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Possible Scales of New Physics[1]

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Abstract

The biggest question in beyond the standard model physics is what are the scales of new physics. Ideas about scales, as well as experimental evidence and constraints, are surveyed for a variety of possible forms of new physics: supersymmetry, neutrino masses, unification, and superstring theory.

[1]Plenary talk on Beyond the Standard Model Physics, DPF99 (UCLA).

1 Introduction

Underlying any discussion of "Beyond the Standard Model Physics" is the question: what are the scales of new physics? For most particle physicists, theorists and experimentalists, the successes of the standard model which we have heard about again at this meeting are impressive, but they are also a source of enormous frustration. We believe that the standard model cannot be complete. A complete theory, we presume, would not have so many free parameters; it would incorporate gravity in a consistent way; it would not be governed by mysterious fine tunings. To understand these questions, we have explored grand unification, supersymmetry, and string theory. We have contemplated neutrino masses and compositeness. As we have heard so beautifully described at this meeting, we now have good evidence for neutrino masses. This certainly means that there is something beyond the standard model. What type of new physics is responsible for these masses – and what are the associated scales – are by no means clear. We have also heard a great deal of discussion of supersymmetry. The evidence, here, is tenuous at best, but only in the case of supersymmetry breaking do we have any real argument for what the relevant scale should be. And even this argument, while oft-repeated, is open to question. In string theory, the conventional wisdom has long been that the fundamental scale of the theory is close to the Planck scale. But this has been called into question in recent years, as has been the significance of the fundamental scale itself. In this talk, I will not answer the question of what these various scales may be, but I will try to phrase the issues as sharply as possible, and discuss some of the suggestions which have been offered.

Let me turn first to supersymmetry. Supersymmetry is a beautiful, hypothetical symmetry of nature. It has been the subject of intense theoretical discussion and serious experimental investigation. What experimental evidence there is lies in the fact that the gauge couplings unify at a high energy if one assumes supersymmetry breaking at about 1 TeV, and not otherwise. This is impressive, but hardly compelling by itself. The real arguments that nature is supersymmetric, and that supersymmetry is broken at scales not too different than the weak scale, are theoretical. The standard model suffers from a difficulty often referred to as the problem of quadratic divergences or the hierarchy problem. More simply, it is a collosal failure of dimensional analysis. In the standard model, the masses of all of the elementary particles are tied to the mass of the Higgs particle. Dimensional analysis suggests that this mass should be of order the largest scale we know in nature, the Planck scale, and this is manifestly false. On the other hand, a similar argument would suggest that the electron mass should be of order the W boson mass. While we don't really understand why the electron mass is small, we do know that the standard model is more symmetric if the electron mass is zero, so it is *natural* that the electron mass is light. More generally, we believe that the smallness of all of the quark and lepton masses (except t) is due to approximate flavor symmetries. On the other hand, the standard model *does not* become more symmetric if the Higgs mass is small. Indeed, if one tries to write a theory with a small value of the Higgs mass, it will generally receive large radiative corrections. If the Higgs particle is to be light as a result of symmetries, it is necessary to enlarge the standard model, so that it is supersymmetric. Obviously, if the laws of nature are supersymmetric, supersymmetry must be a broken symmetry. If the scale of supersymmetry breaking is not much different than the scale of weak interactions, than the Higgs mass is naturally of this order. The only other persuasive explanation which has been offered for the lightness of the Higgs particle is technicolor, the possibility that electroweak symmetry breakdown is due to some new strong interactions among a new set of fermions know as techniquarks. Technicolor has fallen into some disfavor in recent years, and was not discussed at any length at this conference. Technicolor theories generically have problems with precision electroweak tests. It is difficult to construct models which are compatible with constraints from rare processes. As a result, there is no "standard model" of technicolor, and one can only guess at what the generic predictions are. Still, many of us wonder, especially as LEP and the Tevatron put stronger and stronger limits on supersymmetric theories, whether some sort of dynamical symmetry breakdown might be the origin of electroweak symmetry breaking.

Some progress has been made in recent years on a slightly different form of dynamical symmetry breaking than conventional technicolor, known as "top color." These models also may require some exotic new physics. It is interesting that at this meeting we heard a version of this idea which involves large compact dimensions to generate the requisite four fermi operators[1].

2 Supersymmetry

Their are a number of reasons why so many theorists seem convinced that supersymmetry will be discovered in the not too distant future.

- Supersymmetry explains the failure of dimensional analysis to account for the value of the Higgs mass.
- Low energy supersymmetry yields unification of couplings, without ad hoc assumptions.
- Supersymmetry naturally provides candidates for the dark matter, without fiddling with

parameters.

- Supersymmetry fits elegantly into string theory. If string (M-) theory is a unique, ultimate theory of gravity and gauge interactions, it follows that supersymmetry is an essential part of this final theory. Low energy supersymmetry emerges naturally from this story.
- Unlike technicolor, complete models exist, whose phenomenology can be investigated in great detail.
- Finally, supersymmetry is a beautiful possible symmetry of nature; it would be disappointing if nature didn't exploit it.

Only the first three arguments suggest that supersymmetry should be broken at low energies. The last argument, even if persuasive, does not say that the scale of supersymmetry breaking should be such that we could hope to observe this symmetry. Clearly some of these arguments are more compelling than others. The fact that we can build models easily is hardly, in itself, an argument but it certainly accounts for some of the subject's appeal. Indeed, all one needs to do is specify certain "soft breaking parameters" (the masses of the superpartners of ordinary fields, as well as certain trilinear scalar couplings), the particle content (e.g. the MSSM), and certain discrete symmetries, to completely specify the phenomenology.

Assuming that nature is supersymmetric at low energies, the most crucial question in the subject will be: what is the origin of supersymmetry breaking. In the MSSM, for example, there are 105 parameters beyond those of the minimal standard model. These include the masses and mixings of the squarks, sleptons and gauginos (including CP violating phases), certain trilinear scalar couplings, and the parameters which determine the Higgs potential. Without a *theory* of supersymmetry breaking, these quantities are arbitrary (similar to the CKM parameters). However, there are significant constraints from experiment.

- Flavor-changing neutral currents: these require some degree of squark and slepton degeneracy, or alignment of these mass matrices and the quark and lepton matrices.
- Limits from direct searches (more shortly)
- Hierarchy: the scale of supersymmetry breaking should not be too large (presumably not much greater than a TeV, and perhaps even less?). Experiments at LEP and the Tevatron are already squeezing the allowed parameter space.

There are a number of traditional approaches to supersymmetry breaking. The most popular has been "Supergravity" (SUGRA) breaking. In this approach, one assumes that the squarks, sleptons and gauginos are nearly degenerate at the high scale. This has the virtue of simultaneously solving the flavor problem and of reducing the number of parameters. I put "supergravity" in quotes, however, because nothing about supergravity (gauged supersymmetry) by itself enforces this structure. Despite frequent statements to the contrary, there is no sense in which the universality of gravitational couplings insures equality of squark and slepton masses. In string theory (the only theory we have which is locally supersymmetric and at the same time where one can do calculations) this degeneracy does hold approximately in some circumstances[2], but it is difficult to understand why the true vacuum of the theory should have this property. The only other suggestion to solve this problem of universality has been to postulate flavor symmetries[3]. Finally, in the traditional SUGRA approach, no attempt is made to understand the origin of supersymmetry breaking; the scale of this breaking is simply put in by hand.

More recently, a variety of approaches to supersymmetry breaking have been developed which attempt to explain the large hierarchy through supersymmetry-breaking dynamics. Indeed, we know a great deal about SUSY dynamics, and can often show that SUSY is dynamically broken. This can provide an explanation of the hierarchy, since typically

$$M_{susy} \sim M_p e^{-a \frac{2\pi}{\alpha_{gut}}}. \tag{1}$$

There are at least three proposals based on this possibility:

- Gauge Mediation: In these theories, supersymmetry is broken by some new, strong dynamics. The breaking is communicated to the superpartners of ordinary fields by gauge interactions. Masses of squarks and sleptons are then just functions of their gauge quantum numbers, leading to adequate suppression of flavor changing processes, and, more importantly, to predictions. Another appealing feature of this mechanism is that models exist. Still, the scale of the breaking dynamics is not highly constrained. The nicest existing models have large scales[6]. The famed CDF $\gamma\gamma e\, eE_{miss}$ event, on the other hand, suggests a low scale, if this is interpreted as production of a selectron pair (or something similar)[4, 5]. While a low scale is quite plausible in this picture, there is only one explicit model, to my knowledge, with a low enough scale to explain the CDF event[7].

- String theory: here, one often speaks of gluino condensation as a mechanism for supersymmetry breaking[8]. No complete theory of this kind exists, however. The general effect of gaugino condensation is to give a potential for some of the fields, but in regions where one can calculate, one does not find a stable minimum. If there are stable minima at strong coupling, it is difficult to explore them, and to understand how problems such as flavor changing processes are solved.[1] Recently, another suggestion has been put forward, which the authors refer to as "sequestered" supersymmetry breaking. Here, the idea is that the fields of the standard model live on a brane (an object which one can think of as analogous to a domain wall), while the fields responsible for supersymmetry breaking live on a second brane, well separated from "ours." Under certain circumstances, this idea can lead to a spectrum similar to that for gauge mediation. The crucial new element here is a recently appreciated anomaly, which gives rise to large than expected gaugino masses[10, 11]. While complete models of this kind do not yet exist, they are the subject of intense investigation.

- Compositeness: Various authors have used ideas connected with Seiberg duality to construct models with *composite* quarks and leptons. Typically, the first two generations are composite, and the squarks quite massive (greater than a TeV or so). Gauginos and the third generation squarks are lighter[12, 13]. Flavor changing neutral currents are suppressed. These models may well be less fine tuned than gauge mediated models, and are certainly worth further study and development.

It is probably fair to say that no completely compelling model of supersymmetry breaking yet exists. We should be striving to develop a real Supersymmetric Standard Model, including breaking. For now, we have various approaches to model building, each with advantages and drawbacks.

Experiment, meanwhile, is narrowing the allowed parameter space. Indeed, analyses at all of the major experiments reflect appreciation for the many possible patterns of soft breaking (its not just your old MSSM anymore). In the Beyond the Standard Model Session at this meeting, we heard several talks from LEP and the Tevatron closing the susy parameter space, i.e. constraining the possible scales associated with supersymmetry. From LEP, chargino and neutralino limits are now in the range 65.2 and 28.2 GeV, respectively[14] (for $\tan(\beta) > 1$); for sleptons, the limits range from about 75 GeV to 84 GeV, with R parity conserved, and are not much weaker if R-parity is violated[15]; CDF can set chargino limits greater than 100GeV in much of the parameter space[16], with similar limits on stops[17]. DO sets similar strong limits

[1] One suggestion along these lines is known as the "racetrack" model[9]. This is an attempt to obtain a stable minimum at weak coupling. General issues connected with this idea will be discussed in a subsequent publication.

on charginos ($m_{\chi^\pm} > 150$, $m_{\tilde{q}} > 232$[18]). A number of analyses have been performed putting limits on gauge mediated models, assuming that the next to lightest supersymmetric particle decays quickly to gravitinos. DO sees no evidence for excess $\gamma\gamma$ events[18]; ALEPH places limits on neutralinos in such a picture of 90 GeV; indeed, only a tiny sliver of parameter space is consistent with an $\tilde{e}^+\tilde{e}^-$ interpretation of the famous CDF event[19]. Other LEP experiments also place strong limits. Finally, we heard a talk ruling out any light gluino window[20].

These results are interesting and perhaps worrisome for supersymmetry enthusiasts. Given the dimensional analysis or fine tuning argument, the most natural value for the masses of scalars are of order the Z mass. Masses much larger than this suggest the need for fine tuning. These problems are most severe for gauge-mediated models. In these models, one expects that the masses of the scalar doublets are of order $\frac{\alpha_2}{\alpha_1}$ times the masses of the charged singlet scalars. Given limits of order 90 GeV on these scalars, this suggests that the doublets should have masses-squared nearly an order of magnitude larger than M_Z^2. $b \rightarrow s\gamma$ raises similar puzzles; it would seem that the Higgs particles themselves must be surprisingly heavy. Of course, one should always be open to other possibilities, but it is too early to be pessimistic. We may have simply been a little unlucky, and there may be cancellations, or it may be that better models, such as some of the composite models, may explain these facts naturally. In the "SUGRA" picture, things are also getting tighter,and many workers feel it is necessary to relax some of the standard assumptions, e.g. unification of gaugino masses[21].

We can turn these arguments around. If supersymmetry has something to do with the solution of the hierarchy problem, then there is an excellent chance it will be observed at TeV II, and it will certainly be observed at LHC. At the recent Tevatron workshop, for example, it was found that the Tevatron may be able to rule out almost all of the parameter space of the Minimal Supersymmetric Standard Model.

It is fun to take an optimistic view, and imagine that five years from now we are in an era where we are unraveling a new, fundamental symmetry of nature. Having discovered several states, theorists are proposing real, plausible, models of the phenomena, and predicting the masses of new particles. Experimentalists are, with regularity, making new discoveries which cause great confusion. Congress is holding hearings demanding to know why NLC construction has barely started.

3 Neutrino Masses

Just in case we were beginning to despair that there are no new scales in nature, the past year has brought us the news of neutrino masses. SuperK has strongly confirmed the atmospheric neutrino deficit, which suggests, along with the solar neutrino data, that

$$\Delta m^2_{\mu\tau} \approx (10^{-2} - 10^{-3})\text{eV}^2 \qquad sin^2(2\theta) = 0.82 - 1.0. \tag{2}$$

These results are beautifully summarized in Gary Feldman's talk at this meeting. Here I would just remark on the question: what do we learn from this data? What does this tell us about new scales in physics? Neutrino mass is readily accommodated in the standard model, but it necessarily implies the existence of a new scale.[2] In the standard model, a neutrino mass arises from an operator of the form

$$\frac{\gamma_{ff'}}{M} H L_f H L'_f. \tag{3}$$

This is a non-renormalizable operator. It is technically "irrelevant," which is another way of saying that the neutrino mass is very small. It is often described as arising from the "seesaw mechanism," but the appearance of such an operator is more general, and would be expected to occur in any theory in which lepton number was violated at some scale M. At low energies, all we measure is the neutrino mass matrix,

$$m_{ff'} = \frac{\gamma_{ff'} v^2}{M} \tag{4}$$

M represents some scale of new physics. In grand unified theories, for example, it is often of order the grand unification scale, or somewhat smaller. In the seesaw mechanism M is the mass of a right-handed neutrino, and $\gamma_{\tau\tau}$ might be of order the τ Yukawa coupling, squared. Through the years, numerous papers have been written on such models, starting with[22]. It is relatively easy to cook up models which explain the data, though the large mixing angle is surprising. The real question is whether the model makes other testable predictions. In the parallel sessions at this meeting, we heard a description of a model[23] which illustrates the sorts of predictions which may be possible. This model naturally gives for γ a value of order m_t^2/v^2, which is several orders of magnitude larger than one might have expected. A further M_{GUT}/M_p suppression than accounts for the SuperK observations. The model also gives a nice

[2] Unless the masses are Dirac masses. Some physicists have suggested that we define the standard model to include right handed neutrinos, with very tiny Yukawa couplings.

picture of quark and lepton masses and mixings. It makes some definite predictions for proton decay:

$$\Gamma^{-1}(p \to \bar{\nu} K^+) \leq 10^{33}\text{yrs} \qquad \Gamma^{-1}(p \to \bar{\mu}^+ K^o) \leq 10^{34}\text{yrs} \tag{5}$$

The SuperK limits on these modes described at this meeting are 6.8×10^{32} and 4.0×10^{32}, respectively (in each case nearly an order of magnitude improvement over the preexisting limit)[24].

4 String ("M") Theory

String theory has scored many spectacular successes over the past few years. Perhaps most dramatically, it appears that the various known string theories – the only known consistent theories of quantum gravity – are all one and the same theory[25]. It is perhaps not too outlandish to speculate that this structure is the unique theory of gravity, and the unique possibility for a truly unified theory. While a complete non-perturbative formulation of the theory still eludes us, much has been understood about the fundamental dynamics of this theory, and plausible candidates for a complete formulation, at least under certain circumstances, exist. Finally, one of the most serious challenges to reconciling quantum mechanics and general relativity, the paradox proposed many years ago by Hawking[26] has been at least partially resolved[27].

Still, it has proven difficult to extract quantitative – or even convincing qualitative – predictions from the theory. One would like to know, for example:

- Does the theory predict low energy supersymmetry?
- If so, what is the pattern of soft breakings?
- Can one understand the origin of the flavor structure we observe?
- In light of the recent neutrino results, could one account for the scale of neutrino masses and the mixing pattern?

The third and fourth questions are almost certainly premature. Even the first, often casually cited as a prediction of superstring theory, is hard to pin down. The difficulty is not hard to understand. It is closely related to the often repeated statement that string theory is a theory without parameters. What is meant by this statement? What determines the couplings and the scales of the theory?

If string theory describes nature, all of these quantities must be determined dynamically. Within our current understanding the various coupling constants are determined by the expectation values of scalar fields, called "moduli." One would like to compute, say, a potential for the moduli, and show that it has a minimum consistent with the known values of the gauge and Yukawa couplings. Naively, on the other hand, one would expect that any minimum of the potential would occur when all of the couplings were of order one and the scales comparable. This can be made precise. For couplings, any potential one computes will go to zero as the couplings go to zero. Any minimum of the potential must lie in a regime where the couplings are of order one, i.e. where the theory is strongly coupled[28]. Similarly, in any ground state with approximate supersymmetry, the potential must vanish as the compactification radii tend to infinity.

Traditionally, the argument that the compactification scales are of order one has been made slightly differently. In the heterotic string, which was longed viewed as the most promising string theory phenomenologically, it was argued that the Planck scale, GUT scale, and string scale (M_s) must be comparable: $M_p \approx M_{GUT} \approx M_s$, since

$$\alpha^{-1} = g_s^{-2} V_{comp} M_s^6. \tag{6}$$

Here g_s is the dimensionless string coupling, $\alpha^{-1} \sim 25$, i.e. a typical grand unified coupling constant and V_{comp} is the compactification volume (the compactification radius is essentially the grand unified scale). If the theory is to be weakly coupled, so that a perturbative string picture is appropriate, $g_s \leq 1$, so the compactification scale and string tension cannot be very different. Newton's constant, similarly, is given by

$$G_N^{-1} = M_p^2 = V_{comp} M_s^8, \tag{7}$$

so the Planck mass must be of order the string scale.

This particular argument is suspect. After all, it is not obvious that the string coupling should be small. In light of recent understandings of duality, one might try to relax this. For example, the conventional supersymmetric calculation of the grand unification scale yields a value three orders of magnitude below the Planck mass. According to our formula for the gauge coupling, this would mean that the dimensionless string coupling is enormous. Witten argued, based on this, that a more suitable starting point for string phenomenology would be the strong coupling limit of the heterotic string[29]. Horava and Witten had previously shown that at strong coupling, the heterotic string appears eleven dimensional, with two ten dimensional "walls" at the end of the world[30]. If one takes literally these formulas, one finds that the eleven dimensional Planck mass, the GUT scale, and the compactification radius are all of order 10^{16} GeV. The size of the eleventh dimension, R_{11}, is of order 70 times the GUT scale. In this picture, the four dimensional Planck scale is not particularly fundamental. It is of order $R^{3/4}$ times the fundamental eleven dimensional scale.

In this picture, no parameter is particularly large. Still, it is puzzling that a pure number of order 70 should arise. Indeed, as R_{11} grows, the theory effectively becomes five dimensional (if the other compactification lengths are held fixed). Supersymmetry in five dimensions essentially forbids a potential for R_{11}, i.e. the potential must vanish in this limit, and it is hard to understand why stabilization does not occur for $R_{11} \sim 1$[31]. This is similar to the puzzle in the string theory picture of understanding why, in a strongly coupled theory, the gauge couplings should be small and unified[32]. Still, the picture is appealing and suggestive. Given the vast number of string compactifications, one can certainly imagine that some involve large pure numbers. Perhaps anthropic or other considerations might explain why one with a small coupling is favored. We are not currently in a position to pose these questions precisely.

5 A More Radical Proposal: Strings at the TeV Scale

In the last section, we discussed Witten's suggestion that the fundamental scale of physics lies at a scale well below the four dimensional Planck scale. This idea has been taken much further in [33, 34, 35, 36]. These authors explore the possibility that the string scale is of order a TeV. This has the virtue that, because the fundamental scale is of order the scale of electroweak symmetry breaking, one avoids the failure of dimensional analysis (hierarchy problem) which we discussed earlier. In this picture, the standard model fields live, again, on a brane or domain wall. As a result, the gauge couplings are not particularly sensitive to the size of the extra dimensions.

The first interesting question is the value of the other scales. In this picture, the four dimensional Planck scale is again a derived quantity. It is given by a formula of the form

$$M_p^2 = V_D M_s^8 \tag{8}$$

in a ten dimensional picture; here V_D is the volume of the internal space (in an eleven dimensional picture, the relevant scale is the eleven dimensional Planck scale). If n dimensions are large, while the others are comparable to the fundamental scale (now assumed to be a TeV), then $V_D \sim r^n$ in TeV units, and

$$r \approx (M_p/TeV)^{2/n}(TeV)^{-1}. \tag{9}$$

If the six compact dimensions are of comparable size, than the compactification scale is large, $r \sim ((\mathrm{Mev})^{-1}$. If only two of the dimensions are large, than $r \sim$ mm! This is quite a spectacular result. It means, for example, that if one can perform measurements at scales slightly less than a millimeter, one should see a change in Newton's law of gravitation from $1/r^2$ to that appropriate to six dimensions, $1/r^4$!

For general n, the assumption of a low string scale makes other dramatic predictions. For example, individual Kaluza-Klein states couple with gravitational strength, but there are lots of them. At energy scales above the compactification scale, the cross section is that appropriate to a theory of $4+n$ dimensions. It grows rapidly with energy. One can, in fact, set interesting limits on these theories from various processes involving large amounts of missing energy[38].

Apart from direct search experiments, there are many other constraints. The most obvious is baryon number violation. Baryon number violation is suppressed only by powers of $M \sim TeV$, so it is necessary to suppress many operators. In conventional supersymmetric models, it is also necessary to suppress certain operators of dimension four and five; this is usually achieved with discrete symmetries. In the case of a TeV fundamental scale, one needs to suppress operators up to something like dimension 12, so more powerful symmetries are needed. Still, one can postulate discrete symmetries or other possibilities which would do the job. If our goal is to rule out the possibility of large dimensions, then, proton decay is not sufficient. Accelerator searches constrain the scale to be greater than about 2 Tev. Flavor-changing neutral currents require some approximate flavor symmetries[37], and then one still probably requires a scale greater than about $5-10$ TeV[40]. Astrophysics constraints are particularly severe in the case of a low number of large compact dimensions. For $n=2$, in particular, the scale must be greater than about 50 TeV; otherwise, emission of light particles by SN1987a is too efficient[39].

From the work which has been done in this subject, then, it is clear that one cannot so easily rule out the possibility of a low string scale, and surprisingly large dimensions. The arguments described above for the case $n=2$ suggest that it is unlikely one will observe extra dimensions through a change in the power in Newton's law in Cavendish experiments. It is still possible that one will see alterations in gravity due to light particles[40]. One is unlikely, in this case, to be able to observe dramatic effects in accelerators.

It is hard to imagine a more exciting discovery than large extra dimensions, and it is clearly worthwhile to search for phenomena which might indicate their existence. But the fact that large dimensions are possible does not necessarily mean that they are plausible. In assessing the likelihood of large dimensions, there are two questions which seem appropriate:

- Is their any puzzle which large dimensions settle.
- Are their plausible dynamics which might give rise to such large dimensions.

To the first question, to my knowledge, the only motivation which has been suggested is the fine-tuning problem. This requires that the scale be close to the weak scale. The fact that for $n = 2$ the scale already has to be so much larger argues strongly against this possibility. For $n > 2$, the limits on the scale are already uncomfortably large (suggesting fine tuning of better than a part in 100). These constraints will get stronger over time. For the second problem, one typically finds that one must introduce a large number in a rather ad hoc fashion. Otherwise, not surprisingly, one finds that the scales all come out of order one. In some cases[41], the required coincidence is not much worse than that required in other ideas about fixing string moduli. Various other coincidences are also required. The cosmology of these theories is also problematic[42]. My personal view, based on these observations, is that large dimensions are not terribly likely, but that one should keep an open mind, both theoretically and experimentally.

It is worth mentioning variants on the brane ideas, which are not quite as extreme, but which raise similar theoretical issues[10, 43].

6 Flavor Physics

It should be stressed that most ideas about physics beyond the standard model, at scales of order the weak scale, have implications for flavor physics. Many proposals for low energy realizations of supersymmetry predict rare processes should be near the current limits. Similarly, the large dimension ideas which we discussed above almost inevitably to predict rates for rare K decays, $D\bar{D}$ mixing, and lepton number violation at levels not too far from the present limits. If the scale is low, the large dimension picture tends to predict that the CKM matrix is real[40], so that observed CP violation should occur through higher dimension operators. Rare K processes should be close to the experimental limits. Lepton violating processes might be forbidden by discrete symmetries (though this may be hard to reconcile with the observation of neutrino mass), but otherwise should be near the limits. As stressed in Marciano's talk[44], effects may even be observable in the muon $g - 2$.

7 Maximally Enhanced Symmetries

Duality is a very attractive idea. In some cases, dualities represent actual symmetries. A beautiful example is suggested by the discussion of electric-magnetic duality in classical electrodynamics, which you can find in Jackson's chapter 6. Under this symmetry, which everyone has pondered at one time or another, one has

$$\vec{E} \to \vec{B} \qquad \vec{B} \to -\vec{E} \quad e^2 \to \frac{2\pi}{e^2}. \tag{10}$$

When Jackson wrote his text, one could only speculate on the possibility of such a symmetry. Now we know that this is a true symmetry of string theory and some field theories. Apart from being theoretically satisfying, this observation raises the question: could it be that the state we observe in nature is a *fixed point* of such a symmetry[45]. Such a hypothesis is plausible for at least two reasons:

- It is technically natural; such enhanced symmetry points are necessarily stationary points of the effective action of the theory.
- It solves one of the most serious problems of any string cosmology, the "moduli problem."

Such a hypothesis is predictive: it suggests supersymmetry, with supersymmetry breaking at low energies, presumably through something like gauge mediation. All of the couplings are of order one; the scales are comparable. One disturbing feature of this hypothesis is that generically one has $\alpha \sim 1$ at such points. It is not known whether one can find points with small couplings.

8 A Parting, Cautionary Note

The hierarchy problem underlies much of our thinking about physics beyond the standard model. It is the only argument that new physics should appear at some particular, accessible energy scale. However, there is another stupendous failure of dimensional analysis, for which we have no persuasive explanation. The problem is the cosmological constant problem. Why is $\Lambda = 0$, or perhaps $\Lambda \approx 10^{-47} GeV^4$, as suggested by the recent supernova observations? This suggests that there might be some other explanation of hierarchies which we have simply not thought of.

Perhaps the most interesting recent proposal in this regard is that of [46]. These authors have constructed models without supersymmetry which have $\Lambda = 0$, at least in low orders of perturbation theory (I should note that, as of yet, these are toy models, which are certainly not realistic). There are heuristic arguments that these statements are true to all orders of perturbation theory. Harvey has argued that at least in some cases, duality suggests that there can be exponentially small cosmological constants[47]. Does one know what might be the low energy consequences of such a picture? Certainly not yet, but – be alert for surprises.

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