A Letter from the Energy Frontier To the Intensity Frontier

Raman Sundrum

November 27, 2011

Particle physics advances along two fronts, blasting for heavy states at the highest energies, and sifting for rare processes and minute effects at the highest intensities. The intensity frontier itself is multifaceted, consisting of leptonic and hadronic flavor violation, electric dipole moments and anomalous magnetic moments, neutrino properties, baryon number violation, dark matter and dark force searches, even tests of Relativity. With the advent of the LHC, the central mystery of Electroweak Symmetry Breaking is decisively within reach. To unravel this mystery and understand all its nuances will require pushing hard on both fronts. In this grand ambition, it is crucial to have open lines of communication. Theorists on either front can clamber up and peer ahead. As a theorist who struggles to glean what lurks at high energies, I have been asked to say a little about what I see and, just as importantly, what I don't see, and how it pertains to the intensity frontier. I feel inferior to this task, so I write this letter in the spirit of simply musing to myself.

Warm-bloodedness is a *robust mechanism* for animal life operating in a variable climate, with beautiful and distinctive structural features. A specific "model" expemplifying this broad mechanism is the "Rabbit". If one were told to identify an elusive warm-blooded animal in the vicinity, one strategy would be to just look out for a long pair of ears. Of course, this outward feature is not the inner essence of warm-bloodedness, so you might miss the humming bird flitting by. We must keep our eye on mechanism and not just details of a particular model, hard though this is. But suppose you get lucky, and indeed spot a long pair of ears in the distance. If rabbits were the *only* possible long-eared warm-blooded animal, then just spotting the long ears tells us everything. There is no need to examine the animal droppings for indirect clues. But of course this is not true, it might be a kangaroo, and the droppings might have been an important part of nailing this down. Without working this hard we might never learn the deeper story of the animal, where it came from and how it evolved. Another possibility is that, try as we might, we do not catch sight of any animal. In that case, studying the droppings may be our best and only option.

Beyond the Standard Model (SM), the situation is similar; we have uncovered a few basic and robust mechanisms, such as Supersymmetry or Higgs Compositeness, that might be at work in electroweak symmetry breaking, but there is at this point no precisely specified frontrunner *model*. The mechanisms are very deep, very beautiful, very provocative, and theorists have pushed their understanding and their possible variants. Theorists have also put forward numerous specific models that embody these mechanisms. Many of these are worth pursuing in experiments, but at this time none of them truly "sings". Perhaps the closest model that was thought to stand out from the crowd was the minimal supersymmetric standard model (MSSM), and even more specifically, the model known as "minimal supergravity" ("mSUGRA"). A lot of physicists felt it exhibited all the virtues in one compact package: a subtle new symmetry of spacetime, a simple form of breaking this new symmetry, a powerful solution to the hierarchy problem, a top-quark driven mechanism for radiative electroweak symmetry breaking, consistency with electroweak precision tests, a WIMP dark matter candidate in the form of the lightest stable superpartner (LSP), and a striking quantitative agreement with grand unification. It was also thought by some to automatically protect against excessive FCNCs, but this was ultimately understood as more of an unexplained input rather than an output of the model. The model also had long ears, which made it easy to search for. Experiments, from LEP2 to the Tevatron to the LHC, have now excluded a very large part of the natural parameter space.

In high energy searches, we are increasingly learning to search for key structural features of mechanisms, as opposed to *over*-optimizing searches for specific models, so that we don't miss the humming bird while looking for long ears. And if we do see long ears, we know we cannot simply reflexively assume it is a rabbit. We know we will have to study every possible clue to build that case. Let us do the following thought experiment. Suppose that TeV-scale supersymmetry is indeed present in Nature, and takes the form of the variant known as "low-scale" gauge-mediated supersymmetry breaking (GMSB), which has a beautiful and simple mechanism for ensuring that superpartner masses are flavor-degenerate, or "flavor-blind". This structure ensures a high degree of safety from low-energy flavor tests, but there are still non-trivial constraints since superpartners can amplify the flavor violation already inherent in the SM. GMSB is still a broad mechanism, but let us suppose there is a specific model chosen by Nature which has managed to escape past searches, and is discovered in the near future at the LHC, on the heels of a Higgs boson discovery.

We might have enough data to make the case that supersymmetry is at work, a truly grand discovery. Now, the mechanism of supersymmetry breaking becomes as urgent a question as electroweak symmetry breaking, and of course so is the question of how they are connected. Eventually, some physicists even suspect GMSB, partly because the LSP seems to be light, consistent with the light gravitino of GMSB, and scalar superpartners seem to be approximately flavor-degenerate. But at LHC precision we cannot be certain of these things, and what is seen does not fit any existing "text-book" model. Since flavor-blindness is one of the foundations of GMSB, flavor tests now become as vital in the new era as they were previously in testing the GIM and CKM mechanisms of the SM. Another key diagnostic is whether there are new sources of CP violation. Indeed GMSB theories can contribute signficantly to electric dipole moments (and even the muon magnetic moment), and measuring or better constraining these would be important for truly understanding the new physics. We will have to work hard to distinguish between GMSB and, say, the very different mechanism of "high-scale" anomaly-mediated supersymmetry breaking (AMSB) mechanism, where flavor-blindness is more conditional (on another mechanism called "sequestering") and might easily be less perfect than in GMSB. In the absence of singular models for each mechanism, and with the ever-lurking possibility that a weird model of AMSB, say, might fake a model of GMSB, details matter to figure out the bigger story. Even negative information from the intensity frontier represents positive information for understanding new physics at the energy frontier.

Let us go to the opposite extreme from GMSB, and suppose that supersymmetry is not at work, and that instead the Higgs boson is a TeV-scale composite of some new strong force (roughly the Technicolor or composite Higgs scenarios). Such theories cannot be flavor-blind, in fact they must really unify electroweak physics with non-trivial flavor physics. This possibility is challenging for theorists because it involves calculating, or at least guessing, in the face of strong coupling. One way to proceed is to discover new small parameters to control the dynamics. The $1/N_{color}$-expansion is the classic example. Less well known is the 1/scaling-dimension expansion which is at the root of the famous AdS/CFT correspondence. The Randall-Sundrum model and its modern "bulk SM" variants are warped extra-dimensional effective field theories that arise by exploring strongly-coupled weak scale physics in a systematic 1/scaling-dimension expansion. The expansion automatically *geometrizes* strongly-coupled quantum field theory and incarnates it in the form of extra dimensions, rendering bound state resonances as Kaluza-Klein resonances, and strong form-factors as extra-dimensional wavefunction overlaps!

Fig. 1 is a cartoon of a typical model and set of parameters, and it provides a very attractive picture of physics beyond the SM. The extra dimenisonal space is minimally a microscopic line interval so that the higher-dimensional spacetime is essentially a "waveguide". A mode decomposition arises upon studying the different higher-dimensional wave equations: Einstein, Maxwell, Yang-Mills, Dirac, and Klein-Gordon Equations. The lowest mode of each particle species is identified with some SM particle, and excited modes with new physics at roughly several TeV. The modes are distorted in part by the hidden curvature ("warping") of the higher-dimensional spacetime. The wavefunction overlap between the graviton and the Higgs is robustly very small, and indicates that electroweak symmetry breaking gravitates very little, that is SM masses are much smaller than the Planck scale. This is how the hierarchy problem is solved. SM fermion wavefunctions are more volatile in terms of which way they lean and by how much. A SM fermion strongly overlapping the Higgs gets a large electroweak breaking mass, say the top quark. A SM fermion leaning away from the Higgs gets a small mass, say the electron. And one can clearly also get flavor mixing in the mass matrices. In this way a pretty theory of flavor appears above the TeV scale.

Even though the model describes physics up to extremely high energies, the lowest excited "Kaluza-Klein" (KK) modes are redshifted by the high spacetime curvature to appear at roughly the TeV scale, within striking distance of experiments. But this new physics is also sensitive to the flavorful extra-dimensional wavefunctions, and can mediate FCNCs at the TeV scale. Without strong suppression, this would of course spell disaster. Fortunately, the KK modes lean in the same direction as the Higgs, so that the light fermions also have small overlaps with KK modes and KK-mediated FCNCs are very strongly suppressed, enough to allow KK states to be present at several TeV.

Now let us imagine this is how Nature works, and that the LHC experiments just barely discover evidence for the lowest rungs of the KK ladder, say some broad and mild excesses in tails of top-quark production, and some non-minimal Higgs physics arising from the extra-dimensional "radion". Again, we might well guess the broad paradigm, but we cannot really do justice to this massive discovery without pursuing the complementary view on the intensity frontier. We might get luckier, with distinct new resonances in a number of channels showing at the LHC. Even then, LHC data would not come with Fig. 1 attached, we would need to dig this out from a combination of high-energy and high-intensity experiments.

It might be even worse than that and collider signals are somewhat beyond reach of the LHC. Then, improvements in low-energy tests, with their greater virtual reach, might be the only game in town.

There is a kind of standard contemplation in flavor physics. To illustrate it consider new physics, which at low energies reduces to

$$\delta\mathcal{L} = \sum_{i,j,k,l} c_{ijkl} \frac{\bar{\psi}_i \psi_j \bar{\psi}_k \psi_l}{\Lambda^2}, \tag{1}$$

some flavor-changing higher-dimensional interactions among quarks, where the indices label different flavors. Suppose Λ is some common scale far above collider energies and that the (real and imaginary parts of) c_{ijkl} are all of *comparable* size and order unity, but with no other particular pattern. In this situation it is hard to beat neutral kaon mixing and CP violation as a probe of Λ. However, it is also challenging to improve on what we already know, which constrains Λ to lie above $\sim 100,000$ TeV. Even significant gains in other channels in the forseeable future will not compete with this.

But c_{ijkl} "anarchy" of this type is not a firm prediction of new physics. For example, the picture of Fig. 1 does not produce this structure, but rather one which amplifies third generation FCNCs relatively. In such a setting, improvements in other channels really can overtake the kaon system. Because we have a broad mechanism, not a specific model, even if the picture of Fig. 1 is essentially correct, I cannot say at this time whether such a breakthrough on the intensity frontier will happen in the domain of B physics, electric dipole moment measurements, or lepton-flavor violation. Even in the supersymmetric paradigm, there can be major breaks from flavor-blindness, which if realized in Nature would make improvements in flavor tests invaluable. (I am right now toying with a different possible structure of supersymmetry breaking where the dominant action is in lepton flavor violation over hadronic violation.)

Even neutrino masses and mixing may be a decisive arena. Neutrino data has certainly broadened the challenge for any comprehensive theory of flavor. The picture of Fig. 1 can, in more detail, explain why these lightest fermions exhibit a *qualitatively* different pattern of mixing from the quark sector. I think the nicest (but not only) version of the mechanism exploits Dirac neutrinos. Observation of neutrinoless double-β decay would exclude this.

It might be argued that I am being too optimistic. In recent years, the over-riding criterion for new physics posed by the hierarchy problem, and naturalness versus fine-tuning more generally, has been seriously questioned. Naturalness is essentially a gambler's tool, and we are necessarily gamblers on where and whether to hunt for new fundamental physics. The insight of the Anthropic Principle is that we cannot afford to just contemplate absolute probabilities for different physical laws and fundamental constants (as best we can) while viewing ourselves (and complex astrophysical, chemical and biological structures) as merely incidental features. Instead, we must study the *conditional* probabilities (again, as best we can), conditional on the existence of complex structures in which creatures like us would exist to even ask these questions. A theoretical model that seems "tuned" by the usual naturalness criteria of effective field theory may in fact be quite typical when restricted to theories which host complex "enough" structures.

This insight cannot be wrong, but the challenge is in how to apply it. Its dominant application has been to the cosmological constant naturalness problem. In this context, there is simply no well understood *mechanism* for a naturally small cosmological constant, while it is undoubtedly extremely small in our universe. Many theorists believe instead that the anthropic principle offers the best explanation. Such a theory makes an overwhelmingly strong bet for the identity of the dark energy observed to accelerate the universe's expansion (and for which this year's Nobel prize was awarded): it is just a small cosmological constant. Without competitors, this is a singular model of dark energy. There is then little point in making (or funding) detailed precision observations of dark energy, such as its equation of state. We already know the model. The debate over this position is still playing out. The catch is that we do not know if we have just been too stupid to think of competitor mechanisms and whether maybe a little precision data would help. I hope so.

I would say at present, when applied to electroweak breaking, we simply do not know the extent to which a consideration like the anthropic principle skews the odds. It may be that anthropic considerations are negligible, full naturalness applies undiminished, and a full resolution to the hierarchy problem exists. Even LHC data has not yet ruled out this possibility, and there is then an excellent chance for discovery in the coming years. Alternatively, it could be that anthropic considerations play a small but important role, so that new physics exists above several TeV and solves the bulk of the hierarchy problem, but imperfectly, so that naively the physics would appear tuned, say by one part in 1,000 or in 10,000. In this case, the LHC would miss all or most of the new physics. With only a few intriguing new light particles or excesses, or perhaps nothing, visible at the high energy LHC, the intensity frontier (in which I include a lot of LHCb data) would be decisive for the field.

Or it could be that, in the final analysis, the hierarchy problem is completely offtrack in predicting new physics. After all, the SM is itself a rather singular model. It is not just its minimality, which is to some extent in the eye of the beholder. Also, it does not account for all data, for example dark matter. But it automatically contains accidental symmetries (like baryon number symmetry) and structures (like GIM) that explain why we have not seen what we have not seen. In this, it is unparalleled (though not perfect, in that it still suffers the Strong CP Problem). The case in the SM's favor owes a lot to the intensity frontier. Maybe the SM reigns supreme, with dark matter playing out in a quite disconnected sector? But the SM must completely fail by the Planck scale, the origins of flavor must open up before that scale. Something must give in the renormalizable SM by $\sim 10^{16}$ GeV, with or without grand unification. From the $\sim$ GeV scale of the intensity frontier this is 16 orders of magnitude. Proton decay experiments have probed essentially all this way. In a see-saw context neutrino masses may run a close second, though there are other ways to interpret neutrino data. Kaon physics has already probed up to 10^8 GeV, half the possible hierarchy, although it too certainly has its blind spots. These data, even the negative results, represent giant steps between us and the maximal energies of particle physics. If the SM is essentially correct for decades of energies above the weak scale, then I think there is still a noble and interesting goal of establishing this as best we can. Does the SM break down at 30 TeV, 100 TeV, or $100,000$ TeV? Where is the summit? I think trying to go this high, in independent motivated channels is a heroic challenge. But I have not done enough homework to tell which channels are the best bets, how to ration scarce resources, or how to anticipate the effects of improvements in computation and analysis.

One last thought experiment relates to the "real-time" process of hunting new physics on the high energy frontier. A new excess appears beyond SM expecations. An example to keep in mind is the CDF anomaly in the top-quark forward-backward asymmetry. A hundred such anomalies come and go, and will come and go at the LHC. But maybe the hundred-and-first will be the real thing. So we all pay attention, we try to make sense of the data with this and that trial model, along with possible experimental error or miscalculation. But here is what I have observed in myself and many other seekers beyond the SM: we know we are surrounded by darkness, confusion and error at every turn. Therefore for every mental step we take into new physics possibilities, we glance back to the intensity frontier for encouragement or discouragement. We must take chances, stick out our necks, but our best bet is to take precision low-energy data very seriously. We try to understand mechanisms in which new physics explaining the anomaly at hand could robustly have hidden from the intensity frontier. We take encouragement if such a mechanism is available, and ask how we can probe further in other high energy channels. In this way, the low energy precision data is a constant filter for our reactions to new anomalies, for new channels and models, and for which anomalies are plausible enough to even take seriously.

I don't think I am saying anything surprising. The LHC era could take many possible twists and turns on the high-energy frontier, triumphs or disappointments. Yet under any circumstance I can conceive, it seems that advances on the intensity frontier would be very important and highly complementary, rather than redundant. This does not say in detail how to prioritize what to do on this frontier, but it gives urgency to this question, which will require assessing physics considerations that cut across traditional subdisciplines.

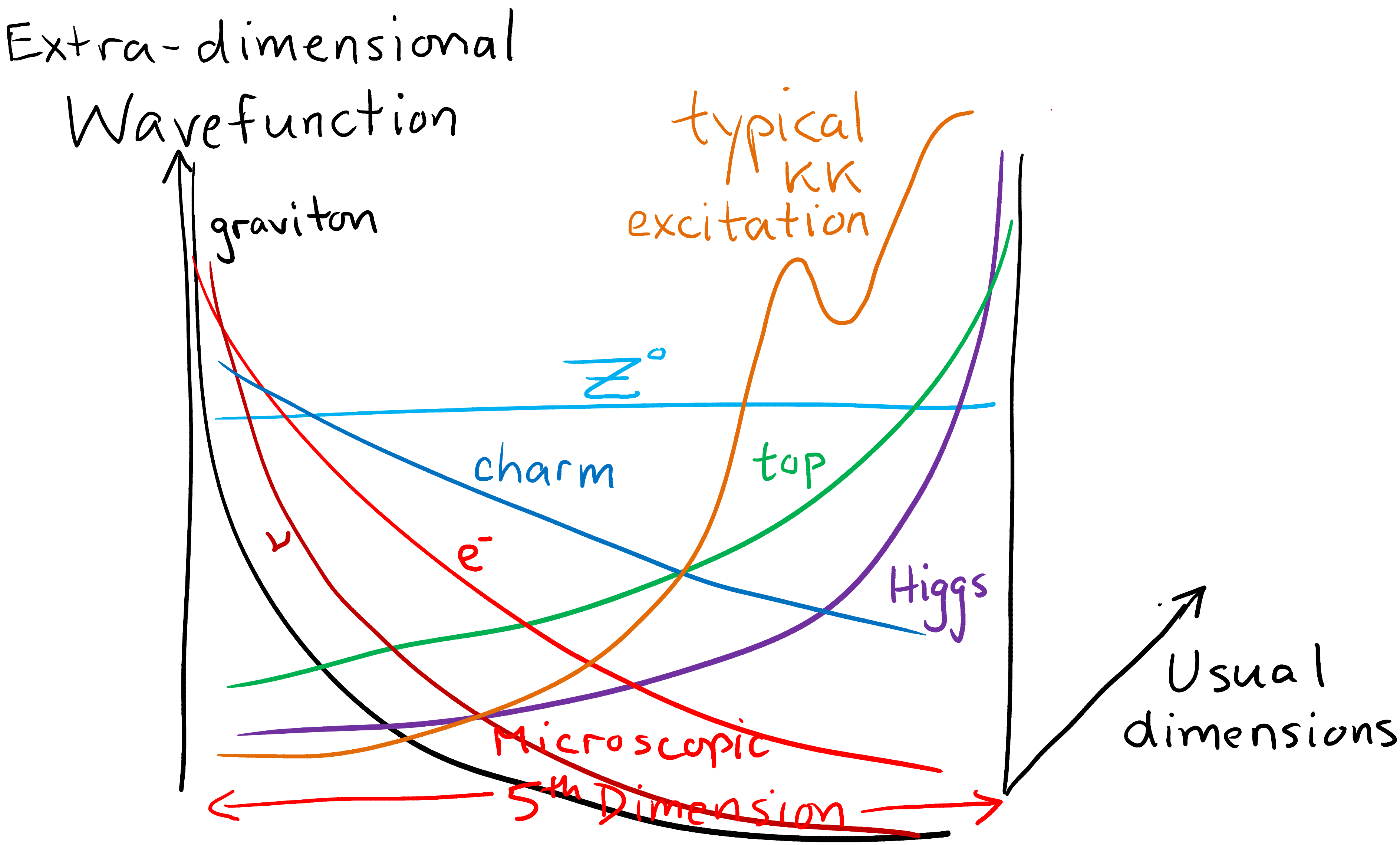

Fig. 1 Particle Geography ≡ Strong Dynamics