

INTRODUCTION

Supersymmetry is a hypothetical symmetry of the basic laws of nature. It proposes that the basic laws are symmetric under interchanging bosons and fermions in the appropriate manner. We do not yet know for sure that nature is actually supersymmetric at the scale of weak interactions, but there is considerable indirect evidence that it is.

One of the remarkable things about supersymmetry is that it was not invented for typical reasons. Most physical theories are invented in response to a puzzle, or to explain data, or to improve the consistency of previous theories. Yang-Mills theories are one exception – they were an extension of Abelian gauge theories to non-Abelian ones, and have turned out to be of great importance for understanding nature. Supersymmetry was also an extension, found ^a in the early 1970's both by examining certain field theories and also by trying to understand if a boson-fermion symmetry could be consistent with relativistic quantum field theory. The key point for our purposes is that supersymmetry was not invented to solve any puzzle or explain any observation.

Over about a decade after the discovery of supersymmetry, it was slowly realized by a number of people that it had the potential to solve an astonishing number of major physical problems. Even though it was not invented to do so, supersymmetry could provide a solution to the hierarchy problem (this and other terminology are clearly explained in Steve Martin's fine pedagogical Chapter), could provide a derivation of the Higgs mechanism (which was technically satisfactory for the Standard Model (SM) but lacked a physical basis there) and in the process successfully predict a heavy top quark in the early 1980's, allowed the unification of the SM forces (that supersymmetry was needed to achieve this was confirmed in the early 1990's, a decade after it was predicted), provided a connection between the SM forces and gravity, and provided a candidate for the cold dark matter of the universe (the lightest superpartner (LSP)) before astronomy demonstrated the need for non-baryonic cold dark matter. All of this was in place by the early 1980's.

More recently, it has been recognized that the scalar potential of supersymmetry, the potential energy from the scalars and their interactions, provides the potential energy that determines the course of inflation(s), and the scalars themselves can be the inflaton(s) (see Randall's chapter). The parameters in that scalar potential also determine how scalars will behave at colliders, and can affect neutrino masses, baryogenesis and other phenomena. The study of fermion masses becomes a serious research problem in a supersymmetric frame-

^aThe history of supersymmetry is complicated and interesting. Rather than treat it superficially here, I will not address it.

work, and supersymmetry affects our approach to proton decay, baryogenesis, and CP violation. It also suggests approaches to understanding the cosmological constant. Given all of these successes and opportunities, it is clear why so many physicists expect the world to be supersymmetric.

Is nature actually supersymmetric on the electroweak scale? Although that is a statement about the theory that describes nature, the way people will finally be convinced is by the explicit detection of superpartners of the SM particles. Contrary to what is sometimes stated, it is not surprising that superpartners have not yet been detected. First, theoretically all particles can get masses from two sources, the breaking of the electroweak symmetry and the breaking of supersymmetry. One set of particles only gets mass from the electroweak breaking, and all of those except one (the Higgs boson) have been observed. The masses of all those particles would vanish if the electroweak symmetry were restored. It is entirely reasonable that the particles that get mass from both sources should be somewhat heavier and not yet observed. The one exception, the Higgs boson, is very difficult to produce and to detect at hadron colliders – it should be observed at LEP soon or at FNAL in a few years. Second, phenomenologically there are no general bounds on superpartners masses that make the non-colored ones heavier than M_W , or colored ones heavier than M_{top} , so searches have not yet reached levels where one might have expected to find the superpartners. (All published limits depend on guesses for parameters and models.)

It would be very nice if clean, unambiguous experimental signals could appear one day. But a little thought tells us that is unlikely — probably impossible. Consider colliders. At least until the LHC, which is unlikely to produce its first paper relevant to supersymmetry until about a decade from now, what will happen is that as energy and/or luminosity increases at LEP and FNAL a few events of superpartner production will occur. Perhaps such events have already occurred. Each event has two escaping LSP's, so it is never possible to find a dramatic Z -like two body peak, or even a W -like peak with one escaping particle. Even worse, often several channels look similar to detectors so simple features can be obscured. And usually there are SM processes that can fake any particular signature, as well as ways to fake signatures because detectors are imperfect.

Thus to make progress it is essential to proceed with limited amounts of incomplete information. Without theory input it is entirely possible that signals would not be noticed, hidden under backgrounds since there was no guide to what cuts to use (an example is discussed below). Further, a particular signal might be encouraging but not convincing – only when combined with other signals that were related by the theory but not directly experimentally

could a strong case be made.

What about information from decays? In the best cases, such as $b \rightarrow s\gamma$, where there is no tree level decay, the SUSY contribution could be comparable to the SM one, say a large effect of order 30%. Then to get a significant effect the combined errors of the theoretical calculation of the SM value and the experiment have to be below 10%. Only $b \rightarrow s\gamma$ of known decays can approach that (see Chankowski and Pokorski's chapter); presently the theoretical error is at about that level, and the experimental error about 20%. At best, in a couple of years this could provide evidence of new physics. If so, taken alone several interpretations would be possible, but with theoretical input it could be combined with collider data and other decays to determine which interpretation was consistent.

When there is a tree level SM contribution, such as for $R_b = \Gamma(Z \rightarrow b\bar{b}) / \Gamma(Z \rightarrow \text{hadrons})$ the SUSY effect has to be a loop and can be $\sim \frac{1}{2}\%$, so experimental errors have to be several times smaller. Just from statistics that requires $\sim 10^6$ events, which is unlikely. Sometimes several decays are related in a particular model, in which case the combined predictions can be tested and the results are somewhat more significant (that is the situation for $b \rightarrow s\gamma$ plus R_b plus α_s). Further input could come from proton decay to channels favored by SUSY, from occurrence of decays forbidden in the SM such as $\mu \rightarrow e\gamma$ or $K \rightarrow \mu e$, from neutron or electron electric dipole moments, from non-SM CP violation, or other rare phenomena.

If we can discover superpartners before LHC, it will be necessary to proceed with fragments of information, check their consistency, make predictions to test them, and slowly build a case. We will see below that there are things to work with (which need not have happened – most fluctuations from the SM could never be interpreted as SUSY). The first goal, of course, is to establish that superpartners indeed exist at the EW scale. Given that, then the goal becomes the measurement of the parameters of the Lagrangian, so that theories implying a particular Lagrangian can be tested, and the form the Lagrangian takes will suggest how SUSY is broken. Measuring the basic parameters will be difficult or impossible at LEP and hard at FNAL, but should be feasible at LHC combined with a lepton collider with polarized beams. For example, $\tan\beta$ cannot be measured at LEP.

The way superpartners will behave — particularly their experimental signatures — is largely determined by the quantum numbers of the LSP. Theoretical and phenomenological arguments probably combine to imply the LSP is either the lightest neutralino (the superpartners of γ , Z , and the neutral Higgs fields of supersymmetry can quantum mechanically mix — the mass eigenstates are “neutralinos”) or the gravitino. If it is the lightest neutralino over

much of the parameter space it turns out to be mainly bino (the partner of the $U(1) B_\mu$ gauge field), or in a smaller but robust part of the parameter space, higgsino, a particular combination of partners of Higgs fields, approximately the partner of the Higgs boson.

Almost all of the studies of supersymmetric models can be classified according to what is the LSP, and fall in three categories, as show in Table 1. A fourth category is “unstable LSP”; see Dreiner’s chapter. On the left several criteria are listed that are often used to compare and test models. The first world listed has a light gravitino LSP ($\tilde{G}LSP$), the second an LSP that is mainly bino ($\tilde{B}LSP$) and the third mainly higgsino ($\tilde{h}LSP$). The implications for SUSY-breaking and experimental signature are very different for the three cases – it will be easy to recognize which is being observed once one is detected. ($\tilde{G}LSP$) corresponds to gauge-mediated SUSY-breaking, and the other two gravity-mediated SUSY-breaking.)

In a $\tilde{G}LSP$ world there is a small window for a signal at LEP but no phenomenological reason to expect one there. If the CDF event were interpreted as evidence for a $\tilde{G}LSP$ world, which is difficult given constraints but not excluded, many such events would occur at FNAL after the collider starts running again in 1999; otherwise there is only a small window at FNAL. Such a world could probably be detected at LHC or a lepton collider about 2010.

In a $\tilde{B}LSP$ world there is no evidence today for sparticles, and all hints must disappear (no more $ee\gamma\cancel{E}_T$ events at FNAL; $BR(b \rightarrow s\gamma) \rightarrow SM$, $R_b \rightarrow SM$, $\alpha_s^{\Gamma z} - \alpha_s^{\text{other}} \rightarrow 0$; no future excess of $\gamma\gamma\cancel{E}$ events at LEP; baryogenesis not at EW scale; etc.). There are small windows at LEP and FNAL but no phenomenological reason for sparticles to be there. Such a world probably be detected at LHC or a lepton collider about 2010.

In a $\tilde{h}LSP$ world sparticles may have already been observed. Confirmation will occur at LEP once $50 \text{ pb}^{-1}/\text{detector}$ at $\sqrt{s} \gtrsim 190 \text{ GeV}$ has been accumulated or before. In my short chapter the hints for an $\tilde{h}LSP$ world and some of the tests are described in a little more detail.

Once superpartners are detected it will be a delightful challenge to extract physics information from the raw data. As discussed above, often signals will come from several channels. The relationship of what is observed by experimenters to the masses and couplings of the superpartners will be complicated and nonlinear, and the relationship to the parameters of the effective Lagrangian in the electroweak symmetry basis is even more difficult to extract. To connect with theoretical work it is necessary to measure $\tan\beta$, μ , and the soft-breaking masses, which are only indirectly related to the particle masses.

Actual measurements of effects of superpartners will produce cross sections and distributions, excesses of events with some set of particles such as gammas

Table 1.

Evidence/Criteria	$\tilde{G}LSP$	$\tilde{B}LSP$	$\tilde{H}LSP$
Absence of FCNC	Yes, if messenger scale low	Mechanisms exist, but don't know if they are applicable	
Number of parameters	Presently all about same – $\tilde{G}LSP$ somewhat more than others now, but will be more predictive after squarks and sleptons observed.		
prompt γ s	all events or none	no	events with 0,1, and 2
trilepton + \cancel{E}_T events	no	yes	no
CDF $e^-e^+ \rightarrow \gamma\gamma\cancel{E}_T$	maybe	no	yes
$R_b, BR(b \rightarrow s\gamma), \alpha_s$	maybe $bs\gamma$ no for R_b, α_s	no	yes
LEP $\gamma\gamma\cancel{E}_T$ events	no	no	yes
Cold Dark Matter	not LSP	ok	yes
\tilde{g} , light \tilde{t} at FNAL?	no	no	ok
EW baryogenesis	no	no	ok
$m_{h^0} \lesssim M_Z$	no reason	no reason	yes

and perhaps with missing energy. They do not produce measurements of the masses or couplings of superpartners, and determination of the soft-breaking parameters or μ or $\tan\beta$ is even less likely. Most distributions get contributions from several processes as well. How can we proceed to extract the physics parameters of interest from data in such a nonlinear situation?

Most analyses have proceeded by assuming a model, and perhaps also assuming values for some parameters, until the problem is reduced to a simpler one. That is of use since there is no guarantee a solution can be obtained, so if a consistent solution can be found it may be a physical one.

In fact, the general problem has been addressed and a procedure (“carving”) given¹ to extract the sparticle masses and/or the parameters of \mathcal{L}_{EFF} . Every measurement provides information. There is an excess of events at a

certain cross section level in one process, none in another. The optimum procedure is to randomly select values for all parameters, calculate all observables, and discard values of parameters that give observables in disagreement with data. While that sounds like a big task it is not so hard in practice since any given observable only depends on a few parameters. Obviously to determine N parameters one will need at least N observables sensitive to the parameters. Also, often it is easy after a little thought to put limits on parameters that reduce the size of the problem. This method could already be used to get general limits on sparticle masses, though it has not been used much.

As data about superpartners is increasingly available, more and more of the parameters of the effective Lagrangians \mathcal{L}_{EFF} of the theory at the electroweak scale will be measured. Constraints from rare decays, CP violation, baryogenesis etc. will be included. Then, assuming the theory to be perturbative to the scale where the gauge coupling unify, the effective Lagrangian at that scale will be calculated by using renormalization group equations. It is not necessary (nor expected) that there be a desert in between, but only that the theory be perturbative. Intermediate matter and scales are expected. There will be consistency checks that allow the perturbativity to be confirmed. Since constraints occur at both ends it is not just an extrapolation. Unification may or may not involve a unified gauge group.

String theory, on the other hand, starts somewhat above the unification scale, at the Planck scale. If the way to select the vacuum was known, and also how SUSY was broken (perhaps once the vacuum is known the latter will be determined), then the \mathcal{L}_{EFF} could be predicted, and compared with the \mathcal{L}_{EFF} deduced from data. In practice I expect it to be the other way, as it has been throughout the history of physics – once we know the experimental \mathcal{L}_{EFF} deduced from data the patterns of parameters will be recognizable and will tell someone how SUSY is broken and how the vacuum is selected. After that it will be possible to derive it from string theory. String theorists and phenomenologists will meet at the unification scale.

The present book is designed to fill several gaps in a coherent way. Anyone with a basic grasp of the Standard Model of particle physics, such as can be obtained from ref. 2, can learn to understand the achievements and goals and issues of supersymmetry from the initial fine chapters of S. Martin, and of K. Dienes and C. Kolda. Martin's chapter is the best pedagogical introduction to supersymmetry anywhere. (A number of chapters had to be shortened for space reasons. Original longer versions are sometimes available elsewhere, and are high quality and useful.)

Supersymmetry is an active, ongoing area of research. The chapter from Dienes and Kolda is a masterful introduction to most of the open theoretical

questions in supersymmetry — it can be viewed as a list of opportunities for theoretical researchers.

A number of chapters focus on perhaps the most important question at present — how do we detect the superpartners if they are there. The chapter on Fermilab will be particularly useful since new results will not come from Fermilab for about three years. Others look in more detail at theoretical questions. Some focus on the connections of experiment and theory, or aspects of the connection of unification or Planck scale theory with low energy data. All of the chapters have introductions that are useful to non-experts, and references that point to places to learn more, as well as summaries of the current status of their subject.

Some topics are not included in this book because they are well covered recently in what can be thought of as a companion volume, “Perspectives on Higgs Physics II”, that I edited for World Scientific. These include the supersymmetric Higgs sector properties and ways to detect the supersymmetric Higgs bosons, the general upper limit of about 150 GeV on the mass of the lightest Higgs boson that is the only quantitative upper limit that is a test of supersymmetry, the role of supersymmetry in electroweak baryogenesis, and more.

Supersymmetry is an adolescent field. The situation with supersymmetry today is actually quite analogous to the situation of the Standard Model in 1973. The theory was attractive and convincing, and all experimental evidence was indirect. The field is poised to move rapidly once data on superpartners begins to come. The main challenge will be to go from the raw data to measure the basic parameters of the theory — as I said above much is understood about how to do that, though not all.

Then the central problem of the field for theorists is to understand how supersymmetry is broken. The result of the breaking is a set of soft-breaking masses and interaction parameters. Measuring these soft parameters (which are not the masses of the physical particles) will be the central problem for experimenters. Soon after these are measured it is likely that their pattern will be recognized as one generated by a particular physical mechanism. Then the cosmological implications of supersymmetry and its connections to formulating a fundamental Planck scale theory will become the central topics of a mature field.

REFERENCES

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