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The State of Exploratory Theory Beyond the Standard Model

David J. Gross

Princeton University, Princeton, NJ 08544

Gordon Kane

University of Michigan, Ann Arbor, MI 48109

Edward Witten

Institute for Advanced Study, Princeton, NJ 08540

1 Introduction

One of the most fundamental unsolved problems in physics is how the electroweak symmetry is broken and why the symmetry breaking scale is so tiny compared to the natural scale of unification or gravitational phenomena. The latter question is a modern version of the "large numbers problem" of Dirac, who wondered why the proton mass was so much less than the Planck mass.

One of the most attractive frameworks for addressing this question is supersymmetry. The standard model has a fascinating supersymmetric extension which has remained viable over the years while other alternatives such as composite models have encountered difficulties. The supersymmetric standard model resolves the large numbers problem and is also compatible with our limited experimental constraints on TeV physics. It hints that a whole new world of physics is just around the experimental corner, just out of reach of present day experimental tools. But is it right? Only experiment can tell; the supersymmetric standard model can be vindicated only by discovery of the Higgs boson and the other new particles that it predicts at energies not much greater than that of the Higgs.

Discoveries of the Higgs boson and other possible TeV scale particles might also give clues about some of the other mysteries left open by the standard model of particle interactions:

- What determines the gauge group of the world at ordinary energies to be $SU(3) \times SU(2) \times U(1)$?
- How does the other force in nature, gravity, enter the picture?
- How can the gauge forces be unified with each other and with gravity?
- What determines the quantum numbers of the quarks and leptons?
- Why does nature seem to repeat itself, with several "families" of quarks and leptons that appear to be identical except for the masses?
- What determines the bare masses of the quarks and leptons and the mixing angles that control their lifetimes?
- Why, in particular, are some of the quarks and leptons – like the electron and the up and down quarks – so much lighter than what would appear to be the natural scale, which is the mass scale of the W and Z bosons, the top quark, and presumably the Higgs boson?
- Why does the cosmological constant vanish?

One of the first significant approaches to these questions, by now twenty years old, was the idea of a "grand unified" gauge theory (GUT) with $SU(3) \times SU(2) \times U(1)$ unified in a simple group such as $SU(5)$. The $SU(5)$ model, and its variants, gave such a beautiful explanation of the strange fractional quantum numbers of the quarks and leptons that it is hard to believe that there is not some truth in it. These theories also made a spectacular prediction of proton decay and gave a strong hint of neutrino masses and mixing. These predictions have not yet been confirmed (though the solar neutrino problem may be giving us an emerging discovery of neutrino masses and mixing); on the contrary the original $SU(5)$ model in its simplest form was eventually excluded by failure to observe proton decay at the predicted rate.

Another early success of the $SU(5)$ model was the rather good prediction it made for the mixing angle of the electroweak theory – the parameter called $\sin^2\theta_W$ that measures the ratio of the $U(1)$ and $SU(2)$ gauge couplings. In more recent years, however, experiments – such as the precision measurements of the strong, weak,and electromagnetic couplings at LEP – have shown that while the $SU(5)$ prediction for $\sin^2\theta_W$ is quite close, it is not close enough.

Just as the standard model of TeV physics has an attractive alternative in the supersymmetric standard model, grand unified theories have viable supersymmetric extensions. Indeed, the effect of supersymmetry is quite fascinating – the small error in the original $SU(5)$

prediction of $\sin^2 \theta_W$ is corrected, bringing the prediction right on top of the best experimental values (if supersymmetry survives to energies close to a TeV); and the proton lifetime is extended beyond the experimental limits. (Of course the supersymmetric grand unified theories also solve the large numbers problem.) Are these fundamental facts about nature or frustrating coincidences? The best way to find out is to find– or not find – superpartners at the TeV scale.

Another fascinating property of GUT's is that the mass scale of unification can be computed using the renormalization group and the values of the couplings at accelerator energies, and,especially in the supersymmetric version, the answer turns out to be rather close to the Planck mass. Though we certainly don't fully know why this is so, it is a promising sign since we do know that the Planck mass is an energy at which new things must be happening in physics.

Yet GUT's, with or without supersymmetry, leave open many of the questions on our above list. They suggested some new perspectives but in fact led to limited progress in explaining the multiplicity of fermions and the quark and lepton masses and mixing. Their potential to constrain the low energy gauge group appears to be limited. They of course leave gravity and the cosmological constant out of reach.

To incorporate gravity is perhaps the thorniest knot in theoretical physics because – as has long been perceived – the nonlinear structure of general relativity clashes with the requirements of renormalizable quantum field theory. To make sense of gravity in the light of quantum physics would appear to require a new framework, and luckily a new framework did appear in the form of string theory, which originally was developed in the late 1960's and early 1970's in the context of hadron physics.

At first, when it was perceived that string theory could be interpreted as a new framework for physics including gravity, it appeared that the theories that arose, though they were unified theories of gravity and matter, were unrealistic (the weak interactions had to conserve parity, for instance). By 1984, however, new developments involving anomaly cancellation and the heterotic string had made it possible to make more or less realistic models of particle physics from string theory. New insights appeared about why the gauge groups that we actually see might be natural and why nature might naturally generate several families of fermions.

The whole framework of string theory has proved to be incredibly rich. It is amazing to see how phenomenologically interesting gauge groups and fermion representations, not to mention general relativity, appear out of elegant and simple world-sheet constructions. Many new ideas have come from string theory – just to give one example, supersymmetry originated nearly twenty-five years ago in attempts to incorporate fermions in string theory. It is hard to imagine what theoretical physics would be like today without this enriching influence.

On the other hand, there is so much that is not understood. We not only do not know how to determine the vacuum state – which underlies observed particle physics – but we do not even have a sensible framework for asking the question. All we really know is how to construct perturbation theory around classical solutions. TeV scale supersymmetry is very natural from the point of view of string theory, but it has not been deduced as a true prediction. Almost all more or less phenomenological work in string theory has assumed TeV scale supersymmetry, but – pending experiments at the necessary energies – it is possible for all we know that this assumption is completely on the wrong track.

Assuming that TeV supersymmetry is correct, one of the most compelling questions is to find a sensible mechanism of supersymmetry breaking. This has not emerged yet. The problem is not to break supersymmetry– this occurs naturally through chiral symmetry breaking in one or another piece of the gauge group – but to break it in a way that leads to a stable vacuum and vanishing cosmological constant.However, the absence of an understanding of supersymmetry breaking does not prevent proceeding with useful phenomenology and studies, because the non- renormalization theorems and the knowledge of the form of the general soft supersymmetry breaking terms means one can write the most general effective Lagrangian at a high scale and study its implications for experiments and measure its parameters.

Studies of dynamics of supersymmetric models beyond perturbation theory have in fact been carried out most intensively in field theory – where better non-perturbative formulations are known and more non-perturbative methods are available. These studies, pursued intensively at different times since the early 1980's, have uncovered many fascinating phenomena and shed much light on four-dimensional quantum field theory in general. But phenomenologically useful mechanisms of supersymmetry breaking have not yet emerged.

Should supersymmetry indeed be discovered at the TeV scale, many other discoveries will inevitably come with it. There should be a whole garden of new particles. Which are lighter and which are heavier? Is the mechanism of supersymmetry breaking visible at the TeV scale, or does it come from higher energies? And in the latter case, through what effective interactions in the low energy theory is the supersymmetry breaking manifested? And what clues can one draw from that about the energy scale at which the supersymmetry breaking originates?

It is impossible to foresee all the ramifications of a discovery of supersymmetry at TeV energies, except that it would open up one of the golden ages in experimental physics as answers appear to some of the questions just raised. And it would give a tremendous, though not fully predictable, boost to the efforts of theorists, as well as experimenters.

2 Experimental Opportunities to Detect Supersymmetric Physics

As we have already suggested, there are stronger reasons than ever to think that nature may be supersymmetric at the TeV scale. The arguments of the early 1980's have held up: a supersymmetric world could maintain a hierarchy of scales and thus allow the particles and forces to unify; the supersymmetric standard model has candidates for cold dark matter, unlike the ordinary standard model; and the Higgs mechanism can be derived more naturally than in the ordinary standard model. Meanwhile, new arguments have been added, both theoretical and phenomenological ones. On the theoretical side, the emergence of phenomenologically attractive superstring models has focussed renewed attention on the possibility that supersymmetry survives down to electroweak energies. Phenomenologically, a major development hinting at supersymmetry – already mentioned above – is the beautiful quantitative agreement of precision measurements of the low energy coupling constants with predictions of supersymmetric unified theories; the non-supersymmetric counterparts of those theories fail by many standard deviations given the precise data now available. Another important phenomenological result, unexpected by most physicists ten years ago,is that the top quark is "heavy" – that is, unlike the other quarks and leptons, its mass is comparable to the electroweak scale. It was already clear in the early 1980's that gauge symmetry breaking would work out correctly for a much larger range of the parameters if the top quark was heavier than about M_W; that prediction, which was not considered terribly attractive at the time, has indeed been satisfied. Further, in recent years very detailed supersymmetric models that impose all known theoretical and phenomenological constraints have been successfully constructed; it could easily have happened that once one went beyond general arguments it would turn out that it was not possible to satisfy all the constraints simultaneously. Among other things, the lightest superpartner of such models (the LSP), indeed has the properties to give about the right amount of cold dark matter.

Of course, the above indications for supersymmetry could be coincidental and misleading. Until superpartners or the supersymmetric Higgs spectrum are discovered experimentally, the situation will not be settled. Nevertheless, the indications sketched above are suggestive enough that, in our opinion, testing supersymmetry and discovering superparticles should be one of the prime goals of future experiments. Discovery of supersymmetry would be one of the great event in physics; it would profoundly affect experimental priorities and decisions about new facilities and refocus the efforts of many theorists. If electroweak physics is not supersymmetric,many theorists are working in the wrong direction, and it would be useful to know that as soon as possible.

How can we get information about the masses of the superpartners and Susy-Higgs bosons, in order to tell us what facilities are needed to detect them? How can we get information on cross sections and signatures so we can know what luminosities are needed to produce and detect them? There has been progress here too in the past couple of years. Detailed quantitative models have been constructed that give a complete set of predictions for masses and couplings, so that all observables can be calculated. There are parameters in the models, and some assumptions underlying them, mainly reflecting our lack of understanding of how supersymmetry is broken, so that most predictions are not unique. Nevertheless, the constraints are strong enough that many useful predictions can be made.

First, the models imply that we would have been very lucky if superpartners or a Higgs boson were already detected; only a few percent of the parameter space has been covered in existing experiments. Further, the masses of the charginos and neutralinos are always expected to be rather light, within a factor of two or so of the W and Z masses, and therefore detectable at LEP2 and at FNAL (particularly if its luminosity or energy or both are upgraded) relatively soon. Other partners are often very light, *e.g.* the stop or superpartner of the top quark. If very recent indications from LEP, particularly concerning the branching ration for $Z \to b + \bar{b}$, persist as the data become more precise, then upper limits of order 100 GeV can be set on chargino and stop masses; they have large production cross sections at LEP and FNAL, and good signatures. Further, the models indicate that the LHC will produce large quantities of most (or all) of the superpartners and SUSY-Higgs bosons, so that with sufficiently good detectors it will be possible to study many of them and learn the basic parameters of the theory. A high-energy electron-positron collider (NLC) will also be valuable to study many superpartners, and it is complementary to the LHC to a large extent; for example, gluinos will be produced copiously at LHC but little at NLC, while charginos and sleptons will be easily studied at LEP or NLC, particularly with polarized beams that can play a major role in separating different states and measuring their masses and couplings. If indeed nature is supersymmetric on the electroweak scale, and a perturbative unification occurs at a scale of order 10^{16} GeV, a remarkable consequence is that it is possible to calculate predictions for collider data from the theory at the unification scale, and conversely to learn the form of the effective Lagrangian at the unification scale from collider data. Effectively we are able to make certain measurements at the unification scale. Connecting the Planck scale to the electroweak scale may seem difficult, but if we can probe the unification scale then it does not seem so implausible to go the remaining two orders of magnitude to the Planck scale.

Another major consequence of finding superpartners is that automatically the LSP is produced in the decays of all of the others. Thus its mass and couplings will be measurable at whatever collider first detects a superpartner. That will allow determination of whether it has the right properties to be the cold dark matter of the universe. Furthermore, knowing the characteristics of a candidate particle would also help focus efforts at direct detection of cosmic dark matter.

If TeV scale supersymmetry is wrong, one may ask how can it best be disproved experimentally? One particularly efficient and rigorous way to do this is known. In the supersymmetric standard model, there is an upper limit on the mass of the lightest SUSY-Higgs boson, independent of detailed assumptions. The numerical value of the limit (but not its existence) depends a little on the gauge theory. For the simplest gauge theories the limit is below about 150 GeV; for more complicated ones it can go up of order $10 - 15\%$. Upper limits on superpartner masses also exist in all models that can satisfy the existing theoretical and phenomenological conditions. These limits, presently of order a TeV for the heaviest partners, are still dependent on parameters and assumptions, but data are becoming strong enough to set lower and tighter limits. These limits depend less on the parameters of the theory and on other assumptions. Thus if experiments do not find superpartners at LHC (assuming sufficiently good detectors) it will be possible to conclude that supersymmetry is not relevant to understanding the physics of the electroweak scale and that the values of the measured gauge couplings are not really results of TeV supersymmetry. Indeed, if the recently reported branching ratio , $(Z \to b\bar{b})$ at LEP does not change by more than about one standard deviation, it maybe possible in a few years to exclude supersymmetry very generally if charginos and stops and the LSP are not discovered at LEP2 or an upgraded FNAL.

There may be further indirect indications of supersymmetry. If protons decay it will be a strong signal of unification in general. In a supersymmetric theory these decay modes are generally different than in other theories (usually $\nu + K^+$ dominates in supersymmetry). The branching ratio for $b \to s + \gamma$ can be greatly affected by superpartners since it must vanish in the supersymmetric limit. Thus depending on what its measured value is and on the errors, this process may be able to distinguish supersymmetry from the standard model. Other rare decay modes and loop contributions may also be important, particularly in conjunction with other measurements for testing supersymmetry.

Because of the existing indirect indications for supersymmetry, and the extraordinary consequences for physics if it is detected, we urge that every opportunity to get evidence of supersymmetry be exploited as quickly and thoroughly as possible, and that planning for detectors, upgrades, facilities, and funding priorities include consideration of opportunities to learn about whether nature is supersymmetric.

3 Recent Theoretical Progress

Even in the absence of direct experimental stimulus exploratory theory remains vital and exciting. New ideas have been developed over the last few years that could shed light on some of the deep issues in string theory, quantum gravity and in supersymmetric theories. In addition many of the methods and techniques that have been developed to explore these questions have applications in other areas of physics and mathematics.

We comment below on four areas of research that have been active over the few years:

- the study of classical solutions of string theory—two dimensional conformal field theory,
- black hole physics,
- matrix models and large N techniques, and
- supersymmetric field theory.

One major focus in the last few years has been on understanding classical solutions of string theory. A basic paradigm here relates classical solutions of string theory to conformally invariant field theories in two dimensions; this paradigm, many of whose implications are still mysterious, has no real analog in pre-string physics. Because of the paradigm, work on classical solutions of string theory often interacts with studies of critical phenomena and many-body physics, such as the Kondo problem and the fractional quantum Hall effect, which can be described by two-dimensional field theory.

Working within this paradigm, string theorists have discovered many unusual physical phenomena that occur in string theory under extreme conditions. One important observation is that there seems to be in string theory a minimum size of any dimension of space-time; if one tries to contract space-time below that scale, it re-expands in a new "direction" – the phenomenon cannot be described in pre-string terms. It seems that the distance at which this occurs is actually in some sense the smallest measurable space-time distance. For instance, the Heisenberg microscope fails to work at very short distances: the ability to probe shorter distances by accelerating particles to ever-higher energies is eventually limited by the fact that at high enough energies strings begin to *grow*. Other strange things occur when one probes the limiting distance scale. One example is that the topology of space-time can change purely at the classical level, with no reference to quantum corrections:

one can continuously interpolate from one classical space-time to another, passing through an intermediate stage in which stringy effects are big.

Closely related to this are new, non-classical symmetries found in string theories, such as mirror symmetry; our knowledge of such symmetries has greatly increased in the last few years. One of the main motivations in this work has been to make more computable some of the models of elementary particle physics that can be derived from string theory. For instance, mirror symmetry was used—and in large part was developed—to make it possible to calculate Yukawa couplings that were otherwise out of reach.

The strange new phenomena found in classical solutions of string theory hint at a new, not yet understood, formulation of the theory in which space-time would be a purely derived and approximate concept, just as traditional concepts like the position and momentum of a particle became approximate concepts in quantum mechanics. These investigations might perhaps be compared to the discovery of the Klein paradox in the early days of quantum electrodynamics—a strange phenomenon in classical solutions of the Dirac equation that pointed the way to a better understanding of the role of the equation in physics.

Some classical solutions found in string theory are closely related to black holes. Thus, one has been led to ask what light string theory—which after all is claimed to be a consistent theory of quantum gravity—sheds on the longstanding paradoxes that arise when black holes are considered in of quantum theory. In the last few years, there has been intensive study of two dimensional black hole models that can be treated quantum mechanically. The analyses are substantially more precise than previous treatments of quantum black holes, and various attempts have been made to extract lessons that could apply to more realistic models. Partly based on some of these analyses, arguments have been made that the details of black hole evaporation – even at an early stage – depend on Planck scale physics, which would then play an essential role in the resolution of the black hole paradoxes. Indeed, one interesting line of argument has claimed that some of the peculiar properties of string theory – like the tendency of strings to grow when probed over short times – are just what is needed to resolve the paradoxes of quantum black holes.

In addition to the study of purely classical solutions of string theory, examples have been found (with low dimensional target spaces) in which the *quantum* theory is soluble. The *matrix model* techniques for finding these solutions grew in part from methods that were originally developed to understand the $1/N$ expansion of QCD. The $1/N$ expansion is of interest because it is widely suspected to offer the best hope for better understanding of those important aspects of QCD (quark confinement, mass generation and chiral symmetry breaking) for which there is only very limited analytic understanding. Techniques of the $1/N$ expansion have been used to make possible exact quantum treatments of special string models. Perhaps these will one day play a role similar to the Schwinger model of two dimensional quantum electrodynamics – a soluble model which despite its special nature is now understood to illustrate many mechanisms of field theory that are important in four dimensions. The success in applying $1/N$ methods to special string models also helped revive interest in the analogy between the $1/N$ expansion and string theory; this analogy, and the possible interpretation of some gauge theories – at least in two dimensions – as string theories, have been much investigated in the last few years.

Dramatic advances in understanding four dimensional strongly interacting field theories have come from another front: the study of supersymmetric models. Supersymmetric field theory may very well turn out to be the real thing at the TeV scale (though if so the parts of the theory that survive down to that energy scale may well be weakly coupled, except of course for QCD). Whether that is so or not, use of the supersymmetric case as a test case has led to much greater progress in understanding four dimensional gauge theory dynamics than has been obtained (from any point of view) at any time since the late 1970's. The phenomena that are now under control – such as composite massless fermions, confinement and chiral symmetry breaking via condensation of magnetic monopoles, baryons as solitons, the dynamical role of electric-magnetic duality in four-dimensional field theory – are not special to supersymmetry, but at present supersymmetry is needed to bring them under control. Some of the ideas in this work are known to have interesting extensions to or analogs in string theory, but the recent developments have mainly involved analyses of the field theories.

Partly stimulated by the progress with gauge theory dynamics but in equal part because of the potential phenomenological importance, there have also been renewed and reinvigorated studies of dynamical supersymmetry breaking, which might be relevant to physics at the next generation of accelerators either directly (if the mechanism of spontaneous supersymmetry breaking is manifest at a TeV or so) or indirectly (if supersymmetry is dynamically broken in a hidden sector). Both new mechanisms of supersymmetry breaking and new ways to use old mechanisms have been developed.

4 Theoretical Challenges

Theoretical particle physics is at the present time in a very unusual state. On the one hand, the standard model, developed in the 1960's and 1970's, has provided an extraordinarily successful theory of the electro-weak and strong interactions that has passed many precise tests over the last twenty years. This is a great theoretical triumph. On the other hand, the very success of this theory has made it clear that its scope is limited. It cannot address nor answer most of the questions posed above. Experiments have not yet come to the aid. Thresholds for new and relevant data seem to be beyond reach of existing accelerators, though perhaps very close.

As a consequence of these developments, many theorists have proceeded to study speculative new theories which do offer the potential to address and answer the questions posed by the success of the standard model, yet are too undeveloped to provide precise predictions. Some of these new ideas, in particular string theory, offer a tantalizing vision of a fundamental revolution in the conceptual framework of physics and the possibility of a truly unified and totally predictive theory of gravity and matter.

Under these circumstances a rather unhealthy rift has developed in the community of theoretical physicists, between the more phenomenologically oriented theorists and those who are engaged in more theoretical, formal, speculation. This rift threatens the vitality of theoretical particle physics. This danger is made more severe by the fact that new clues from experiment may be some years off and that the new thresholds suggested by current speculation are difficult to reach. It is imperative that in these difficult times, as experimental particle physics is being subjected to increasing fiscal constraints, that theoretical particle physics remain as healthy as possible. Therefore we feel it important to address this issue.

To begin with, we feel that it is crucial that the community of particle theorists show more tolerance towards the attitudes and approaches of others. More communication and interaction is required between the phenomenologists and the speculators. One can aim to bridge the gap through conferences that attract a mixed audience, but most important is a change of attitude.

We urge our formal colleagues not to ignore experiment and phenomenology. Present day experiments do not provide much help in solving string theory–but that can change and it can change overnight. Unless you continue to follow current phenomenology and experiment you will not be in a position to take advantage of the new experimental discoveries when they do take place. One piece of experimental data is often more valuable than dozens of technical and formal advances.

We also urge them not to ignore the many interesting and difficult theoretical problems that remain within the standard model. These are not only interesting and important problems, but the ideas and methods that are developed to solve them are often useful in other areas of physics, even in string theory. For example, the development of soluble toy string theories based on matrix model methods was a direct outgrowth of old attempts to solve QCD.

And we urge our more phenomenological friends not to ignore the more formal and speculative parts of our field. They should remember that what appeared to be pure speculation and formal theory in the past has now become enshrined in the standard model. It is a mistake for them to ignore the hints that speculative, yet rich and evocative, theories can provide for those interested in inventing new phenomenology.

Contrary to claims that are sometimes heard, speculative theories such as string theory can be disproved. String theory, for instance, risked being eliminated in the early 1980's because it appeared to predict that the weak interactions would conserve parity; it survived only because of unexpected theoretical advances. While string theory survived in this perilous way, other ambitious theories have gone by the wayside. For example, the more ambitious versions of supergravity and Kaluza-Klein theory have been all but excluded by combinations of theoretical and experimental difficulties. The very fact that string theory and the supersymmetric standard model have survived while other theories have been excluded is a hint of their vitality. The history of physics is filled with experimental and theoretical surprises and it is foolish to claim that a theory with the richness of string theory cannot be tested.

Talented young people should not be discouraged by gloomy claims occasionally made about the state of theoretical physics. The problems are great, but the opportunities are great also. Particle physicists are grappling with wonderful questions and marvelous, mysterious ideas. The best and the brightest continue to be attracted to particle physics because, even in these difficult times, it addresses the most fundamental questions. There is every possibility of new synthesis in coming years and what looks like calm may be the eye of the storm.