We are emerging from a period of consolidation in particle physics. Its great, historic achievement was to establish the Theory of Matter. This Theory will serve as our description of ordinary matter under ordinary conditions – allowing for an extremely liberal definition of “ordinary” – for the foreseeable future. Yet there are many indications, ranging from the numerical to the semi-mystical, that a new fertile period lies before us. We will discover compelling evidence for the unification of fundamental forces and for new quantum dimensions (low-energy supersymmetry). We will identify new forms of matter, which dominate the mass density of the Universe. We will achieve much better fundamental understanding of the behavior of matter in extreme astrophysical and cosmological environments. Lying beyond these expectations, we can identify deep questions that seem to call for ideas outside our present grasp. And there’s still plenty of room for surprises.

It is altogether appropriate, I think, to end this celebration of the completion of a great scientific program, and the retirement of a great machine, with a look to the future. The historic achievement of LEP has been to establish, with an astonishing degree of rigor and beyond all reasonable doubt, what will stand for the foreseeable future – perhaps for all time – as the working Theory of Matter.

Many years ago, on the occasion of the founding of the Cavendish lab, James Clerk Maxwell said

The history of science shows that even during that phase of her progress in which she devotes herself to improving the accuracy of the numerical measurements of quantities long familiar, she is preparing the materials for the subjugation of new regions, which would have remained unknown if she had been contented with the rough methods of her early pioneers.

These words of Maxwell have turned out to be prophetic, of course, many times over. Precision measurements of the blackbody spectrum helped lead to early quantum theory. Precision measurements on the spectrum of hydrogen helped lead to modern quantum mechanics and ultimately to quantum field theory. Precision measurements on the K-meson system helped lead, in one way or another, to the discovery of parity violation, CP violation, and charm. Precision measurements in deep inelastic scattering helped lead to modern quantum chromodynamics, or QCD. I believe, for reasons I will enumerate shortly, that they apply again today. For the

*Closing talk delivered at the LEPfest, CERN, October 11, 2000. It closely resembles the closing talk I delivered at the DPF meeting, Columbus, Ohio, August 2000. It will be published in the Proceedings.
precision measurements made at LEP, besides establishing the Theory of Matter, give us some very definite and specific clues for what lies beyond that Theory. They hint at new worlds of phenomena, connected with the completion and structural unification of the Theory of Matter, and with the existence of new quantum dimensions. It is fitting, poetic – and very exciting – that the systematic exploration of these new worlds will very likely commence with the commissioning of LEP’s literal descendant, the Large Hadron Collider (LHC).

1. The Theory of Matter

Before discussing where we might be going it is reasonable to reflect on where we are, and how we got there.

In 1900, physics had a completely different character from what it has today. It described how matter will evolve, given its initial condition. Classical physics cannot explain why there are material substances with definite, reproducible properties at all, much less why there are just the particular molecules, atoms, and nuclei with the specific properties we observe in Nature. In short, classical physics cannot address questions about what matter is, or why it is that way.

Modern physics does address these “what” and “why” questions. Indeed, most of us believe – for very solid reasons, I think – that we have in hand, formulated quite precisely, the laws that in principle answer the central “what” and questions about matter, and advance the “why” questions to new levels of abstraction and sophistication. It is a great achievement, of historic proportions.

When the modern theories of matter, based on relativistic quantum field theory, the gauge principle, spontaneous symmetry breaking, and asymptotic freedom, were first proposed, they were provisional and hypothetical. Indeed, for some years there were various competing “models” for electroweak interactions, there was considerable skepticism that straight local quantum field theory is adequate to describe the strong interaction, and the experimental evidence for both theories was meager. In those circumstances, it was appropriate to speak of a “Standard Model” of particle physics. It seems to me however that by now this name no longer does justice to what has been achieved. Far more grandiose names have been used for far less substantial achievements. So I propose to call the “Standard Model” what we now know it to be – the Theory of Matter. More precisely, I propose ‘Theory of Matter’ to refer to the core concepts (quantum field theory, gauge symmetry, spontaneous symmetry breaking, asymptotic freedom) and the assignments of the lightest quarks and leptons. These concepts provide us with an extraordinarily powerful, economical description of matter. It will never erode, whatever is eventually discovered “Beyond the Standard Model”. We can keep ‘Standard Model’ to describe the (time-dependent!) minimalist position on more negotiable bits, such as the number of Higgs doublets.

\*In practice, of course, we can only deduce quantitative consequences directly from the fundamental laws in special, simple cases.
1.1. Pre- and Post-LEP

The Theory of Matter has two distinct, but smoothly meshing components: the $SU(2) \times U(1)$ electroweak gauge theory, and the $SU(3)$ color gauge theory, QCD. When LEP began operation, the frameworks for each of these theories were in place, but neither had been nailed together very tightly, and they could not support much weight. Now it is very different.

Figure 1: Data on fundamental electroweak parameters in 1990. There was broad consistency with minimal $SU(2) \times U(1)$, but little sensitivity to radiative corrections.

Let’s compare, for example, the state of the determination of electroweak parameters in 1990 and today. Figure 1, although it represents only a small selection of the results, conveys their character. In 1990, there was essentially no sensitivity to weak-scale radiative corrections, and in particular no very meaningful constraints on the properties of not-yet-observed components of the Standard Model ($t$ quark, Higgs boson) from their virtual effects.

Now, largely thanks to work at LEP, we have exquisite results for many independent observables. To visualize the difference, note that Figure 2, displaying the 2000 data, is a blow-up of the tiny central ellipse in Figure 1. The newer results provide stringent consistency tests for the Theory of Matter. For example, the agreement between experiment and theory is adequate only after nonlinear gauge boson interactions and multi-loop virtual gluon exchange are properly included. The results are also accurate enough to resolve radiative corrections due to heavy particles. Among other things, this work indicated the remarkable heaviness of the $t$ quark before that particle was observed.
Figure 2: Data on fundamental electroweak parameters in 2000. Careful inclusion of the radiative corrections, including loops containing both $W$ and $Z$ bosons and the color gluons of QCD, is necessary to do justice to the data. One can discriminate the effects of the top quark mass and the Higgs boson mass.

Looking to the future, these results provide powerful guidance in searching for the Higgs particle, in assessing the plausibility of technicolor or large-extra-dimension scenarios as compared with low-energy supersymmetry, and in formulating ideas for unified field theories, as I shall discuss below.

Turning to QCD, the classic “picture worth a thousand words” is Figure 3. It shows the running of the strong coupling. The LEP points mostly appear at the high-energy end. Of course, summarizing the hundreds of QCD tests done at LEP in a couple of points hardly does them justice. There is a very rich and extensive story here, with chapters including direct tests of flavor-universality, studies of color coherence, and beautiful work, leading to another determination of $\alpha_s$, on $\tau$ decays.

But time is limited, so I will mention just one especially remarkable aspect of the experimental tests of QCD. As you can see from Figure 3, the QCD prediction for the running of couplings has a focussing property: a fairly wide range of values at low energy scales, or equivalently of the scale parameter $\Lambda_{\text{QCD}}$, implies accurately the same value for the strong coupling $\alpha_s(M_W)$ governing LEP results. Also, of course, the precise values of quark mass parameters become irrelevant at these high energies. Thus the QCD predictions for LEP represent essentially zero parameter predictions for a host of measurable quantities, such as relative frequency of two-, three- and four-jet events, angular and energy distributions, and the variation of all these with energy. There’s no wiggle room. If the Theory is right, all these
The deepest-going results are sometimes those which become so embedded in our world-view that we take them for granted. Along this line, the Theory of Matter has made it credible, in a way that as recently as 30 years ago it was not, that we can succeed in understanding, quantitatively and in detail, the “what” as well as the “how” of Nature. No amount of faith or philosophy is as convincing as a few dozen successful two-loop calculations!

2. Completion? (The Higgs Particle)

The Higgs particle is the only ingredient of the Standard Model that has not yet been observed directly. From the study of radiative corrections to electroweak parameters, as indicated in Figure 2, one can infer limits on the Higgs particle mass. These limits assume, of course, that no additional unknown particles are contributing. They are displayed in a more expansive format in Figure 4.

It is quite impressive and significant how well the mass is boxed in. This makes the challenge facing the Fermilab Tevatron very tangible and concrete. That challenge is made graphically evident in Figure 4. I’d like to advertise the interesting, though controversial, possibility of looking for the Higgs signal in $bb$ events with rapidity gaps, which could further improve the prospects. More theoretical work is needed here.
Frank Wilczek

Figure 4: Experimental constraints on the mass of the Higgs boson, derived from the study of radiative corrections, interpreted in the framework of the minimal Standard Model.

2.1. Some Sense of Proportion

There is no doubt that discovery of the Higgs particle would be a wonderful event. However I do think it is important to keep a realistic sense of proportion, and to be clear just what would be wonderful about it, and what it would mean.

First of all, what is important is not so much the existence of a new highly unstable particle – there are lots of such particles, after all – but the idea it embodies: the idea of spontaneous symmetry breaking. An analogy: there is a useful and intellectually rich theory of spontaneous chiral symmetry breaking in QCD, but the analogue of the ‘Higgs’ – namely, the $\sigma$ meson – is hardly crucial to that theory; indeed, its nature and even its existence is still debated.

From a conceptual perspective, the essential thing is not so much the Higgs particle, but the doublet of which it is a member, and the dynamics it implements. And out of that (complex) doublet, three out of four components have already been discovered, and studied in great detail, doing their dynamical duty! I mean, of course, the longitudinal components of the $W^\pm$ and $Z$.

Second, the Higgs particle (or the doublet) is certainly not – despite much loose talk to the contrary – the Origin of Mass. (Still less is it the God Particle, whatever that means.) Most of the mass of ordinary matter is concentrated in protons...
and neutrons. It arises from an entirely different, and I think more profound and beautiful, source. Numerical simulation of QCD shows that if we built protons and neutrons in an imaginary world with no Higgs mechanism—purely out of quarks and gluons with zero mass—their masses would not be very different from what they actually are. Their mass mostly arises from pure energy, associated with the dynamics of confinement in QCD, according to relation $m = E/c^2$. This profound account of the origin of mass is a crown jewel in our Theory of Matter.

Third, the Higgs particle is most unlikely to be an isolated phenomenon. More likely, it is the tip of an iceberg. As I’ll document shortly, there are good reasons to believe that there are at least two complex doublets scalar “Higgs” doublets—and thus at least five real particles, plus the three appearing as longitudinal vector mesons.

So to me, the most exciting aspect of the discovery of a Higgs particle (or particles) will be that the value of its mass will provide concrete guidance regarding extension of the Theory of Matter. For example, my favorite extension—minimal supersymmetry, effectively broken at nearly electroweak energies, but with a clear separation of scales—predicts a relatively light Higgs particle. The upper bounds extend to 130 Gev or so, but they are not easy to saturate, and I’d be happier with 115 Gev. On the other hand, if we had the minimal Standard Model, a Higgs particle of this mass would be cause for concern, since it would indicate that we live in a metastable vacuum!

3. Structure?

Because the Theory of Matter is so successful, we should hold it to high standards,
and take its shortcomings very seriously. Perhaps the most profound of these shortcomings, because they relate so closely to the core concepts, leap out from Figure 6.

\[
\begin{pmatrix}
  u & u & u \\
  d & d & d \\
\end{pmatrix}_{1/6} \quad \begin{pmatrix}
  u^c & u^c & u^c \\
\end{pmatrix}_{-2/3} \quad \begin{pmatrix}
  d^c & d^c & d^c \\
\end{pmatrix}_{1/3} \\
\begin{pmatrix}
  \nu \\
  e \\
\end{pmatrix}_{-1/2} \quad e^c_i
\]

|   | R | W | B | G | P |
|---|---|---|---|---|---|
| u | + | - | - | + | - |
| u | - | + | - | + | - |
| u | - | - | + | + | - |
| d | + | - | - | - | + |
| d | - | + | - | - | + |
| u^c | - | + | + | - | - |
| u^c | + | + | + | - | - |
| u^c | + | + | + | + | + |
| d^c | - | + | + | + | + |
| d^c | + | + | - | + | + |
| d^c | + | + | - | - | - |
| ν | + | + | + | + | + |
| e | + | + | + | + | + |
| e^c | - | - | - | + | + |
| N | - | - | - | - | - |

Figure 6: Top part: the organization of fermions in the lightest family, based on $SU(3) \times SU(2) \times U(1)$. Bottom part: Organization of the fermions in the lightest family, based on the spinor 16 representation of $SO(10)$.

In upper part of this figure I have displayed the transformation properties of the lightest quarks and leptons under the gauge groups $SU(3) \times SU(2) \times U(1)$. Left-handed fields are used exclusively, so we employ charge conjugation $u^c$ to get the right-handed $u$ quark into the game, through its (left-handed) conjugate. $SU(3)$ acts horizontally, $SU(2)$ acts vertically, and the hypercharge $U(1)$ assignments are indicated by subscripts.

There are two evident shortcomings to this structure. First, the particles fall into five disconnected pieces. Second, there is no evident rhyme or reason to the hypercharge assignments. They are simply chosen to fit experiment.

Along the same lines, the gauge symmetry falls apart into three independent pieces.

All these shortcomings can be overcome, in a way I find quite pretty and compelling, by building upon the core concepts of the Theory of Matter itself.
3.1. Unification of Multiplets

Escalating the concept of spontaneous symmetry breaking, it is natural to ask whether the $SU(3) \times SU(2) \times U(1)$ of the Theory of Matter, which breaks to $SU(3) \times U(1)$, might itself arise from breaking of a larger symmetry.

As is by now well-known, the $SU(3) \times SU(2) \times U(1)$, and the fermions fit snugly into an $SU(5)$. Using a simple breaking scheme (condensate in the adjoint $24$ representation), and starting with fermions in the antisymmetric tensor $10$ and vector $5$ representations, we arrive at precisely the gauge groups and fermion multiplets of the Theory of Matter, including the hypercharge assignments. This is a highly non-trivial coincidence. Since it cuts the number of multiplets down from five to two, and uniquely fixing the hypercharge assignments, this unification achieves substantial esthetic gains over its starting point.

Still more beautiful is the possibility of unification afforded by the slightly larger group $SO(10)$. Now the fermions all fit into a single spinor $16$ representation. This is a particularly elegant representation, with remarkable properties, as indicated in the bottom part of Figure 6. The components of the spinor representation can be specified by their transformation properties under the diagonal $SO(2) \times SO(2) \times SO(2) \times SO(2) \times SO(2)$. These have the physical interpretation of values of five color charges. All possible combinations of charges $\pm \frac{1}{2}$ are allowed, subject to the constraint that the number of $+\frac{1}{2}$ charges is even. From these abstract mathematical rules, the gauge multiplets of the Theory of Matter arise, with the pattern observed in Nature. In particular, the hypercharges are uniquely predicted from the strong and weak charges, according to the simple formula

$$Y = -\frac{1}{6}(R + W + B) + \frac{1}{4}(G + P).$$

The spinor $16$ contains, in addition to the fermions of the Theory of Matter, an additional particle $N$. Since $N$ is a singlet under $SU(3) \times SU(2) \times U(1)$, it has none of the standard interactions with matter, so its “non-discovery” does not pose immediate problems. Indeed it plays a major constructive role in the theory of neutrino masses, as I shall discuss a little later.

3.2. Unification of Couplings

Unified gauge symmetry requires universal gauge coupling strength. This does not hold, of course, in the Theory of Matter. The $SU(3)$ coupling is observed to be larger than the $SU(2)$ coupling, which in turn is larger than the $U(1)$ coupling.

Fortunately, as we have seen in Figure 3, a great lesson of the Theory of Matter is that coupling constants evolve with energy. The same sorts of calculations that give us asymptotic freedom in the strong interaction allow us to evolve, theoretically, the effective couplings up to large energy, or equivalently short distance, scales. If the Theory of Matter derives from a larger gauge symmetry, spontaneously broken at a unique large energy scale, we should expect that these couplings meet at a point. Indeed, in running from high to low energies, the couplings only started to diverge
once the big symmetry was broken.

Figure 7: Near-unification of couplings, based on extrapolating the running of couplings in the minimal Standard Model.

If we evolve the couplings up to high energy using only the particles of the Standard Model, we get the result shown in Figure 7. Notice that to a good approximation the inverse couplings are predicted to run logarithmically, so the running generates straight lines in this log plot. The width of the lines indicates the experimental uncertainties, post LEP. It is a remarkable near-miss; but a miss nonetheless.

One might, and many still do, try to repair this small discrepancy in any number of ways, with slight perturbations on the Standard Model. In the absence of any powerful guiding principle, however, such fixes lack conviction.

Much more compelling, I think, is to start with a deep idea, and to discover that it unexpectedly solves a problem it wasn’t originally specifically built for. Rather than tweaking the Standard Model, let us consider the apparently drastic, but independently motivated, idea that supersymmetry is broken only at relatively low ($\lesssim$ Tev) energies. This modifies the running of the couplings, in a way that is easy to compute, because there are more virtual particles to consider. If we extend the Standard Model in the most economical way to include low-energy supersymmetry, we find the result shown in Figure 8. The unification now works extraordinarily well. This is a greatly encouraging result, both for unification and for low-energy supersymmetry.

It was very surprising, at first, to discover that such a drastic modification of the Standard Model caused only small changes in the predicted relation among low-energy couplings. After a bit of thought, however, it’s not hard to see why that
relation is robust against certain classes of perturbations. Basically, any addition of particles forming complete $SU(5)$ multiplets, modestly split, will cause only small changes. Indeed, the major reason that the minimal supersymmetric result differs from the Standard Model result is that low-energy supersymmetry requires two Higgs doublets which do not have accompanying triplets, together of course with their fermionic superpartners. (The triplets have very exotic quantum numbers and potentially mediate rapid proton decay. They must be super-heavy.)

3.3. **Significance**

The quantitative success of the unification of couplings calculation (with low-energy supersymmetry) is undeniable. What is its significance?

At the most formal level, the unification of couplings is an over-constrained fit of three measured quantities – $\alpha_1(M_W), \alpha_2(M_W), \alpha_3(M_W)$ – to two theoretical parameters, the scale of unification and the strength of coupling at unification. Given the precision of the measurements, it is remarkable that a fit can be obtained.

But simply saying that one number falls into place does not do justice to the state of affairs. For there are many other things that could have gone wrong, besides failure to find a good numerical fit. If the couplings had met at too small an energy scale, we would have difficulties with rapid proton decay. If they had met at a significantly larger a mass scale, at or above the Planck scale, we would have had to
worry about quantum gravity corrections. The actual scale at which they meet, not far on a logarithmic scale, but still significantly, below the Planck scale, is uniquely acceptable. Similarly, if the unified coupling were much larger we could not trust the perturbative calculation.

I have heard it said, in reference to Figures 7 and 8, that “two straight lines will always meet in a point, and it’s not so remarkable that three happen to”. This attitude, I believe, is profoundly wrong-headed. Some of my reasons are those given in the previous paragraph. Another is that the “straight line” nature of the running is in itself a profound result, reflecting the nature of vacuum polarization in quantum field theory. It appears semi-trivial only because of the way it is plotted (inverse couplings on a log scale). Perhaps Figure 9 is more impressive!

Figure 9: The same as Figure 8, using different variables.

To my perception, the unification of multiplets and the unification of couplings are the crown jewels of physics beyond the Standard Model. Together, they make a powerful prima facie case for the elements that went into their derivation: unified gauge symmetry, for the unification of multiplets; renormalizable quantum field theory, operating smoothly up to near-Planckian scales, for the proper logarithmic running of couplings; and low-energy supersymmetry, for detailed numerical success.

Nowadays, in the context of string theory, we know – or, rather, we have incomplete suggestions about – many alternative ways that the low-energy $SU(3) \times SU(2) \times U(1)$ symmetry of the Theory of Matter might emerge, from constructions that involve neither effective unified gauge field theories nor symmetry breaking through condensates. Of course, there is no necessary contradiction, since early reduction to an effective unified gauge field theory also still remains a viable option. Along this line, perhaps we should take the striking apparent successes of
the “good old” ideas I just reviewed (and there are more to come!) as indications that in searching for string-based models of Nature, we should look to those that reduce to something like an effective supersymmetric $SO(10)$ renormalizable gauge field theory just below the Planck scale. Certainly, any other scheme has some coincidences to explain.

4. Seven Pillars of Unification Wisdom

I’ve just now discussed the first two of these:

4.1. Multiplet Unification

4.2. Coupling Unification

These unifications are motivated by the structure of the Theory of Matter. They are firmly based on extrapolating the deep core concepts of that Theory (quantum field theory, gauge symmetry, spontaneous symmetry breaking, asymptotic freedom) to new energy scales. They lead us into a framework including unified gauge symmetry, renormalizable field theories effective up to near-Planckian scales, and low-energy supersymmetry.

This framework is usefully specific, and has several other desirable consequences, that I’ll summarize briefly now.

4.3. Neutrino Mass Scale

The oscillation of atmospheric neutrinos discovered by the SuperK collaboration can be interpreted as evidence for a mass $m_{\nu_{\tau}}$ of the tau neutrino $\nu_{\tau}$ of order $10^{-2}$ eV. Within the framework of electroweak $SU(2) \times U(1)$, this can be accommodated by means of a non-renormalizable interaction

$$\Delta \mathcal{L} = \frac{1}{M} \phi^\dagger l \phi l ,$$

where $l$ is the lepton doublet and $\phi$ the Higgs doublet. With $\phi$ replaced by its vacuum expectation value $v$, this becomes a Majorana neutrino mass of magnitude $v^2 / M$. With $M$ of order $10^{15} - 10^{16}$ Gev, this is about right. That mass scale is equal to that which appears in the unification of couplings, as the scale at which unification symmetry breaks.

There is a simple, concrete dynamical mechanism for generating neutrino masses that explains this coincidence. It involves the $N$ particle we met before as the missing component of the $SO(10)$ spinor 16. Since it is an $SU(3) \times SU(2) \times U(1)$ singlet, this particle can acquire a large mass $\sim M_U$ at the scale where $SO(10)$ symmetry is broken, without breaking those low-energy symmetries. It also can connect to the conventional left-handed neutrino $\nu$ by a normal Higgs-type mass term acquiring a mass $m$. By second-order perturbation theory, passing through the intermediate $N$, we generate a Majorana mass of order $m^2 / M_U$ for $\nu$. Finally, we expect $m \sim v$ for the heaviest neutrino, since this mass is related by symmetry to the large top quark mass (see below).
There are significant uncertainties in both steps of the argument, so this calculation of the scale of neutrino masses is semi-quantitative at best. Still, it is very impressive how the outlandishly small value of the neutrino mass, relative to other quark and charged lepton masses, gets mapped to the outlandishly large value of the unification scale, and how the existence of $N$, at first sight an embarrassment, turns out to be a blessing.

4.4. Basic Features of Electroweak Symmetry Breaking

The point of departure for many ideas about physics beyond the Standard Model is dissatisfaction with the minimal Standard Model account of electroweak symmetry breaking.

In the minimal model electroweak symmetry breaking is, of course, implemented by minimization of a simple potential for the Higgs doublet. At the classical level, and if we confine our gaze to the electroweak sector alone, this would seem to be unobjectionable, and indeed very much in line with how Occam might suggest that we parametrize our ignorance of the symmetry breaking dynamics. But if we consider the quantum version of the model, we find ourselves in the somewhat distasteful situation of having quadratically divergent radiative corrections to the Higgs doublet mass parameter.

That is not quite a contradiction, but it does beg the question of what provides the cutoff. The Standard Model by itself is not a well-behaved quantum field theory. It is not asymptotically free, and therefore most likely it does not exist, nonperturbatively. (Perturbation theory, which alone makes the renormalization program plausible, goes bad in the ultraviolet.) One might reasonably expect that whatever additional physics we must add to the Standard Model to make it a good theory is characterized by some much larger mass scale, simply because we’ve seen no direct sign of that physics. And then we have to understand why this larger mass scale does not infect the Higgs doublet mass parameter, through radiative corrections.

The difficulty is exacerbated considerably if we take the unification of couplings calculation seriously (as, of course, we should). For this indicates a unification scale of order $10^{16}$ Gev – a whopping factor $10^{28}$ larger than the electroweak scale, using the appropriate quadratic measure. Any corrections to the Higgs doublet mass arising from this sector must by highly suppressed compared to naive dimensional analysis. There are generally both classical and quantum corrections.

Low-energy supersymmetry cleanly suppresses the quantum corrections, by canceling off contributions between ‘nearly degenerate’ ($\Delta M \lesssim$ Tev) virtual bosons and fermions. Requiring adequate suppression gives a condition of the rough form

$$\frac{\alpha}{\pi} \Delta M^2 \lesssim v^2,$$

with $v$ as before. This is the foundational argument, independent of the unification of couplings calculation, for low-energy supersymmetry. It is possible, and important, to be more precise about this naturality condition, as we’ll see shortly.
The suppression of classical corrections is a different, and much murkier, question. In fact it raises several issues: doublet-triplet splitting, the so-called $\mu$ problem, the magnitude of soft supersymmetry breaking terms, and perhaps others. No simple or uniquely compelling answers are available at present, so I’ll say no more here.

This simple but powerful argument for low-energy supersymmetry could be, and was, made before LEP. During the LEP era, additional supporting evidence has emerged.

The first, and most profound, piece of evidence may seem a little paradoxical: it is how well the Standard Model has stood up to detailed quantitative scrutiny. To put this in perspective, we should contrast the approach of low-energy supersymmetry with other attempts to address the problem of stabilizing the weak scale.

Instead of invoking cancellations to keep radiative corrections to the Higgs mass small, one might imagine that there are form-factors. For this, the Higgs doublet must be composite on the weak scale, with some strong-coupling dynamics to bind it. That is the central idea of ‘technicolor’ theories. In such theories, since there is no small coupling nor super-large mass suppressing the new strong-coupling dynamics, there is no reason to expect radiative corrections in general to be small. One would expect, generically, relatively large deviations from Standard Model predictions at the one-loop level. The situation deteriorates further if one tries to account for flavor physics along these lines, since there are severe empirical bounds on the expected neutral flavor-changing interactions.

It is still worse if one tries to put unification or string physics at the weak scale, since this would appear to bring in proton decay too.

Perhaps some clever pastiche of tricks allows Nature to circumvent these pitfalls? But I prefer to think that Figure 10 is pointing the way. It shows how minimal implementations of low-energy supersymmetry are easily consistent with precision electroweak data. Weak coupling and good ultraviolet behavior make this self-effacement possible; the facts make it make mandatory.

A second piece of evidence is also visible in Figure 10. Whereas without supersymmetry the Higgs particle mass is essentially unconstrained, supersymmetric models relate it to directly to the $Z$ mass, and after careful calculation one finds masses below $m_H \lesssim 130$ GeV in generic models. This fits quite well with the indirect observations, summarized in Figure 4 – and still better with the possible discovery at $m_H \approx 115$ GeV.

Finally, the observed large value of the top quark mass supports an elegant mechanism for electroweak symmetry breaking, as shown in Figure 11, through running of the effective Higgs mass. This nicely fills the requirements of minimal supergravity models, with soft supersymmetry breaking terms.

I am quite skeptical of this. In particular, the idea that proton decay can be suppressed by putting quarks and leptons on different walls seems quite dubious to me. Since the Standard Model itself supports proton decay, albeit at imperceptible rates, through weak instantons, there cannot be a universal suppression mechanism consistent with obtaining the Standard Model as a low-energy limit.
4.5. Dark Matter Candidates

If baryon and lepton number are accurate symmetries, then so is R-parity

\[ R = (-1)^{3B+L+2S}, \]

where \( B \) is baryon number, \( L \) is lepton number, and \( S \) is spin. Ordinary particles are even under R-parity; their supersymmetric partners are odd. Therefore the lightest supersymmetric particle is likely to be extremely stable. In many models of low-energy supersymmetry it is a neutral fermion, generically called the neutralino. The neutralino interacts quite weakly with ordinary matter. Much study has been devoted to minimal models of low-energy supersymmetry with universal soft breaking terms. In this framework, one generally finds that the neutralino is a linear combination of bino and Higgsino (partners of the hypercharge gauge boson and the Higgs particle). Given a concrete model, one can calculate the production of neutralinos in the early Universe. For a significant range of parameters the calculated relic abundance of neutralinos, and their feeble interactions with ordinary matter, makes them excellent candidates to supply the missing mass that astronomers need. I will discuss some specific parameters and search strategies in a few moments.

A possible complication is that in plausible extensions of the minimal framework, the lightest supersymmetric particle could have quite a different character. The neutralino (defined as the lightest R-odd member of the Supersymmetric Standard
Figure 11: Running of effective masses down from the unification scale, in a minimal supergravity model. The running is mainly driven by radiative corrections from virtual tops and stops. It drives the Higgs (mass)² parameter negative, inducing electroweak symmetry breaking.

Model) might well decay very slowly on particle physics or laboratory time scales, but rapidly on cosmological time scales, through ultra-weak interactions, into a still lighter R-odd particle. A prime candidate is the axino. I find the idea that the missing mass is dominated by axions, with quasi-stable neutralinos having decayed into axinos, quite entertaining.

4.6. Fermion Coupling Unification

The observed ratio $m_b/m_\tau$ plausibly derives from equality at the unified scale, as required in all unification schemes extending SU(5). The large value of the “Dirac” neutrino mass, fixed to $m_\tau$ through an SO(10) relation, played a role in our earlier discussion of neutrino masses.

For the lighter quarks the pattern is murkier, as might be expected, since their masses and mixings are buffeted by subleading effects in the mass matrices. However there are some significantly successful attempts to go further, by exploiting group-theoretic constraints among matrix elements that arise if one assumes minimalistic gauge symmetry breaking (condensates in small representations) and simple coupling patterns.

4.7. Emergence of the Planck Scale

We can attempt to extend the calculation of Figure further, to include gravity. Whereas the couplings of the Theory of Matter are dimensionless, and run
with energy only logarithmically, due to vacuum polarization effects, the gravitational coupling has dimension \((1/\text{mass})^2\). Therefore it grows in importance with energy, even classically – and much faster. A simple-minded estimate, using dimensional analysis, indicates that the effective gravitational coupling becomes strong at \(Q \sim 10^{18} \text{ GeV}\), the Planck mass. The other couplings unify at \(Q \sim 10^{16} \text{ GeV}\), and plausibly become strong at a slightly higher energy. Thus the unification of couplings calculation, naively extended to include gravity, is not far off. This is quite a remarkable result, since the physical ingredients entering into the calculation are so disparate. The small residual discrepancy between the Theory of Matter unification scale and the Planck scale has been ascribed to the opening up of an extra spatial dimension near these scales, though of course in the present state of knowledge there are other possibilities.

A dramatic, but I think not unfair, way to state this result is that we have, within this framework, convincingly solved the central “hierarchy problem” of fundamental physics. By that I mean the question of why gravity, acting between life-size lumps of matter, is so feeble. Or, in more technical language, the problem of why the ratio of the Planck mass to the proton mass is so large. In our calculation this ratio is given as the inverse of exponentials of inverses of the observed coupling constants in the Theory of Matter. No spectacularly small (“unnatural”) quantities are involved. The big ratio of mass scales arises basically because the strong coupling \(\alpha_3\) at the unification scale is about 1/25, and the couplings run only logarithmically. Therefore quite a long run is required before one reaches the scale where \(\alpha_3\) approaches unity, protons are assembled, and ordinary life begins.

Of course other major hierarchy problems (doublet-triplet splitting, smallness of the soft supersymmetry breaking, \(\mu\) problem), more recondite but still fundamentally significant, remain open, as I’ve mentioned before.

5. Is It Right?

As I’ve now discussed, I think we’ve been given some excellent clues for figuring out a substantial chunk of physics beyond the Standard Model. They point us in the directions of gauge unification and low-energy supersymmetry. How will we find out whether these ideas or right – or kill them off for good?

5.1. Small Effects Among Known Particles

While low-energy supersymmetry features weak couplings and good ultraviolet behavior, it also introduces a profusion of new particles, with attendant possibilities for introducing new contributions to flavor-changing neutral processes, CP violation, and of course diagonal radiative corrections.

The first class is exemplified by \(K - \bar{K}\) mixing and \(B \rightarrow s\gamma\), to mention two processes that are particularly sensitive and have received a lot of attention. We should also include \(\mu \rightarrow e\gamma\) and allied processes in this class. The second class is exemplified primarily by electric dipole moments of neutrons or electrons. The third
class is exemplified primarily by corrections the muon anomalous magnetic moment \( g_\mu - 2 \). The modern experimental limits on deviations from the Standard Model in each of these processes puts very significant pressure on the supersymmetric parameter space already. It is very important to continue improving these limits.

It may be useful to mention that quantitative interpretation of many experiments in this field is limited by the accuracy with which we can calculate even rather simple strong matrix elements. The technique of lattice gauge theory, and the available computing power, have markedly improved recently. Given the appropriate investments, there could be considerable progress on this front before long.

5.2. Proton Decay

The Standard Model has the beautiful feature that all baryon- and lepton-number violating processes require non-renormalizable interactions. Such interactions are characterized by coupling constants whose dimensions are inverse powers of masses. If the masses involved are extremely large, we can have a simple universal explanation of the smallness or rarity of such processes. Of course, the unification of couplings calculation does suggest that the relevant mass scale is extremely large.

As was realized very early in the modern history of gauge unification, two processes above all are exquisitely sensitive to highly suppressed interactions. These are neutrino oscillations and proton decay. Neutrino oscillations have now been observed, with roughly the predicted oscillation length (neutrino mass), as I’ve already discussed. We’re waiting for the other shoe to drop.

The minimal implementation of gauge unification, without supersymmetry, has a severe problem with modern experimental limits on proton decay.

Extension of the Standard Model to incorporate low-energy supersymmetry changes the situation considerably. The scale of unification goes up a bit, which removes the outright contradiction between the rate of proton decay through gauge particle exchange and experiment that we had without supersymmetry. On the other hand, dangerous new sources of proton decay arise, through exchange of the Higgsinos associated with unified symmetry breaking. Quantitative analysis is complex and fraught with uncertainties, but it will not be easy to reconcile limits \( T_{\text{proton}} \gtrsim 10^{34} \text{ yrs.} \) with straightforward models. A striking prediction characteristic of supersymmetric unified theories is that modes involving strange final states, particularly \( p \to K^+\bar{\nu} \) and \( n \to K^0\bar{\nu} \), will dominate.

5.3. Focus Point

An interesting recent development, which seems capable of easing the quantitative pressure on low-energy supersymmetry from all these sources, is the “focus point” scenario of Feng, Matchev, and Moroi. The central phenomenon is shown in Figure 12. One finds that the predicted value of the weak scale is remarkably insensitive to the assumed value of the soft supersymmetry breaking parameter \( m_0 \). Consequently, that parameter can be taken much larger than one might naively
Figure 12: The scale of electroweak symmetry breaking is surprisingly insensitive to the value of the scalar mass parameter $m_0$. This is the “focus point” phenomenon. It makes large values of the physical squark and slepton masses more plausible.

The physical consequence is that squark and slepton – but not gaugino – masses can be significantly larger than was previously believed to be natural. Masses of 2 Tev are comfortably allowed. These larger masses systematically suppress all the unobserved possibilities mentioned above.

5.4. Dark Matter Searches

Many of the ideas I have just discussed come together in Figures 13 and 14. There we see: first, that a wide swath of supersymmetric parameter space, including a big contribution from the focus point region, gives rise to a desirable dark matter density; and second, that a wide variety of experiments for dark matter detection, together with direct accelerator searches and foreseeable improvements in $B \to s\gamma$ and $g_{\mu} - 2$, should plausibly give some indication for low-energy supersymmetry even before the LHC.

5.5. Produce the New Particles!

Of course, the ultimate test for low-energy supersymmetry will be to produce some of the predicted new $R$-odd particles. Even in the focus point scenario, there must be several accessible to the LHC.
6. Ultimate Questions

Finally I’d like briefly to discuss a few questions that definitely belong on the agenda of future physics, although they fall somewhat outside the circle of ideas I’ve been developing so far.

6.1. Can We Understand Extreme Conditions?

We know the equations of QCD, but there are several potentially awesome applications which await better solutions of those equations. At present the theory of neutron star interiors, supernovae explosions, and leading models of gamma ray bursters are based on crude phenomenological models of the high-temperature and high-density behavior of hadronic matter. It is a great challenge to do better theoretically. Some quite beautiful concepts have emerged already, including the liberation of quarks and gluons in a plasma phase, and the prediction of color superconducting phases with remarkable properties. We can test our mettle on experimental simulation of the Big Bang, in relativistic heavy ion collisions. Eventually, we may hope that neutrino and gravity-wave detectors will give us meaningful access to the most extreme astrophysical processes (a nearby supernova would be very helpful!).

There has been spectacular progress recently in observational cosmology, especially in the determination of cosmic microwave background anisotropies. These observations seem to indicate the spatial flatness of the Universe and an approximately scale-invariant fluctuation spectrum, which broadly supports simple models
Figure 14: The same as Figure 13, but with the parameter $\tan \beta = 50$ (instead of $\tan \beta = 10$).

of inflation. The general idea of inflation, of course, came out of particle physics. Inflation is supposed to be triggered by a phase transition associated with breaking of some fundamental symmetry – perhaps the unification symmetry. But existing models of inflation are only very thinly rooted in specific world-models, and their main parameters have not been related to microphysics. It is a great challenge either to show that inflation really occurs as a consequence of fundamental physics, and to find an identity and a torso for the inflaton and its potential; or else to replace it with something different (this is still not inconceivable, perhaps, since the “evidence” is rather generic). Upcoming experiments mapping out the accurate fluctuation spectrum, and particular polarization measurements capable of separating out the gravity wave spectrum, will be very interesting to watch.

The highest energy cosmic rays, including perhaps a neutrino component, will for the foreseeable future provide us with our highest-energy collisions. It would be wonderful to exploit this resource more fully. Even their origin is still an open problem, and might involve new fundamental physics.

6.2. Are the “Fundamental” Couplings Universal?

According to the basic hypothesis of inflation, the presently observable Universe arose from the rapid expansion of a tiny patch early on. This hypothesis was largely motivated by the “horizon problem”: the observation that observable Universe, as characterized by the large-scale distribution of galaxies and the cosmic microwave background, is accurately homogeneous and isotropic. Inflation guarantees these uniformities, even in the absence of any dynamics enforcing them, by postulating a common origin for the observable patches of the sky. There is no implication that
uniformity characterizes the entire Universe; only the part we presently observe (with an unknown “safety factor” to spare).

Given this context, it is natural to ask whether other of the observed uniformities of Nature are likewise cosmologically conditioned. Specifically, one can wonder whether quantities we ordinarily regard as “constants of Nature”, such as particle masses and mixing angles, are truly universal, or instead have frozen-in values that vary from patch to patch.

This phenomenon arises quite concretely in axion physics. If the Peccei-Quinn transition occurs before inflation, different amplitudes of the axion field will be frozen into different patches. Observers in (very) widely separated portions of the Universe would report different values of a fundamental constant of Nature, namely the QCD $\theta$ parameter. Eventually, as the Universe cooled, the amplitudes would relax, producing different cosmological mass densities of axions in the different patches. Some large portions of the Universe would be axion-dominated, while others will have only a small axion density.

There are many theoretical suggestions for promoting other constants of Nature into dynamical variables. It may be attractive, for example, to suppose that the symmetry one would have among different families in the absence of Higgs couplings is only spontaneously broken. In that case one would have axion-like ‘familons’, with similar possibilities to those in the previous paragraph. In string theory, it is commonly assumed that all the physical constants are fields capable of variation. And many apparently consistent solutions of the static equations have been found, that predict wildly different versions of the laws governing observable (low-energy) physics.

These considerations emphasize the significance of experiments to look for very light, very weakly interacting particles, the quanta of physical constants which are actually dynamical variables. Recently there have been remarkable improvements in the search for new macroscopic forces. We look forward to additional experiments looking for small violations of the equivalence principle and for monopole-dipole forces, in particular.

6.3. **Why is Empty Space (Almost) Weightless?**

The smallness of the cosmological term, compared to other scales of physics, is notorious. The vacuum energy density, as seen by gravity, is not more than $\sim 10^{-12}$ eV$^4$. Recent observations suggest it is not zero. In any case, it is many orders of magnitude below the Planck or unification energy density scales $\sim 10^{108}$ eV$^4$, $\sim 10^{66}$ eV$^4$, the weak energy density scale $\sim 10^{44}$ eV$^4$, or even the QCD spontaneous chiral symmetry breaking scale $\sim 10^{32}$ eV$^4$.

We do not understand the disparity. In my opinion, it is the biggest and worst gap in our current understanding of the physical world.

The problem has both classical and quantum aspects. Classically, it is very difficult to understand why gravity does not notice the presence of various symmetry-breaking condensates. Quantum mechanically, we must also worry about the energy
associated with zero-point oscillations of modes of quantum fields. Cancellations due to supersymmetry could partially address the quantum mechanical aspect, but do not help with the classical aspect; in any case, supersymmetry in Nature is nowhere near accurate enough for the job. Even if we do assume that supersymmetry (or something else) takes care of the quantum zero-point energy from high-energy modes, we are still left with the classical contributions, and the contributions of low-energy modes.

Prior to the recent apparent discovery of a non-zero value for the cosmological term, it was tempting to suppose that some hidden symmetry somehow put it to zero. Perhaps now it seems more likely that a dynamical mechanism is involved. Some of us like the idea that it will involve relaxation through some exotic, very light, very weakly coupled matter – in the spirit of axions relaxing the $\theta$ term – though I freely admit that our detailed realization needs work. In any case, ideas like this reinforce the interest of searches for new macroscopic forces, and will surely suggest other sorts of experiments.

The question of why empty space weighs so little is every bit as fundamental as the question of insuring good ultraviolet behavior of quantum gravity. Furthermore, it is much more sharply posed by Nature. It would be marvelous if string theory, which promises to provide a unique and consistent theory of quantum gravity, could meaningfully engage this question.

7. Future Summary

I believe we are about to experience a new Golden Age in fundamental physics. The physics of electroweak symmetry breaking, low-energy supersymmetry, and unification is ripe. Its fruit will include Higgs particles, superpartners galore, identification of the dark matter, proton decay, and more. The astronomers will chip in with detailed information about the primordial fluctuations, perhaps including a gravitational wave component, and perhaps some surprises from high-energy cosmic rays. As this tide of discoveries rolls in, we will understand the world better in many concrete ways. We will also gather precious information about physics at the unification scale, and about physical events in the earliest moments of the Big Bang.

There may also be a Golden Age in the exploitation of the fundamental physics we have recently achieved. QCD is a young theory, and not an easy one to handle. But continuing advances in computing power, and in fundamental algorithms (notably, in maintaining chiral symmetry while discretizing), have brought us to the point that definite quantitative calculations of a host of quantities are becoming feasible. For example a fully microscopic calculation of the proton-neutron mass difference, so crucial to the structure of the world, would be a milestone achievement, and is well within sight. There is a ferment of ideas in understanding QCD at high temperatures and at high density; it seems realistic to hope that we will produce usable predictions for the structure of neutron stars and for behavior in extreme astrophysical environments.
Less easy to anticipate with confidence, but to me a very real and exciting prospect, is progress on the frontier of ultra-light, ultra-weakly interacting matter, with “firm” connections to the strong CP problem and somewhat less firm connections to the vacuum selection and cosmological term problems.

So I expect that in ten to fifteen years we will know a lot more. Will we know Everything? More likely, I think, is that as we learn many additional facts, we will also come to comprehend more clearly how much we don’t know – and, let us hope, learn an appropriate humility.

Acknowledgments

This work is supported in part by funds provided by the U.S. Department of Energy (D.O.E.) under cooperative research agreement #DF-FC02-94ER40818. I would like to thank Jonathan Feng for helpful comments on the manuscript.

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These references only touch a few very recent or specialized topics. Given the scope of the talk, and the manuscript deadline, this was the only practical solution for me. My congratulations to authors of classic papers too well known to need citation here.

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