. In that extra dimension, wave packets correspond to left-handed and right-handed fermions. For reasons to be supplied by a future theory, each wave packet rides on a different rail (is centered on a different value of the new coordinate, x_{new}). It is the overlap between a left-handed wave packet, a right-handed wave packet, and the Higgs field—assumed to vary little with x_{new} —that sets the masses of the fermions. If the wave packets are Gaussian (how else could they be?) then they need only be offset by a little in order for the overlap integral to change by a lot. I don't know whether this story can possibly be right, but it is very different from any other story we have told ourselves about fermion masses. For that reason, I think it is an important opening.

Other extra-dimensional delights may present themselves, provided that gravity is intrinsically strong but spread out into many dimensions. Tiny black holes might be formed in high-energy collisions [62]. We might have the possibility of detecting the exchange or emission of gravitons—not as individual gravitons, but as towers of them [63].¹⁴ At all events, gravity is here to stay in particle physics. It's been present for years as a headache, in the form of the hierarchy problem and in the challenge of the vacuum energy problem. Now it is perhaps presenting itself as an opportunity!

8. THE DOUBLE SIMPLEX

As I intimated in §1.1, I have been concerned for some time with the prevailing narrow view of the goals of our science. It is troubling, to be sure, when we read in the popular press that the sole object of our endeavors is to find—to check off, if you will—the Higgs boson, the holy grail (at least for this month) of particle physics.¹⁵ What is more troubling to me, the shorthand of the Higgs search narrows the discourse within our own community. In response, I have begun to evolve a visual metaphor—the double simplex—for what we know, for what we hope might be true, and for the open questions raised by our current understanding. While I have a deep respect for the refiner's fire that is mathematics, I believe that we should be able to explain the essence of our ideas in languages other than equations. I interpolated a brief animated overview [64] of the double simplex¹⁶ at this point in my lecture. For a preliminary exposition in a pedagogical setting, see Ref. [65]. A more complete explanation of the aims of particle physics through the metaphor of the double simplex is in preparation.

9. ANTICIPATION

9.1. A decade of discovery ahead

I spoke at the beginning of the hour about the decade of discovery just achieved. I believe that the decade ahead will be a real golden age of exploration and discovery.

- ▷ We will make a thorough exploration of the 1-TeV energy scale; search for, find, and study the Higgs boson or its equivalent; and probe the mechanism that hides electroweak symmetry. Decisive progress will come from our (anti)proton-proton colliders, notably the Large Hadron Collider at CERN, but we envisage a TeV-scale electron-positron linear collider to give us a second look, through a different lens.¹⁷
- ▷ We will continue to challenge the standard model's attribution of \mathcal{CP} violation to a phase in the quark mixing matrix, in experiments that examine B decays and rare decays—or mixing—of strange and charmed particles. Fixed-target experiments, as well as e^+e^- and $p^\pm p$ colliders, will contribute.
- ▷ New accelerator-generated neutrino beams, together with reactor experiments and the continued study of neutrinos from natural sources, will consolidate our understanding of neutrino mixing. Double-beta-decay searches may confirm the Majorana nature of neutrinos. And do not dismiss the possibility that three neutrinos will not suffice to explain all observations!
- ▷ The top quark will become an important window into the nature of electroweak symmetry breaking, rather than a mere object of experimental desire. Single-top production and the top quark's coupling to the Higgs sector will be informative. Hadron colliders will lead the way, with the LC opening up additional detailed studies.
- ▷ The study of new phases of matter and renewed attention to hadronic physics will deepen our appreciation for the richness of QCD, and might even bring new ideas to the realm of electroweak symmetry breaking.¹⁸ Heavy-ion collisions have a

¹⁴In the colloquy cited in §1.3, Freeman Dyson asserts that we don't need a quantum theory of gravity because single graviton emission can never be detected. We would say that he is mistaken, but the dialogue reveals an interesting contrast of styles and world-views.

¹⁵I say this as someone whose obsession with electroweak symmetry breaking is no secret!

¹⁶Any resemblance to Kepler's *stella octangula* is purely coincidental.

¹⁷It is wrong to say, as well-meaning people sometimes do, that the LHC is a blunt instrument and the LC a scalpel. A more apt analogy is to the suite of telescopes—radio, infrared, optical, ultraviolet, X-ray, etc.—that enrich astronomical observations. Each instrument is made more capable by the dialogue with its companions.

¹⁸It bears repeating that we owe most of our ideas about electroweak symmetry breaking to the superconducting phase transition.

- special role to play here, but ep collisions, fixed-target experiments, and $p^\pm p$ and e^+e^- colliders all are contributors.
- ▷ Planned discoveries and programmatic surveys have their (important!) place, but exploration breaks the mold of established ideas and can recast our list of urgent questions overnight. The LHC, not to mention a whole range of experiments down to tabletop scale, will make the coming decade one of the great voyages into the unknown. Among the objectives we have already prepared in great theoretical detail are extra dimensions, new strong dynamics, supersymmetry, and new forces and constituents. Any one of these would give us a new continent to explore.
 - ▷ Proton decay remains the most promising path to establish the existence of extended families that contain both quarks and leptons. Vast new underground detectors will be required to push the sensitivity frontier.
 - ▷ We will learn much more about the composition of the universe, perhaps establishing the nature of some of the dark matter. Observations of type Ia supernovae, the cosmic microwave background, and the large-scale structure of the universe will extend our knowledge of the fossil record. Underground searches may give evidence of relic dark matter. Collider experiments will establish the character of dark-matter candidates and will make possible a more enlightened reading of the fossil record.

These few items constitute a staggeringly rich prospectus for search and discovery and for enhanced understanding. Exploiting all these opportunities will require many different instruments, as well as the toil and wit of many physicists. Fred Gilman [66] will offer a roadmap to the future at the end of the school, but it is plain that one of our great challenges is to think clearly about the diversity of our experimental initiatives, and about scale diversity of those initiatives. It is relatively easy to write the major headlines of the program we would like to see. But how do we create the institutions that year after year make important measurements? How do we create the next set of Greatest Puzzles? That, it seems to me, is a very significant issue for people who will be part of our field over the next thirty years.

I leave you with a list of advances that I believe can happen over the next decade or so. I put up my list for the same reason, I think, that the organizers of the school gave you their list—because then you can object to it, and make your own! We will . . .

Understand electroweak symmetry breaking, Observe the Higgs boson, Measure neutrino masses and mixings, Establish Majorana neutrinos through the observation of neutrinoless double-beta decay, Thoroughly explore \mathcal{CP} violation in B decays, Exploit rare decays (K , D , . . .), Observe the neutron's permanent electric dipole moment, and pursue the electron's electric dipole moment, Use top as a tool, Observe new phases of matter, Understand hadron structure quantitatively, Uncover the full implications of QCD, Observe proton decay, Understand the baryon excess of the universe, Catalogue the matter and energy of the universe, Measure the equation of state of the dark energy, Search for new macroscopic forces, Determine the gauge symmetry that unifies the strong, weak, and electromagnetic interactions, Detect neutrinos from the universe, Learn how to quantize gravity, Learn why empty space is nearly weightless, Test the inflation hypothesis, Understand discrete symmetry violation, Resolve the hierarchy problem, Discover new gauge forces, Directly detect dark-matter particles, Explore extra spatial dimensions, Understand the origin of the large-scale structure of the universe, Observe gravitational radiation, Solve the strong \mathcal{CP} problem, Learn whether supersymmetry operates on the TeV scale, Seek TeV-scale dynamical symmetry breaking, Search for new strong dynamics, Explain the highest-energy cosmic rays, Formulate the problem of identity, . . .

. . . and learn the right questions to ask!

10. NATURE'S NEGLECTED PUZZLES

I've given you my view of how our puzzles and opportunities and clues fit together, of how we might think about our field and evolution. The organizers have given you their picture, with ten themes for ten days of our school. To encourage lively participation and debate, I issued . . .

The NNP Challenge: Propose a question not on the SSI2004 list, and explain briefly why it belongs in the pantheon of Nature's Greatest Puzzles [67].

The contest was open to any student at the SLAC Summer Institute—anybody willing to propose a new question to be judged by our international panel of experts.

I presented the reward for the Best Eleventh Question on Wednesday, August 11 to SISSA/SLAC graduate student Yasaman Farzan, for her question about the validity of Poincaré invariance:

To what extent is Poincaré symmetry exact? Looking back on the history of science, discovering that different symmetries are not exact has ushered in a new era. Poincaré symmetry is particularly interesting because it is currently considered the most sacred geometry. Moreover, its evolution to the form we learn about today has marked great revolution in physics, in the past.

Yasaman's trophy, a bottle of California's finest sparkling wine,¹⁹ bears the autographs of Nobel Laureates Martin Perl and Burton Richter; SLAC notables Jonathan Dorfan, Persis Drell, Sid Drell, and Vera Lüth; High Energy Physics Advisory Panel Chair Fred Gilman; SLAC Summer Institute organizers JoAnne Hewett, John Jaros, Tune Kamae, and Charles Prescott; and my own. Even more precious was the opportunity—need we say, obligation—to present and defend the best eleventh question in an eleven-minute talk at that day's afternoon Discussion Session. Padova student Marco Zanetti and Colorado State/UCSD student Thomas Topel received special commendations for their questions on the nature of time and the mechanism that breaks the strong–electroweak symmetry. Their prizes are copies of Peter Galison's recent book, *Einstein's Clocks, Poincaré's Maps: Empires of Time*. Thanks and congratulations to all [68] who entered the Challenge!

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