Where and How to find \textit{susy}:
The auxiliary field interpretation of supersymmetry

John P. Ralston
Department of Physics & Astronomy, University of Kansas, Lawrence KS 66045

The gauge hierarchy problem found in perturbation theory is one of the main attractions for supersymmetry. Yet the quantum mechanical coupling of a low energy system to a high energy one invariably leads to perturbative instability, which is not a valid signal of dynamical inconsistency. We show by examples how perturbation theory with widely separated scales gives false results. We also identify the flaw in perturbative fine-tuning arguments. Non-perturbative features of random subsystems maintain and preserve the hierarchy in which they are embedded. After reviewing the likelihood the hierarchy problem is a perturbative fiction, we suggest a new interpretation of \textit{susy} as practical auxiliary fields. Their function is much like Feynman's gauge ghosts, developed in perturbation theory to repair illnesses of perturbation theory. \textit{susy} will be found useful when it is considered a tool of applied mathematics and data-fitting. We propose that \textit{susy} data fits should be customized to the particular experimental situations they are suited to improve, without dilution from the needless assumption that \textit{susy} must describe universal new physics. It is likely that \textit{susy} will soon be discovered a useful part of data analysis and diagnostics towards improving the understanding of the Standard Model, and possibly towards discovering what may constitute new physics after all.

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\textbf{Rationale for Supersymmetry}

\textit{a. Is susy in Trouble?} So far there are no experimental signals from the LHC favoring minimal models of supersymmetry, and both experimentalists and theorists are asking whether \textit{susy} is in trouble\cite{1}. By tracing the history of arguments for supersymmetry (\textit{susy}) we find a compelling new motivation for \textit{susy}. It is possibly the first motivation that hard-boiled experimentalists would find credible. We predict that \textit{susy} will soon be confirmed as a useful element of particle physics.

Supersymmetry was invented as a way to do something beautiful with Fermions while extending the Lorentz group\cite{2}. Seeking beauty in mathematical creation is a long tradition in theoretical physics and also our first clue. The reality of physics is often un-beautiful. The stunning ugliness of the Standard Model happens to exist. There was never a viable phenomenology of unbroken \textit{susy}, which is another clue. The attempt to identify Standard Model fields as super-partners of each other failed early, which is another clue\cite{3}. Almost all \textit{susy} particles need to be heavier than Standard Model particles, meaning that most would never be directly observable, and this is a clue. (We use “directly observable” here in a strict sense, as asymptotic states of the S-matrix, for reasons soon clear.) Once they are not directly observable, the \textit{susy} particles can only modify the observable correlation functions of Standard Model fields. Given current assumptions, the least massive superpartner might possibly be observable, but once again, it might just as well not.

The well-advertised attraction of \textit{susy} is improving a technical problem with renormalization of Standard Model fields. Perturbative calculations of the Standard Model\cite{7}, and particularly quadratic divergences of Higgs fields, show that that huge extrapolations of perturbative calculations are not reliable. By remarkable and diligent technical work, it has been shown that extending the Standard Model with \textit{susy} partners yields acceptable behavior of running couplings: and mainly on this point, \textit{susy} became popular. Minor problems such as proton decay caused a general acceptance of a seemingly artificial symmetry known as \textit{R parity} conservation. The decision to give \textit{susy} particles and Standard Model particles different \textit{R}-parity eliminated most expectations to discover \textit{susy} by finding sharp bumps in invariant mass distributions. The upshot is that \textit{susy} has grown more and more un-discoverable. The predictions have largely merged into slope changes, mild shoulders and kinematically smeared endpoint effects that are difficult to distinguish from Standard Model backgrounds, which is another clue.

\textit{b. Why Trust A Quadratic Divergence to Begin With?} The motivation for \textit{susy} is clear: it stabilizes an illness of perturbation theory found in the Standard Model. If this idea was once new it is now old: it is particularly unimaginative to keep repeating it, as if any other new ideas had become taboo.

New ideas and interpretations should not be taboo. We ask whether perturbation theory itself might be giving us false guidance. Perturbation theory is not designed to handle problems with widely separated time or energy scales. The outputs might be like a faulty computer operating system that gives some correct answers, and some false ones, without reliable messages of system error. When the question is raised there is a “patch.” The patch says that perturbation theory using Feynman diagrams is the only thing known to be systematic and practical\cite{16}. Therefore it has to be our guide, take it or leave it. While that might be used cynically, we present a new idea which accepts the main motivation for \textit{susy}, along with the practicality of Feynman diagrams, and
transforming the two revises the Lorentz group algebra, hence falls into the class of supersymmetry. There is even a precedent of technically needed supersymmetry in BRST gauge ghosts, the ultimate successor to Feynman’s tinkering. We say: fine, let us accept susy’s merits and calculations at face value without the unjustified and untestable naive picture they need any physical reality. Remember: there is not a scrap of actual evidence that the Standard Model itself is either unstable or intricately coupled to high energy fields. The putative instability lies only in the unphysical ultraviolet divergences of crude approximations pushed over 14 orders of magnitude.

What is wrong with a practical calculational purpose for susy? Everyone working in susy phenomenology knows the approach cannot be ruled out. As soon as one parameter region might be extinguished experimentally, there are 150 or more parameters to go, and in many variations. In the classic sense of testing a theory by falsifying it susy is rather like strings, and untestable. What’s so bad about embracing this? The known facts support our idea: you cannot rule out a new method of applied mathematics by doing physics experiments. A mere 100 years ago the trick of representing one field by two plus a calculable Gaussian integral would have been considered futuristically sophisticated. It is no longer sophisticated, and the future physicists will be able to handle a variety of model Universes via a nearly formless, multipurpose susy auxiliary field formalism with many parameters. None of these parameters will come “from Nature.” They will all be parameters designed to rearrange perturbation theory and tune its flaws into representing the original theory.

Given abundant susy-model calculations we think the future is not far away. It needs only a modest adjustment of discovery attitude to discover susy in the near future.

I. ILLUSTRATIVE EXAMPLES

We present some examples exploring our idea with calculations.

e. First Example: Convergence We notice that integrating over a field $t$ with an action $e^{-t}$ to find the correlation function of $t^2$. Change variables to $t = zt'$, which seems harmless, but actually leads to difficulties we will discover. The result is

$$\Gamma(z + 1) = \int_0^\infty dt \, t^2 e^{-t},$$

For a physical analogy we can imagine a functional integral over a field $t$ with an action $e^{-t}$ to find the correlation function of $t^2$. Change variables to $t = zt'$, which seems harmless, but actually leads to difficulties we will discover. The result is

$$\Gamma(z + 1) = z^2 \int_0^\infty dt' \, t'^{z-2} e^{-zt'},$$

$$= z^2 \int_0^\infty dt' \, e^{z\log(t')-zt'}. \quad (1)$$
The integrand has its maximum at \( t = 1 \), and for large \( z \) is well approximated by a Gaussian with width \( 1/\sqrt{z} \). The Gaussian defines a free theory for the field shifted by its \( \text{vev} < t \geq 1 \). The Gaussian integral is the zeroth order approximation in perturbation theory. Powers of \( (t - 1) \) in the exponent are expanded in a power series around the zeroth order term, exactly as in perturbation theory:

\[
e^{\lambda \log(t')-\lambda t'} \sim e^{-z}e^{-(t-1)^2z/2}(1 + c_3(t-1)^3 + c_4(t-1)^4)...
\]

The Gaussian integral gives \( \sqrt{2\pi}e^{-z}/\sqrt{z} \) for the first term. Higher order terms produce the expansion known as the Stirling series

\[
\Gamma(1 + z) \sim \sqrt{2\pi}z^{-1/2}e^{-z}(1 + \frac{1}{12z} + \frac{1}{288z^2} + \frac{139}{51840z^3} + ...) \quad (2)
\]

Just as in field theory, this series is asymptotic, meaning it does not converge as more terms are added. For any given \( z \) only a certain maximum number of terms improves accuracy, after which adding more terms makes accuracy worse. The error in truncating the series is of order the first omitted term. Every single term increases with large \( z \) due to the \( z^2 \) prefactor: keeping any particular number of terms always produces an arbitrarily large error as \( z \to \infty \). The perturbative series of any given order contradicts those of any other order: this is the large-\( z \) Gamma function hierarchy problem, although mathematicians do not ask Nature to solve it by changing the integral.

A more subtle series approximation developed by Lanczos and cast into our language goes as follows. In the integral, the scale of the field “\( t' \)” is connected to renormalization group parameters, and should be extracted earlier. Extract the scale with a change of variables:

\[
\int dt t e^{-t} = \mu^{z+1} \int \frac{dt}{\mu} \left( t/\mu \right)^z e^{-t/\mu}.
\]

Now \( \Gamma(1 + z) = A(z, \mu)B(z, \mu) \) where \( A(z, \mu) = \mu^{z+1} \) and \( B(z, \mu) \) is the integral. The left hand side predicts \( (\mu \partial \Gamma/\partial \mu) = 0 \) because the Gamma function is “physically observable”. The \( \mu \) dependence of approximations on the right hand side creates a powerful tool. After three variable changes, and manipulation tricks characteristic of the genius of Lanczos, a series expansion emerges:

\[
\Gamma(1 + z) = \sqrt{2\pi}(z + \gamma + 1/2)^{z+\gamma+1/2}e^{-z-\gamma-1/2}A_\gamma(z), \quad (3)
\]

where \( \gamma = 1/(1+\mu) \), and \( A_\gamma(z) \) is a certain expansion with known coefficients

\[
A_\gamma(z) = \frac{1}{2} \rho_0 + \rho_1 \frac{z}{z+1} + \rho_2 \frac{z-1}{(z+1)(z+2)} + ...
\]

For completeness we list

\[
\rho_k = \sum_n C_{2k}^{2n} F_n; \quad F_n = \frac{\sqrt{\pi}}{2} \left( n + 1/2 + \gamma \right)^{n+\gamma+1/2}; \quad \cos(2n\theta) = \sum_n C_{2n}^{2n} \cos^{2n}(\theta).
\]

Unlike Eq. [4] the Lanczos series is convergent for all \( \text{Re}(z) > -\gamma \). It is not just convergent, it is rapidly convergent. Setting \( \gamma = 1.5 \) and keeping just two terms in the series gives

\[
\Gamma(1 + z) = \sqrt{2\pi}(z + 2)^{z+1/2}e^{-z-2}(0.999779 + \frac{1.084635}{z + 1}).
\]

This approximation has a relative error of less than \( 2 \times 10^{-4} \) everywhere in the right half complex plane. Keeping 7 terms with \( \gamma = 5 \) has a relative error of less than \( 2 \times 10^{-10} \) over the same region.

There are similar results for an infinite number of free parameters \( \gamma \). What is even more impressive is found by taking the "ultraviolet cutoff" \( \gamma \to \infty \). The series simplifies and the Lanczos limit formula is

\[
\Gamma(1 + z) = \lim_{\gamma \to \infty} \gamma^z + \int \frac{\lambda}{2} - e^{-1/\gamma} \frac{z}{z+1} - e^{-4/\gamma} \frac{z-1}{(z+1)(z+2)} + ..., \quad (4)
\]

The result is exact everywhere in the complex plane. And so it is very intelligent to introduce non-existent variables and manipulate their non-existence to improve approximations.

\[ f. \text{ Second Example: False Signals from Heierarchy} \]

Consider the Hermitian eigenvalue problem

\[
(H_0 + \lambda V)|\psi\rangle > n = E_n|\psi\rangle > n, \quad \langle H_0 + \lambda V|\psi\rangle > n = E_n|\psi\rangle > n,
\]

where the eigenvalues \( E_n^{(0)} \) and eigenstates of \( H_0 \) are known. Rayleigh-Schroedinger perturbation theory proposes a series expansion

\[
E_n = E_n^{(0)} + \lambda V_{nn} + \lambda^2 \sum_m V_{nm} \frac{(1-E_n^{(0)})}{E_m^{(0)}-E_n^{(0)}} V_{nn} + ... \quad (4)
\]

The matrix elements \( V_{nm} = < n|V|m > \) are evaluated in the zeroth order states. Cases where \( \lambda \gg 1 \) are called "strong coupling" and the series is generally recognized as worthless. We expect readers recognize that field-theoretic perturbation theory has just the same character, buried many degrees of freedom. It is not so well recognized that the interaction of a high-energy system with small coupling constant \( \lambda \ll 1 \) is also a "strong coupling" problem, if the limit of "high energy" is pushed far enough.
To explore this, let the eigenvalues of \( H_0 \) be of order "one" in our units, and acceptably close to the exact "small" eigenvalues of \( H \). Let the exact \( H \) have some "large" eigenvalues of a scale \( E_{\text{gut}} \gg 1 \). Let the small and large scale systems communicate by matrix elements \( V \). Consider the second-order expression of Eq. \[ \Delta E_{(2)} \sim \sum_{nm} \lambda^2 \frac{E_{\text{gut}}^2}{E_n^{(0)} - E_{\text{gut}}} \sim \lambda^2 E_{\text{gut}} \]

Unless something special prevents it, high energy sectors treated in perturbation theory tend to push high energy into lower energy systems. But is that phenomenon reliable?

What is reliable depends on the order of limits. Perturbation theory may be well-motivated for \( \lambda \to 0 \) at \( E_{\text{gut}} \) fixed. Yet the series expansion fails for \( \lambda = \text{fixed} \ll 1 \) when \( E_{\text{gut}} \to \infty \). The energy hierarchy problem has limit-interchange features which make it unsafe to ever take \( E_{\text{gut}} \to \infty \).

Some calculations illustrate this in more detail. Consider \( N = 20 \), and let the exact mass spectrum have 18 small eigenvalues of order 1 and 2 large eigenvalues of order \( E_{\text{gut}} \). Treat \( E_{\text{gut}} \) as a free parameter, and compute the first and second order perturbative predictions for its eigenvalues. Fig. 1 shows the results. (The vertical scale in the figure is logarithmic.) The perturbative energies as a function of \( E_{\text{gut}} \) are spread over the whole range \( 0 < E < E_{\text{gut}} \) for almost all states. This is a "hierarchy problem." It is also a very poor representation of the exact eigenvalues shown in the figure, which always consist of 2 large and 18 small numbers.

In a complementary study the complementary high-energy block is diagonalized. We do this because diagonalizing the largest matrix elements might be a better procedure than neglecting them. Fig. 2 shows that the perturbative calculation is relatively good. The number of large eigenvalues is trivially correct, and the numerical values are good. The flow of low energy to high energy in perturbation theory causes no problem for high energy. Perhaps future physicists will do something intelligent with the high energy sector that would allow easy hierarchies moving down. Not surprisingly, the small eigenvalues develop errors of relative order 1: the low energy sector incorrectly gives up an arbitrary fraction of its energy, but it does not run away. Both cases suggest that the worries of instability (excessive coupling to hidden freedoms) when there is an energy hierarchy are due to bad approximations.

It is possible to complain that conditions of self-consistent perturbation theory were not considered in taking \( E_{\text{gut}} \gg 1 \). That’s not a problem invented by our analysis. It is the problem and the fact of blindly computing divergent quantities (very large numbers) in perturbation theory with \( E_{\text{gut}} \to \infty \), while staying focused on particular reference parameters called coupling constants. The perturbative limit of \( \text{couplings} \to 0 \) with \( E_{\text{gut}} = \text{fixed} \) is seldom the same as \( \text{couplings} = \text{fixed} \) and \( E_{\text{gut}} \to \infty \).

8. Third Example: Hierarchical Self-Consistency We contrast the perturbation theory with a interesting solvable model. Consider a set of \( N \) quadratically coupled
fields $\phi_i$ with action

$$S = \int d^4x \sum_i \frac{1}{2} \partial_\mu \phi_i \partial^\mu \phi_i - \frac{1}{2} m^2_{ij} \phi_i \phi_j,$$

$$= \int d^4x \frac{1}{2} \partial_\mu \phi \cdot \partial^\mu \phi - \frac{1}{2} \phi \cdot m \cdot \phi. \tag{5}$$

Just as in the Standard Model, a selected low-energy sector of this theory defines a Lagrangian $L_0$, which is solved when decoupled. The low-energy sector will be defined by using only a restricted set of low mass fields $\phi^{(0)} = P \phi$, where $P = P^2$ is a projector of a certain rank $\text{dim}(P)$. The remaining fields will be called “gut-scale” and defined by $\phi^{(\text{gut})} = Q \phi$, where $Q^2 = Q, \quad QP = PQ = 0, \quad P + Q = 1$. The full Lagrangian is

$$L = L_0 + L_{\text{int}} + L_{\text{gut}},$$
$$L_0 = \frac{1}{2} \partial_\mu \phi \cdot P \partial^\mu \phi - \frac{1}{2} P \cdot m \cdot P \phi;$$
$$L_{\text{int}} = -\frac{1}{2} \phi \cdot (Q \cdot m \cdot P - P \cdot m \cdot Q) \cdot \phi;$$
$$L_{\text{gut}} = \frac{1}{2} \partial_\mu \phi \cdot Q \cdot \partial^\mu \phi - \frac{1}{2} \phi \cdot Q \cdot m \cdot Q \phi.$$

We specify that the exact system has a hierarchy where the exact spectrum of masses falls into two groups. As before the “standard model” or “observable” sector will have small eigenvalues of order $E_{\text{small}} \lesssim 1$ in low-scale units of 100 GeV, say. The “gut” sector will have large eigenvalues of order $E_{\text{gut}} >> 1$. The “number of large (small) dimensions” will mean the number of eigenvectors associated with large or small eigenvalues. However we do not know the exact eigenvalues nor the number of small scale and gut-scale fields a priori, because we have not solved the interaction.

How shall we choose the approximate low energy projections defining $L_0, L_{\text{int}}, L_{\text{gut}}$? Since we are interested in generic features we will consider random systems. A random matrix of a given system is found by making a coordinate transformation with a random unitary operator $U_{\text{random}}$:

$$m_{\text{random}} = U_{\text{random}} \cdot m \cdot U^\dagger_{\text{random}}.$$

A random mass matrix of a given system is defined by $m_{PP} = P \cdot m_{\text{random}} \cdot P$.

The matrix $U_{\text{random}}$ will be distributed by the invariant Haar measure using standard numerical codes.

We now come to the root of the perceived hierarchy problem. Random diagonal and off-diagonal matrix elements of systems with large hierarchies are large. Fig. 3 shows a three-dimensional plot of the matrix elements $m_{ij}$ of a random $100 \times 100$ mass matrix. One eigenvalue $\lambda_{\text{gut}} = E_{\text{gut}} = 10^3$; the other 99 eigenvalues are random numbers between 0 and 1. A single large eigenvalue is sufficient to cause the matrix elements to be distributed over the whole energy range with a width $\sigma_m \sim E_{\text{gut}}/N$. The off-diagonal distributions are centered on zero, while the diagonal elements are centered on $E_{\text{gut}}/N$, which can be anticipated by considering $\text{tr}(m) \sim E_{\text{gut}}$. The arbitrariness of choosing an appropriate “low energy subsystem” creates concerns of naturalness and fine tuning. When the hierarchy is large it appears that any sub-matrix with self-consistent small eigenvalues must require a great deal of fine tuning. The lore of fine-tuning suggests we must choose the projector $P$ very carefully to get small eigenvalues. Once chosen, the lore maintains the subsystem is either kept from mixing with repeated fine-tuning, or protected by profound symmetries.

Those worries are false, and they come from false calculations of perturbation theory. A example will illustrate this. Consider a system with the very large dimension 7 (to accommodate typesetting). Let the exact mass eigenvalues be six random numbers between 0 and 1 (observable sector), and one large number of order $10^3$ (gut sector.) The actual eigenvalues selected were $(0.586, 0.231, 0.219, 0.161, 0.119, 0.109, 0.999)$. Generate a random real matrix having these eigenvalues:

\[
\begin{pmatrix}
80.7 & -24.48 & 165.6 & 67.91 & 115.5 & 165.7 & -21.58 \\
-24.48 & 7.667 & -50.22 & -20.6 & -35.01 & -50.13 & 6.495 \\
165.6 & -50.22 & 340.8 & 139.8 & 237.6 & 341. & -44.55 \\
67.91 & -20.6 & 139.8 & 57.52 & 97.5 & 140. & -18.33 \\
115.5 & -35.01 & 237.6 & 97.5 & 165.9 & 237.8 & -31.08 \\
165.7 & -50.13 & 341. & 140. & 237.8 & 341.7 & -44.77 \\
-21.58 & 6.495 & -44.55 & -18.33 & -31.08 & -44.77 & 6.148
\end{pmatrix}
\]

Choose the low energy sector to be 4 dimensional, because we want 3 generations of low energy, plus one window into TeV-scale new physics. Yet many matrix elements are large. How will we find an appropriate
low energy subspace without searching through every possible subspace?

The answer lies in common experimental self-consistency. Ask an experimentalist to arbitrarily select the upper-left $4 \times 4$ block as a trial low-energy subspace. The eigenvalues are 0.300463, 0.183553, 0.118482, 486.088. Note the hierarchy: somehow the arbitrarily selected sub-matrix has 3 small eigenvalues, without any “fine tuning”.

The result appears to be a fluke. Choose the lower-right $4 \times 4$ block. It has eigenvalues (0.366832, 0.194968, 0.142922, 570.548). Note the hierarchy.

Repeat this experiment 10,000 times, with $E_{\text{gut}} = 999$ fixed, while using random small eigenvalues distributed over the interval 0-1. (If not specified otherwise, “random” numbers come from a flat distribution.) Save the eigenvalues of an arbitrarily selected $4 \times 4$ block each time. Every single run shows a hierarchy with 3 small eigenvalues and one large one. In more detail: the mean eigenvalues are (0.3, 0.5, 0.7, 570), with standard deviations (0.15, 0.15, 0.15, 205). The $p$-value probability to find any large eigenvalue less than 100 is about $6 \times 10^{-3}$. There were no events with any of the three small eigenvalues greater than 1.

There is nothing special about $4 \times 4$ projections of $7 \times 7$ matrices and the same phenomenon is found in thousands of examples. The mathematical literature on random matrices is dominated by finding invariants (eigenvalues), given random matrix elements. For physics we need the inverse problem of characterizing given matrix elements that represent couplings, given invariants. The theory of eigenvalue hierarchy has not been explored this way before, and it is fascinating[9]. In brief, hundreds of experiments led to two remarkable regularities:

- We find an empirical conservation law of large eigenvalues, which is that (1) the number of large eigenvalues is conserved on almost any subspace, and (2) their magnitudes scale like $E_{\text{gut}}$. The exact large eigenvalues can even be predicted in a statistical sense from the eigenvalues on a subspace. Fig. 4 shows the correlation of subsystem eigenvalues with exact eigenvalues.

- There is a more detailed “scaling law” of spectral similarity. Let $\zeta = \dim(P)/\dim(m)$ be the ratio of dimensions reducing to the subsystem. Then ordering the eigenvalues by size, and normalizing the sum to one, the exact spectrum $\lambda(k)$ for state $k$ “tries to be” reproduced in a scale-similar way on the small system with eigenvalues $\lambda_{\zeta}(k)$:

$$\lambda_{\zeta}(k) = \zeta \lambda(\zeta k). \quad (6)$$

Eq. 6 is an approximate fact. It is violated for small numbers of eigenvalues by the law of conservation of large eigenvalues.

Our proposal to explain these observations is geometric. The exact eigenvalues of $m$ are extrema of $<\psi|m|\psi>$ over normalized $<\psi|\psi>$. The eigenvalues of $P \cdot m \cdot P$ are the extrema of $<\psi|P \cdot m \cdot P|\psi>$. The locus of these expectations are generalized ellipsoids in high-dimensional spaces. An ellipsoid projected onto a subspace is an ellipsoid partaking of the dimensions found in the subspace. When there is a great hierarchy of eigenvalues on a big space, it produces much the same hierarchy on almost every subspace, or “the shadow of a needle is a needle.”

We have provided non-perturbative evidence that fine-tuning and naturalness concerns over energy hierarchies do not exist. We conclude this section by remembering that experimentalists easily diagonalize subsystems by measuring the spectra of masses and energies in the lab. The experimental low energy sector defines itself self-consistently, and without needling any dynamical conspiracy. Yet reproducing that outcome using perturbation theory absolutely needs significant theoretical re-arrangement, with integration over auxiliary fields being the most attractive method.
II. PREDICTIONS FOR TODAY’S SUSY SEARCHES

Can we use these observations to do something productive in present day? We do not know where the important perturbative errors actually occur. However Nature will do the calculation exactly, so we should take our guidance from experiment. Many groups are currently fitting susy model parameters\[10\], without finding any evidence they are on the right track. I observe these fits are based on the needless assumption susy is new physics. When we use susy as an effective theory of the Standard Model expressed with auxiliary fields, each set of susy parameters needs to be self-consistently adjusted to the particular theory defects it is designed to ameliorate. For example, the anomalous magnetic moment $g - 2$ of the muon\[11\] provides a very important constraint on ordinary parameter fitting. Other strong constraints included in current studies include branching ratios\[12\] such as $B \rightarrow X_s \gamma$, $B_s \rightarrow \mu^+ \mu^-$, and $B_d \rightarrow \tau v$. Yet when fitting LHC data, where the uncertain theoretical issues lie largely in perturbative QCD, Monte Carlo simulations, unknown correlations of jet substructure, etc, it is contradictory to bring in $B \rightarrow s \gamma$, the muon’s $g - 2$, etc. In fact, there is more than one reason why the discrepancies of $g - 2$ may well lie in Standard Model physics not needing any susy corrections at all\[13\]. In recognition that $g - 2$ is suspicious\[14\], some groups are already dropping it selectively to check its effects.\[15\] Yet constraints from dark matter direct detection and relic abundance pose further barriers that may be entirely specious, given the full range of unknowns. With the new interpretation, I propose dropping from studies of LHC data all irrelevant constraints, and exploring whether susy auxiliary fields produce much better fits.

The proposal is scientifically conservative: it is more conservative, and modest, than assuming susy must exist so that Nature can take care of theoretical approximation schemes. The point of developing models is to fit data and test theories. The theory being tested is the Standard Model. The Standard Model fits so much data so well it is difficult to improve by adding parameters. We have no intention of selecting data in order to fit a model. We suggest taking an existing, standard-\(\text{LHC}\) collider physics analysis and finding if a few-parameter susy effective action will improve the fit at a statistically significant level. This idea is new, since there is no prior evidence of statistically significant improvement, and it is testable when the statistical penalties of adding parameters are taken into account. For reference, one expects on statistical grounds to improve a $\chi^2$ statistic by about one unit ($1\sigma$) by adding one parameter. If a statistic improves by an additional $3\sigma$ above expectations, then enthusiasts can get excited, while large experimental collaborations often cite $5\sigma$ improvement as their objective. If (say) adjusting two parameters known as the standard $(m_0, m_{1/2})$ set gives enough units of improvement to pass a pre-selected threshold of statistical significance, my prediction will find support.

What good is the information from an improved fit? It is first of all indefensible not to make the effort to find one. Once an improved fit is found its details will give a theoretical microscope into the possible causes. The differences between interaction rates computed in a theory is always much more dramatic before applying hadronization and acceptance factors. Then comparing simpler, pre-hadronized theory to theory will help identify what features of the Standard Model calculation need help from parameters.

Suppose susy really does exist. If it cannot be falsified, can our suggestion help truthify it? Absolutely! It is a first principle of scientific sleuthing to deal with simpler problems before taking on over-complicated ones. If a multi-sigma minimum is found in the $(m_0, m_{1/2})$ plot when omitting some of the standard but superfluous assumptions, it may well be a clue where to focus on discovery. It may also give clues where to go back and re-assess prejudices about supposedly established backgrounds and facts - such as the reliability of $g - 2$ computed in perturbation theory, which we claim is not solid. Suppose new physics exists which is not susy. Same procedure: we claim it is generally useful to use a flexible method of applied mathematics to first establish a statistically significant fit. Once a “signal” exists, the details are sorted out by identifying the theory elements that distinguish it.

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[16] Lest a claim be made that the renormalization group controls a hierarchy, and is in principle exact, there is actually little to trust in extrapolating inexact approximations of the renormalization group over many orders of magnitude.

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