A Constructive Critique of the Three Standard Systems*

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It has become conventional to say that our knowledge of fundamental physical law is summarized in a Standard Model. But this convention lumps together two quite different conceptual structures, and leaves out another. I think it is more accurate and informative to say that our current, working description of fundamental physics is based on three standard conceptual systems. These systems are very different; so different, that it is not inappropriate to call them the Good, the Bad, and the Ugly. They concern, respectively, the coupling of vector gauge particles, gravitons, and Higgs particles. It is quite a remarkable fact, in itself, that every nonlinear interaction we need to summarize our present knowledge of the basic (i.e., irreducible) laws of physics involves one or another of these particles.

1 The Gauge Sector

The unambiguously good system is one describing couplings of the $SU(3) \times SU(2) \times U(1)$ gauge bosons. Deep principles of symmetry and locality greatly constrain the form of these couplings. When we combine these principles with the demand of renormalizability, we come down to a theory containing just one continuous parameter for each gauge group, namely its overall coupling strength. (Strictly speaking this is only true for the nonabelian factors, and only if we put aside the $\theta$ terms for those factors. I’ll return to the first of these points below; for more on the other, see [1].) This system gives us an extraordinarily economical account of the central features of the strong, weak and electromagnetic interactions, which is in excellent agreement with a host of accurate experiments.

This is not the place for yet another retelling of that story, wonderful though it is. Let me just invoke it with two familiar icons, which I’ll want to refer to later. We should not let familiarity blind us to their beauty and power.

Figure 1 shows the running of the coupling in QCD. It summarizes the results of many hundreds of independent measurements in different situations at different energy-scales, all

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of which conform to the predictions of an extremely tight theory. Two features are especially noteworthy. First, there is a special point labeled “lattice gauge theory”. Whereas the other points are grounded in perturbative QCD (including, to be sure, use of the renormalization group, non-perturbative factorization theorems, and multiloop calculations), this one employs the basic algorithmic definition, with no compromises, of the only relativistic quantum field theory that can withstand such use. Second, the theoretical band of allowed couplings focuses to the right, as we approach large energy scales. This means that by the time we reach the LEP experiments, our predictions are essentially parameter-free! Indeed, any reasonable value for the QCD $\Lambda$ parameter produces the same value of $\alpha_s(M_W)$, within a few percent (and the relevant quark masses have become, at these high energies, negligible).

Figure 2 shows the over-constrained fit of electroweak parameters to precision measurements. Such measurements were used to predict the mass of the W and Z bosons and the top quark before their discovery. Looking ahead, they suggest that the Higgs particle will be reasonably light. We expect $150 \text{ GeV} \leq m_H$ - eminently accessible at the LHC - unless of course the particles and interactions we know are somehow part of a larger conspiracy, which mimics the standard model accurately in all other respects.

1.1 Self-Transcendence

The empirical success of our gauge theories, and the tight, elegant mathematical structure they share, is so clear and impressive that we can build upon it with confidence. We
must take very seriously, and should strive to remove, the remaining esthetic flaws of these theories.

Looking critically at the structure of a single standard model family, as displayed in Figure 3, one has no trouble picking out flaws.

The gauge symmetry contains three separate pieces, and the fermion representation contains five separate pieces. While this is an amazingly tight structure, considering the wealth of phenomena described, it clearly fails to achieve the ultimate in simplicity and irreducibility. Let me remind you, in this context, that electroweak “unification” is something of a misnomer. There are still two separate symmetries, and two separate coupling constants, in the electroweak sector of the standard model. It is much more accurate to speak of electroweak “mixing”.

Worst of all, the abelian $U(1)$ symmetry is powerless to quantize its corresponding charges. The hypercharge assignments – indicated in Figure 3 by the numerical subscripts – must be chosen on purely phenomenological grounds. On the face of it, they appear in a rather peculiar pattern. If we are counting continuous parameters, the freedom to choose their values takes us from three to seven (and more, if we restore the families). The electrical neutrality of atoms is a striking and fundamental fact, which has been checked to extraordinary precision, and which is central to our understanding of Nature. In the standard model this fact appears, at a classical level, to require finely tuned hand-adjustment.

By demanding full quantum-mechanical consistency, specifically the cancellation of gauge symmetry anomalies, we can derive constraints among the hypercharges that very much improve the situation. Even at this level, however, there is no theoretical barrier to prevent a small admixture proportional to B-L into the electric charge operator, for example. In the minimal standard model there is a cubic anomaly in B-L itself, but that can be cancelled by including a standard model singlet, which we can identify as a right-handed neutrino. (It is interesting to note that even a tiny electric charge for the neutrino would forbid neutrino-antineutrino oscillations, or in other words Majorana mass terms, and con-
versely. This circumstance adds fundamental interest to an unmet experimental challenge, to determine whether the neutrino masses are of Majorana type.)

1.2 Unification

These two shortcomings of the gauge system in standard model, that is the occurrence scattered of multiplets and peculiar hypercharges, are both overcome quite beautifully if we are willing to postulate larger gauge symmetry (spontaneously broken, of course). With the natural embedding of $SU(3) \times SU(2) \times U(1)$ into $SU(5)$ we find the fermions of a single family fall into just two multiplets, a conjugate vector $\bar{5}$ plus an antisymmetric tensor $10$. Moreover their hypercharges are uniquely determined, and in agreement with experiment. The ugly ducklings of the standard model, upon unification, mature into graceful swans. The ultimate in unification, while not addressing family replication, is obtained with the slightly larger gauge symmetry $SO(10)$, as shown in Figure 4. Now the fermions are all in a single multiplet, the spinor $16$. The additional particle needed to fill out this multiplet is a right-handed neutrino. It plays an important constructive role in the theory of neutrino masses, by allowing the seesaw mechanism. A zealot might make a case that it has thereby already been observed, albeit indirectly. Be that as it may, the forced incorporation of a right-handed neutrino should probably be viewed as an asset rather than an embarrassment.

At first sight there appears to be a grave difficulty with these unification schemes, in that they appear to predict too much, specifically equality of the strong, weak, and properly normalized hypercharge couplings, which is definitely not what we observe. By now it is becoming a familiar story, depicted in Figure 5, that after calculating the dynamics of these theories properly and quantitatively, taking into account the effects of vacuum polarization, we find that this apparent difficulty might be resolved triumphantly. Indeed, extending the logic of QCD running, already displayed in Figure 1, to include the other interactions, we realize that the relative values of the couplings are scale-dependent. We should have equality of the couplings only at very short distances, or high energies, before the asymmetric screening and anti-screening clouds distort it.

Following out this idea, we find that by accounting for the renormalization effects of
virtual particles in the minimal standard model (as usually understood) we get qualitative but not quantitative agreement. But if we include expand the calculation to implement the effects supersymmetry, in a minimal realization, beginning at a mass scale of order 1 Tev, we find quite remarkable agreement, as shown in Figure 5. Low-energy supersymmetry is an attractive hypothesis for several other reasons, as I’ll review shortly, but this result, because it is quantitative, seems to me by far the most compelling.

The unification of gauge couplings occurs at a very large mass scale, of order $M_U \approx 10^{16}$ GeV, indicating that this is the scale of unified symmetry breaking. It is extraordinary that measurements at accessible energies, $10^2$ GeV or less, can point us so specifically to this enormously larger scale. It happens because the running of the (inverse) couplings is logarithmic, so that it takes a lot of leverage to overcome a modest difference. The logarithmic running of formally dimensionless couplings is a profound consequence of relativistic quantum field theory in four space-time dimensions. The apparent success of this unification calculation therefore, on the face of it, suggests that the principles of quantum field theory continue to be valid up to energies, or down to distances, many orders of magnitude beyond where they were discovered or have been tested directly.

The occurrence of a large mass scale $M_U$ has important conceptual advantages. Unification of gauge interactions inevitably involves putting quarks and leptons on the same footing, and upon doing this it becomes difficult to avoid the occurrence of significant transitions between them. From that arises baryon number violation, and thus proton decay, unless some conspiracy intervenes. Baryon number violation certainly occurs through gauge boson exchange in $SU(5)$ or $SO(10)$, or any large gauge symmetry group. For bringing the predicted rates down to an acceptable level, the heavy propagator suppression $M_U^{-4}$ is most welcome. On the positive side, the see-saw mechanism for neutrino masses gives the estimate $m_\nu \sim m_t^2/M_U$ for the heaviest observed neutrino mass, where $m_t$ is the top quark mass, and that’s close to what is observed.
It’s also most intriguing that $M_U$ is close to, although significantly smaller than, the Planck mass $G_N^{-1/2} \approx 10^{18}$ GeV. Gravity responds directly to energy-momentum, so its effective strength evolves with the mass scale (for virtual exchanges) at which it is defined, even classically. Another aspect of this is the Newton constant is, in our usual $\hbar = c = 1$ units, a dimensional coupling. It is as we approach the Planck scale that amplitudes involving graviton exchange, straightforwardly extrapolated, from being much feebler than the other interactions, approach quantitative equality. Independent of any detailed theoretical implementation, this numerical circumstance strongly supports the idea that a further stage of unification, in which both gauge interactions and gravity take part, is a physical reality.

### 1.3 Low Energy Supersymmetry

Low-energy supersymmetry has several other important advantages, besides its helpful role in the quantitative aspect of unification.

Low-energy supersymmetry protects the Higgs $(mass)^2$ term, which governs the scale of electroweak symmetry breaking, from quadratically divergent radiative corrections. As long as the scale of mass splittings between standard model particles and their superpartners is
less than a Tev or so, the radiative corrections to this (mass)$^2$ are both finite and reasonably small. (In detail, things are not quite so clean and straightforward; there is the “µ problem”, which is a very interesting and important subject, but too intricate to discuss here.)

This general qualitative relationship between mass splittings of supersymmetry multiplets and the observed weak scale penetrates also has a more specific and quantitative aspect. Supersymmetry relates the physical mass of the lightest, “standard model-like” Higgs particle, which in the absence of supersymmetry is a free parameter, to the masses of W and Z bosons. There is some model dependence in this relationship. But within minimal or reasonably economical supersymmetric extensions of the standard electroweak model the Higgs mass is generally predicted to be near – or below! – existing experimental limits, as shown in Figure 6. This renders the models subject to quick falsification at LHC or, more optimistically, to fruitful vindication. With vindication would come the emergence of a rich Higgs-sector phenomenology starting somewhere below 150 GeV.

The optimistic scenario gains credibility from another advantage of supersymmetry. This is the important though negative virtue, that for precision electroweak measurements supersymmetric extensions of the standard model generally yield only small deviations from the predictions of the standard model itself. That’s a good thing, because the standard model agrees remarkably well with these measurements. The situation is depicted in Figure 7. Several large classes of rival models to low-energy supersymmetry associate electroweak symmetry breaking with new strong interactions. In these models, which include Technicolor and both in its original form and in its extra-dimensional disguises, radiative
corrections to the Higgs (mass)\(^2\) are rendered finite by form-factors, rather than cancellations; and though the additional radiative contributions in these models are finite, there is no general reason to expect that they are especially small. Indeed, to the extent that they support specific calculations, one finds that such models generically have severe difficulty in accommodating existing precision measurements.

Finally, low-energy supersymmetry can provide an excellent candidate to provide the dark matter of cosmology. It’s plausible that the lightest particle with odd \(R\)-parity, where \(R \equiv (-)^{B+L+J}\) is stable on cosmological time scales, because the quantum numbers that go into the definition of \(R\) are well respected. The lightest \(R\)-odd particle, usually called the LSP (Lightest Supersymmetric Particle) could be some linear combination of the photino, zino, and Higgsino. Indeed, the production of these particles in big bang cosmology is about right to account for the observed density of dark matter.

2 Gravity

General relativity manifestly provides a beautiful, conceptually driven theory of gravity. It has scored many triumphs, both qualitative (big bang cosmology, black hole physics) and quantitative (precession of Mercury, binary pulsar). The low-energy effective theory of gravity together with the other interactions is defined algorithmically by the minimal coupling prescription, or equivalently by restricting to low-dimension operators. Since, in this context, “low” means compared to the Planck energy scale, this effective theory is very effective indeed. We can and do obtain unambiguous, apparently accurate answers in the applications above and many others by using this theory. And it is perfectly quantum-
mechanical, supporting for example the existence of gravitons as the particulate form of gravity waves.

What makes this very tight, predictive, and elegant theory of quantum gravity “bad” is not that there is any experiment that contradicts it. There isn’t. Nor, I think, is the main problem that this theory cannot supply predictions for totally academic thought experiments about ultrahigh energy behavior. It can’t, but there are more pressing issues, that might have more promise of leading to contact between theory and empirical reality.

A great lesson of the standard model is that what we have been evolved to perceive as empty space is in fact a richly structured medium. It contains symmetry-breaking condensates associated with electroweak superconductivity and spontaneous chiral symmetry breaking in QCD, an effervescence of virtual particles, and probably much more. Since gravity is sensitive to all forms of energy it really ought to see this stuff, even if we don’t. A straightforward estimation suggests that empty space should weigh several orders of magnitude of orders of magnitude (no misprint here!) more than it does. It “should” be much denser than a neutron star, for example. The expected energy of empty space acts like dark energy, with negative pressure, but there’s much too much of it.

To me this discrepancy is the most mysterious fact in all of physical science, the fact with the greatest potential to rock the foundations. We’re obviously missing some major insight here. Given this situation, it’s hard to know what to make of the ridiculously small amount of dark energy that presently dominates the Universe!

Another disappointing aspect of our effective theory of gravity is that it walls off the description of gravity from the theory of the other interactions. The minimal coupling procedure for incorporating gravity can accommodate any quantum field theory of the rest of Nature; it neither constrains nor significantly modifies the physical content of such theories. Thus it fails to live up to the promise of the unification of couplings calculation, with its pointer to the Planck scale, as I discussed above.

From these perspectives, a profoundly exciting aspect of supersymmetry is that the extension of non-gravitational (flat-space, rigid) supersymmetry to supergravity is considerably more complicated and delicate than straight minimal coupling. One must include very specific additional terms, some of which do not include gravitons or gravitinos. Indeed, these terms play important roles in attempts to construct phenomenologically viable models realizing low-energy supersymmetry. More specifically, the non-minimal terms are used, in conjunction with a gravitational “hidden sector” to generate soft supersymmetry-breaking mass terms, or a small $\mu$ term. People have discussed hidden sectors that derive from structures living elsewhere in extra spatial dimensions, or from additional gauge structures as suggested by the heterotic string. It’s remarkable that there are genuine prospects for accessing the deep structure of gravity, and maybe even discerning evidence for these other exotic structures, by experiments at accelerators!

3 The Flavor/Higgs Sector

The third sector consists, one might say, of the potential energy terms. They are the terms that don’t arise from gauge or space-time covariant derivatives. (Note that field strengths and curvatures are commutators of covariant derivatives.) All these terms involve the Higgs field, in one way or another. They include the Higgs field mass and its self-coupling, and
the Yukawa couplings. We know of no deep principle, comparable to gauge symmetry or general covariance, which constrains the values of these couplings tightly.

For that reason, it is in this sector where continuous parameters proliferate, into the dozens. Basically, we introduce each observed mass and weak mixing angle as an independent input, which must be determined empirically. The phenomenology is not entirely out of control: the general framework (local relativistic quantum field theory, gauge symmetry, and renormalizability) has significant consequences, and even this part of the standard model makes many non-trivial predictions and is highly over-constrained. In particular, the Cabibbo-Kobayashi-Maskawa (CKM) parameterization of weak currents and CP violation has, so far, survived close new scrutiny at the B-factories intact.

Neutrino masses and mixings can be accommodated along similar lines, if we expand the framework slightly. The simplest possibility is to allow for minimally non-renormalizable (mass dimension 5) “ultra-Yukawa” terms. These terms involve two powers of the scalar Higgs field. To accommodate the observed neutrino masses and mixings, they must occur with very small coefficients.

The flavor/Higgs sector of fundamental physics is its least satisfactory part. Whether measured by the large number of independent parameters or by the small number of powerful ideas it contains, our theoretical description of this sector does not attain the same level as we’ve reached in the other sectors. This part really does deserve to be called a “model” rather than a “theory”.

There are many opportunities for experiments to supply additional information. These include determining masses, weak mixing angles and phases for quarks more accurately; the same for neutrinos; searches for $\mu \to e\gamma$ and allied processes; looking for electric dipole moments; and others. The big question for theorists is: What are we going to do with this data?

### 3.1 Breakout Possibilities

It is very important to gather all that information, and in the process of doing so we might very well find direct evidence for new physics “beyond the standard model”, which would be great fun. But I don’t find it plausible that pinning down quark masses and the CKM matrix, or their leptonic analogues, or even discovering evidence for new sources of flavor change and CP violation, will give us the sort of impetus we’ll need to break through to a theory that’s better than, as opposed to just larger than, the third (Ugly) system as we know it.

If low-energy supersymmetry is indeed discovered, there will be many additional masses, mixings, and phases to sort out. There are profound questions to be answered here. The dark side of low-energy supersymmetry is that it offers many new potential sources of flavor and CP violation, including baryon number violation, which Nature has made surprisingly sparing use of. Some of these are associated with low-dimension operators, so that $a$ priori they are sensitive to physics at high mass scales, where exotic effects of quantum gravity and exchange of the new particles associated with unification are unsuppressed.

So various special mechanisms and symmetries have been postulated, to keep the basic ideas of low-energy supersymmetry and unification phenomenologically viable. None is uniquely convincing, and we will surely need experimental guidance to figure out which, if
any, is on the right track.

Here’s a specific example of an exciting and bizarre, but not altogether gratuitous or crazy possibility. There’s a parameter $m_0$, the universal soft mass term, that appears in models of low-energy supersymmetry. It’s a kind of fudge factor, and several theoretical ideas suggest that putting $m_0 = 0$ might be desirable. But concrete implementations of this idea appear to run into severe phenomenological problems. Specifically, they predict that the LSP is a charged particle, the stau; but cosmology puts severe constraints on the mass density in any new stable charged particles.

Now there’s a very interesting possibility to evade this problem. It could be that what appears to be the LSP, if we take into account only the supersymmetric partners of standard model particles, is not truly the lightest $R$-parity odd particle in Nature. Gravitinos, axinos, or some other weakly coupled “inos” might be lighter. The staus would decay into them. In that case, the stau lifetime could be very long by particle physics standards, but short on cosmological scales. These charged particles could leave long tracks, and one might even have difficulty observing that they decay at all!

Finally let me mention one redeeming virtue of the Higgs sector. (“Virtue” might be too strong; actually, what I’m about to do is more in the nature of advertising a bug as a feature.) The Lagrangian of the standard model, due to constraints of gauge symmetry, is constructed almost entirely from hard (mass dimension 4), strictly renormalizable terms. The lone exception is the term responsible for the Higgs field (mass)$^2$ term, which is proportional to the operator $\phi^\dagger \phi$.

Now let’s entertain the notion that there might be reasonably light, standard-model singlet particles deriving from physics at high energy scales. How could they couple to the particles we know? Since the constraints of gauge symmetry still operate, the couplings of these new particles have to be built on top of the couplings that appear in the standard model. Therefore – with one exception – they will have mass dimension greater than four, and they will be suppressed by coefficients in which inverse powers of the high scale appears. The exception, of course, is when they couple to Higgs particles. So the Higgs field is uniquely susceptible to this sort of exotic coupling.

Light particles of the sort just mentioned could arise as the Nambu-Goldstone bosons of spontaneous symmetry breaking, but in that case they’d be derivatively coupled, and the derivatives boost the mass dimension of possible couplings back up past four. More interesting from this perspective are moduli fields associated with flat directions in supersymmetry models. In the limit of exact supersymmetry they are massless but not derivatively coupled; if the scale of supersymmetry breaking is low, they could remain light enough to be accessible – but only through the Higgs sector!

4 Outlook/Apology

In keeping with my assignment here, I’ve focused on fundamental issues that have some fairly direct connection with the LHC program. Although this brief discussion has necessarily been quite selective, even given that assignment, I trust that it has served to emphasize that this program has extraordinary promise to advance our understanding of Nature in truly fundamental ways. Of course, there are other, complementary ways we can hope to
gain fundamental insights. I’ve discussed some of them recently in a similar style elsewhere [1]. But the LHC program is uniquely powerful and sure-fire.

The ideas about unstable LSPs and moduli fields allude to ongoing work with J. Feng and with B. Patt and D. Smith, respectively, which will appear shortly. I thank them for discussions and inspiration.

References

[1] F. Wilczek, “The Universe is a Strange Place”, keynote talk at SpacePartII, Washington, December 2003 (to appear in the Proceedings). ArXiv astro-ph/0401347