Future experimental programs

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Abstract
I was asked to discuss future experimental programs even though I am a theorist. As a result, I present my own personal views on where the field is, and where it is going, based on what I myself have been working on. In particular, I discuss why we need expeditions into high energies to find clues to where the relevant energy scale is for dark matter, baryon asymmetry and neutrino mass. I also argue that the next energy frontier machine should be justified on the basis of what we know, namely the mass of the Higgs boson, so that we will learn what energy we should aim at once we nail the Higgs sector. Finally, I make remarks on dark energy.

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(Some figures may appear in color only in the online journal)

1. Introduction
The discovery of a ‘Higgs-like particle’ on 4 July 2012 was a truly historic moment in the history of science [1, 2]. Many of us in the United States watched the seminar at CERN by webcast in the midnight hours. Given that it was announced on the Independence Day of the United States, we celebrated the Higgsdependence Day in the early morning.

So far, what we have seen looks minimal. Later, a CERN announcement made in March 2013 said it is a Higgs boson. Indeed, the newly discovered particle looks very much like the Standard Model Higgs boson. We have been after this particle ever since 1933 when Fermi wrote his theory of nuclear beta decay. There, he introduced a constant $G \approx 10^{-5}m_p^{-2}$ which we now call the Fermi constant $G_F$. It corresponds to the energy scale $G_F^{-1/2} \approx 300$ GeV, and we learned from him that something is going on at this energy scale. It took a whopping 80 years to come to the point where we now have a UV-complete theory of strong, weak and electromagnetic forces with all of the parameters measured. In fact, it is a renormalizable and consistent theory that may be valid all the way up to the Planck scale. Coincidentally, even cosmology looks minimal given the Planck data [3], which suggests a minimal single-field inflation. Maybe the year 2013 will be remembered in history as the year of elementary scalars.

Despite this achievement, or rather because of it, there is a building anxiety in the community. How come we do not see anything else? Will the progress stop? There is no sign of physics beyond the Standard Model in the Large Hadron Collider (LHC) data. For a typical search for supersymmetric particles, for example, squarks and gluinos are excluded up to 1.3 TeV or so. On the other hand, the conventional arguments based on the naturalness concept suggested that we should have new particles that stabilize the electroweak scale below TeV. It appears that ‘natural and simple’ models have been excluded (figure 1). Then we have two directions to go in: the less natural, namely fine-tuned, or the less simple, namely contrived. At the same time, theorists are trying to come up with models that can evade the current experimental limits, pushing back on this problem. See appendix A for my own recent attempts.

I have to point out, however, that certain levels of fine-tuning do occur in nature. All examples I am aware of, with the glaring exception of the cosmological constant, are at most at the level of a few per mille. The current LHC limit has not quite reached that level; the next runs at 13–14 TeV may well reveal new physics as we hoped for. I will come back to this question later in this paper.

In any case, it is true that experimental limits have started to haunt theorists. Theorists used to complain that the experiments had a hard time keeping up with their new ideas. Now the tide has reversed. Theorists are being threatened by new data. I believe this is quite a healthy field!
Nonetheless, having a fully UV-complete theory of the Minimal Standard Model, now supported by the new particle that has been discovered, makes us ask the following question.

2. Is particle physics over?

On this question, fortunately, the answer is a resounding no. Since 1998, we have discovered five pieces of empirical evidence for physics beyond the Standard Model thanks to tremendous progress in experiments.

First, non-baryonic dark matter. Even though dark matter had been discussed since 1930s by Fritz Zwicky, it was not clear whether dark matter would be dark astronomical objects or hidden baryons. This issue was completely settled in 2003. The search for dark astronomical objects (massive compact halo objects (MACHOs)) excludes the possibility that the galactic halo consists solely of MACHOs between about $10^{-7} M_\odot$ and $10 M_\odot$. On the other hand, the power spectrum in the cosmic microwave background (CMB) anisotropy by the Wilkinson microwave anisotropy probe (WMAP) excludes the baryonic dark matter completely as a discrepancy between the overall matter density $\Omega_m h^2 = 0.14 \pm 0.02$ and the baryon density $\Omega_b h^2 = 0.024 \pm 0.001$ [5]. We are learning what dark matter is not, but not what it is. In fact, we know so little that the only model-independent lower limit on the dark matter mass comes from the requirement that its 'Bohr radius' in the gravitational potential must fit within the galactic scale [6]. Combined with the MACHO search, we managed to narrow down its mass from $10^{-31}$ to $10^{50}$ GeV, i.e., to within 81 orders of magnitude. Zwicky must be happy to see our progress! Thus, we need to keep our minds very open about the nature of dark matter.

The flavor oscillation of neutrinos, and hence their finite masses, is not a part of the Minimal Standard Model either, arguably the first established physics beyond the Standard Model in 1998 [7], revealing the mixing angle $\theta_{13}$. Later on, the oscillation (or rather lack of it as a result of the matter effect) of solar neutrinos [8] and oscillation of reactor neutrinos [9] pointed to the same parameter set (and the angle $\theta_{13}$) in 2002 resolving a puzzle that goes back half a century. The final mixing angle $\theta_{13}$ was discovered in 2012 [10]. Some people think it is only a minor extension of the Standard Model, but it should be emphasized that we do not yet know how it should be extended.

The accelerated expansion of the Universe came as a big surprise to all of us [11, 12]. Its cause is now called dark energy, even though we are very far away from understanding what it is. It may be cosmological constant, due to a miraculous cancellation between quantum fluctuation of the vacuum and a classical constant energy density for 120 digits. It may be some dynamical substance called quintessence. Either way, it is very difficult to understand its overall amount.

At the same time, the observed apparently acausal density fluctuations in the CMB cannot be explained by the Standard Model. The CMB photons that came from one end of the Universe have just reached us; they seem to be correlated with the CMB photons that came from the other end, when they have had no chance to meet and set up their temperatures. This is what I mean by acausal. The best explanation is that they were in fact in causal contact early on because the entire visible Universe was much smaller than a nucleus; it was later stretched to a macroscopic size by an exponential expansion called inflation. The latest Planck data strongly supports this idea [3]. We normally assume that it was caused by a scalar field called the inflation rolling slowly down the hill, but we do not know what it is, nor how it couples to the Standard Model particles.

Finally, once we accept the inflationary paradigm, the cosmic baryon asymmetry $n_b/n_\gamma \approx 5 \times 10^{-10}$ cannot be assumed to be the initial condition of the Universe. This is because the enormous exponential expansion (normally assumed to be more than $e^{50}$) wipes out any pre-existing asymmetry. This implies that the baryon asymmetry needs to be created after the inflation by a microphysical process. On the other hand, the CP violation in the Standard Model is now known to be incapable of producing enough baryon asymmetry. This is because that we now have understood the known CP violating phenomena by the Kobayashi–Maskawa theory thanks to the $B$-factory experiments starting in 2001 [13, 14]. This means that the Standard Model cannot generate baryon asymmetry larger than the Jarlskog invariant $J = \Delta m (\text{Tr}[Y_u^T Y_u, Y_d^T Y_d]) \approx 10^{-20}$ [15], further suppressed by small efficiencies or powers of coupling constants in known mechanisms.

So, it is clear that particle physics is far from over. There are at least five important pieces of data that are crying out to be explained and understood. The catch is that we do not know the energy scale of physics relevant to these mysteries. Right now we are on fishing expeditions (figure 2). In particular, we are and will continue to be looking for new phenomena and new sources of CP violation in the quark sector (LHCb, SuperKEKB, rare kaon decays), lepton sector (neutrino oscillations, neutrinoless double-beta decay and electric dipole moments) and their combination (proton decay). We try to cast a wide net, hoping to catch any interesting fish, so that we learn where the next important energy scale is. In a sense, this is what Fermi succeeded in doing; by observing rare phenomena of nuclear $\beta$-decays, which violate conservation law of neutron and proton numbers that all other known forces respect, they were caught in the net and we learned about the Fermi scale. Whatever the next energy scale beyond the Standard Model is, it plays the role of the UV cutoff of the Standard Model as a low-energy

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{schematic_constraints.png}
\caption{Schematic constraints on space of theories.}
\end{figure}
In the Standard Model Lagrangian \( \mathcal{L} \) designed by John Ellis, which has the structure as shown on T-shirts from CERN, the neutrino mass can be viewed as the Majorana neutrino mass operator.

After substituting the expectation values for the Higgs field, it is nothing but the Majorana neutrino mass operator

\[ \frac{1}{\Lambda_{\text{UV}}} L_5 = \frac{\nu^2}{\Lambda_{\text{UV}}} v^{	ext{v.l.}}. \]

In other words, the neutrino mass can be viewed as the leading order effect of the physics beyond the Standard Model!

The neutrino mass is actually a tiny effect. Any kinematic effect is suppressed by \( m^2_{\nu}/E_\nu^2 \approx (0.1 \text{ eV GeV}^{-1})^2 \sim 10^{-20} \)!
quarks clearly do need additional input, which is yet to be understood.

The idea can be extended to the sector of right-handed neutrinos by assuming that they have a hierarchy akin to those in the charged leptons or quarks, $\varepsilon^2 : \varepsilon : 1$. We take $\varepsilon \approx 0.1$. With this structure, we can randomly generate the full left- and right-handed neutrino mass matrices. Xiaochuan Lu and I identified that the Gaussian measure is the unique choice based on a certain set of criteria, and found that the baryon asymmetry comes out extremely well (figure 3 right) [20]. This is encouraging; in particular it is promising that the anarchy predicts that the distribution in the CP-violating effect would peak at $\sin \delta = \pm 1$ (or flat in $\delta$).

In fact, the CP violation in neutrino oscillation is the holy grail in neutrino experiments currently being planned and discussed. A possible CP violation (assuming no matter effect) is given in terms of a product of many factors

$$P(\nu_\mu \rightarrow \nu_e) - P(\bar{\nu}_\mu \rightarrow \bar{\nu}_e) = -16 s_{12} c_{13} s_{13}^2 s_{23} c_{23} \sin \delta \sin \frac{\Delta m_{12}^2 L}{4E} \sin \frac{\Delta m_{13}^2 L}{4E} \sin \frac{\Delta m_{23}^2 L}{4E}.$$  \hspace{1cm} (6)

It is remarkable that all factors are now found to be large enough to make this search feasible, the only unknown being the size of the CP violation $\sin \delta$ itself. Nature seems kind to us once again! It was also interesting to learn at this symposium that beyond the LBNE in the US and HyperK in Japan, there is a new discussion to use the European Spallation Symposium that would further improve the reach by 20–30%. We still have quite a bit of room for discoveries. More recently, there are discussions about a potential 100 TeV $pp$ collider with a much bigger tunnel around CERN.

However, I see a problem in arguing for the next much higher energy machine now. Given that the discovery of the Higgs boson made the theory apparently complete, and the five mysteries I discussed have not yet set particular energy scales, I do not know how we can justify the energy of the next machine. Does this mean that there is no case we can make to build another high-energy collider? Is the energy frontier dead?

It remains true that the best argument we have right now to expect new physics in the TeV range is the naturalness: would like to avoid fine-tuning between the bare $m_\gamma$ and the radiative correction (see e.g. [23] for a plot). Even though many in the community are ditching the naturalness altogether, I still take the argument seriously because it has worked many times before.

One example I always bring up is the discovery of the positron [24, 25]. In classical electrodynamics, the Coulomb self-energy of the electron is linearly divergent, $\Delta m_e c^2 \sim \frac{r_e}{\gamma}$, where $r_e$ is the ‘size’ of the electron. It would have required a fine cancellation between the ‘bare’ mass of the electron (which must be negative by the way) and the correction to yield a small mass $m_e c^2 = 0.511$ MeV. However, the discovery of the positron and quantum mechanics told us that the vacuum is always fluctuating, producing a pair of $e^+ e^-$, that annihilates back to the vacuum within the time allowed by the uncertainty principle $\Delta t \sim h/\Delta E = h/2m_e c^2$. When you place an electron in this fluctuating vacuum, it may find a (virtual) positron near it and decide to annihilate it. Then the other electron that was originally in the vacuum fluctuation is now left out and becomes a ‘real’ particle. It turns out that this process cancels the linear divergence exactly, leaving only a logarithmic divergence $\Delta m_e c^2 = \frac{3r_e}{4\pi} \log \frac{m_e}{r_e}$. Even for an electron as small as the Planck distance, it amounts to only 9% correction. The cancellation is guaranteed by a (softly broken) chiral symmetry. You can see that the naturalness problem was solved by doubling the number of particles.

The idea of supersymmetry was pushed to repeat the history. Because the Higgs boson must repel itself,
it also has a divergent self-repulsion energy $\Delta m^2_{11} \sim \lambda / r_{\text{H}}^4$. But by doubling the number of particles (namely introducing superpartners), there is a cancellation between the self-repulsion among Higgs bosons, and the induced attraction arising from the loop of higgsinos (fermionic partner of the Higgs boson). Again, the correction is down to a logarithmic divergence, $\Delta m^2_{11} \sim \frac{1}{4\pi^2} m^2_{\text{SUSY}} \log(x^{\text{MIN}})$.

In the case of the electron, new physics (positron) appears ‘early’ at the Compton wavelength $\frac{\hbar}{m_e c} \approx 400 \text{ fm}$ well before we get down to the smaller ‘classical radius of electron’ $r_e = e^2 / m_e c^2 \approx 1 \text{ fm}$ where the theory becomes fine-tuned. In another well-known case, however, nature did fine-tune it so that the discovery was delayed.

The example is cosmic background explorer (COBE) that discovered the CMB anisotropy. People expected anisotropy at the level of $10^{-5}$ so that the observed large-scale structure can be explained. But the search went on, so much so that people started writing papers questioning the inflationary cosmology itself. When COBE discovered the quadrupole moment, it was small. Actually, compared to our best prediction today based on the WMAP data, it was nearly an order of magnitude smaller than theory. This is usually understood today as a consequence of cosmic variance, namely that the quadrupole moment has only 2 numbers to be measured and hence is subject to a statistical cosmic variance an order of magnitude smaller than theory. This is usually so that people started writing papers questioning the inflationary cosmology itself. When COBE discovered the CMB anisotropy. People expected it to enter only by a finite distance (the penetration depth).

The magnetic field is short-ranged inside the superconductor. It is caused by the instability of the Fermi surface when electrons are attracted to each other by a weak force from the phonon exchange. Cooper pairs condense, making the magnetism short-ranged. On the other hand, the Standard Model does not tell us why the Higgs boson condenses in our Universe. This is not only artificial, it is unsatisfying.

There are ideas to give context to the spinless Higgs boson. There may be many siblings and relatives. The Higgs boson is just one among the big spinless tribe, one which happens to condense because of an attractive force induced by the top-quark loops. This idea is known as supersymmetry. An additional Higgs doublet is its sibling, and there are many other spinless squarks and sleptons, that are its relatives. On the other hand, it may be composite, just like spinless pions are made of spin 1/2 quarks. In this case a new dynamics would be required to bind the constituents together. Or the Higgs boson may actually be spinning, but if it does in extra dimensions we cannot see, so we perceive it to be spinless. In such a case, the Higgs boson may actually be a gauge boson or even graviton. These are all familiar ideas we discussed for solving the naturalness problem. Here I am not using the naturalness argument at all; but I still come back to a similar set of ideas, namely that there are good reasons to continue discussing these ideas.

Then what should we do? Of course, we should study this intruder as much as we can. If we look closely enough, maybe we can tell it has siblings or relatives. We may find it has a finite size. Or we may bring it back to spin in our dimensions.

Fortunately, the observed mass of 125 GeV is the best case scenario. It allows us to measure branching fractions to $b\bar{b}$, $WW^*$, $ZZ^*$, $gg$, $\tau^+\tau^-$, $c\bar{c}$, $\gamma\gamma$, $Z\gamma$, possibly even $\mu^+\mu^-$. Some of them would not be accessible if the Higgs were lighter or heavier by just a few tens of GeV. It is actually a dream case for experiments!

Looking back at the history of collider experiments, precision measurements using leptons often revealed the next energy scale; we went up there with hadrons, and we indeed found new things, which we further studied with lepton.
probes. One full cycle is the precision measurements of neutral currents in polarized electron deuteron scattering at SLAC. The measured $\sin^2\theta_W$ predicted the masses of $W$ and $Z$. SpS was built to discover them, which indeed did. After that the Large Electron Positron Collider (LEP) was built to study them precisely and we nailed the gauge sector of the Standard Model.

The next cycle starts with LEP predicting the top quark and Higgs boson masses. Tevatron and LHC were built for this purpose, and as we know, they did discover the predicted particles. The obvious thing to do next is to study them precisely to nail the top and Higgs sector at another lepton machine.

If the history is any guide, the future precision measurement of the top and Higgs sector would tell us the next energy scale we should go after. We are on a scavenger hunt. The Higgs boson discovered is a lamp post; we need to look carefully at what is under it, and hope to find a clue to the next destination.

Another reason why the precision study of the Higgs boson is exciting is that the Higgs boson may be a portal to a new sector outside the Standard Model. It may, for example, be a sector of the dark matter particle. To probe an operator $O$ (with mass dimension $d$) in the new sector, we need its coupling to the Standard Model particles. As we discussed before, all operators in the Standard Model are of dimension four, except for the Higgs mass-squared. Therefore the coupling is suppressed as e.g. $\frac{1}{\Lambda^2} \mathcal{O} F^2_{\mu\nu}$, while the coupling to the Higgs goes as $\frac{1}{\Lambda^2} \mathcal{O} H^H$. Thus the coupling to the Higgs is enhanced by $\Lambda^2$ relative to other operators. The Higgs boson may be the window to the new world.

In addition, once we build a new lepton collider to study Higgs and top precisely, we can still hope that it discovers new particles directly. It is not true that LHC excluded everything below TeV. Even a slepton of, say, 150 GeV is still allowed if it decays into a neutralino heavier than 80 GeV or so. LHC will improve limits to heavier sleptons, but not much to close the gap when their masses are close.

Given this, I would think the strategy is clear. We start with what we have got. We build a lepton collider that can study the top quark and Higgs boson precisely. This will be an evolutionary program, starting with the $Zh$ threshold, measuring branching fractions and couplings to $Z$, $W$, $b$, $c$, $\tau$, $g$, $\gamma$, even the decay into invisible particles. Then on to the $if$ threshold to study the top quark compositeness, say, going up further in energies to make use of new processes such as $WW$-fusion, $tth$ production for $\gamma_{stth}$, and multiple Higgs production for $\gamma_{hhh}$. But we should keep our eyes open to the possibility that we may also discover new particles along the way. Just in case we obtain a new piece of information on new particles from the LHC, the lepton collider should be extendable. If we do see new particles, we should have the capability of studying them in a model-independent way, and to determine their quantum numbers, spins, masses and couplings. The machine should be one that we know how to build, so that we can propose it as soon as an opportunity presents itself.

The planned International Linear Collider (ILC) fits this bill very nicely, and its scientific case was judged to be very strong in the European Strategy document adopted by the CERN Council in May (http://council.web.cern.ch/council/en/EuropeanStrategy/ESParticlePhysics.html). The technology required for this is mature, thanks to the Global Design Effort led by Barry Barish that finished the Technical Design Report this year (www.linearcollider.org/ILC/Publications/Technical-Design-Report). It is extendable, so that we can increase the energy if needed and affordable. In addition, the longitudinal beam polarization provides a crucial tool. The bottom diagrams in figure 4 show that different electron polarization has different gauge bosons in the $s$-channel. At these energies, we can neglect $m_Z^2 \ll s$ as an approximation. Then the gauge bosons exchanged are either $U(1)_{Y}$ gauge boson $B_{\mu}$, or the neutral $SU(2)_{L}$ gauge boson $W_{0}^{0}$. The right-handed electron is a singlet under $SU(2)_{L}$ (subscript $L$ stands for left-handed), and does not couple to $W_{0}^{0}$. Therefore, the $s$-channel production goes as $|g'_{Y_{f}}|^2$ and directly measures the hypercharge $Y_{f}$ of the new particle $f$. On the other hand, the left-handed electron couples to both, and the cross section goes as $|g'_{Y_{f}} + gI_{3f}|^2$. Knowing $Y_{f}$, we can determine $I_{3f}$ model-independently. One can see how much the cross sections vary depending on the beam polarization in the top plot of figure 4.

There are many studies of spin and mass measurements, which can be done even when particles decay into invisible particles. A combination of the LHC and ILC data may allow us to even compute the cosmological abundance of the invisible particle, possibly verifying that it is the dark matter of the Universe [28].

But is not ILC too expensive to be ever built? Through some miracle, many politicians in Japan are interested in hosting the ILC as a global project. They would like to open up the country to talented, intellectual people from abroad. They would like to find prestige in hosting a highly visible large international project. They also want to use the ILC to build up infrastructure, a technological base and they hope to find economic benefits. More than 20% of the Diet members signed up to support the ILC, in a group named Federation of Diet Members for Promotion of the Universe [29].

![Figure 4](image_url)
of the ILC. When Lyn Evans visited Japan in March, the prime minister Shinzo Abe agreed to meet him (www.kantei.go.jp/jp/96_abe/actions/201303/27ilchyo.html), and he said that he appreciated the significance of the ILC as ‘a dream for humankind’. His opening address in the 183rd session of the Diet mentioned advanced accelerator technology as one of the innovation areas in which he wants Japan to excel (www.kantei.go.jp/jp/96_abe/statement2/20130228siseuhousin.html). There are many industry associations actively supporting the ILC; the media is highly interested as well. And the discovery of the Higgs boson has fueled interest even further. I would think there is a high enough level of interest for the Japanese government to initiate discussions with other potentially interested countries to form an international framework for a global ILC project hosted in Japan. I am not absolutely sure, but it does not look impossible so far.

Having discussed expeditions to high-energy scales, and precision studies of the Higgs and top to identify the next energy scale(s), there are plenty of things that we can and will do in the near future in our field. However, it still leaves one question that has been haunting me.

4. Will we ever understand dark energy?

Dark energy is such a big mystery that I cannot gauge how we may ever understand it. Does this mean that it is useless to try to measure its properties precisely?

I do not know. But all I can say is that a per cent-level or better measurement is what I consider precise. If the equation of state parameter \( w = p/\rho = 0 \pm 0.01 \), I may give up and say it is the cosmological constant, accidentally small in a landscape of \( 10^{500} \) universes. But it may turn out to be \( w = 0.05 \pm 0.01 \), pointing us in a new direction. I believe that it is worth the try.

I lead a major dark energy experiment called Subaru Measurement of Images and Redshifts (SuMIRe). It combines imaging and spectroscopy on the 8.2 m-diameter Subaru telescope, a major step up from the wildly successful Sloan Digital Sky Survey (SDSS). The first stage is the approved imaging survey with Hyper Suprime-Cam for 300 nights, with nearly 0.9 billion pixels with a field of view of 1.7 square degrees. It will image hundreds of millions of galaxies.

The next stage is a spectroscopic survey with the Prime Focus Spectrograph (PFS) for (hopefully) 300 nights, with 2400 optical fibers, controlled robotically, being targeted at galaxies chosen from the imaging survey. For instance, it will yield a model-independent measurement of the evolution of the dark energy fractions as a function of the redshift (left figure 5) and provide a test of general relativity at cosmological distances (right figure 5).

We should do what we can do, and we will see what we find!

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Appendix. Pushing back on fine-tuning

Take supersymmetry. There are two issues facing the experimental data. The first one is that the mass of the discovered Higgs boson \( m_h \) is a little too high within the MSSM, which predicts \( m_h \leq m_Z \) at the tree-level. Even though the Higgs mass can be pushed up by the radiative correction as

\[
m_h^2 \simeq m_Z^2 + \frac{3}{4\pi} h_1^2 v^2 \log \frac{m_{\tilde{t}_1} m_{\tilde{t}_2}}{m_{\tilde{t}_1}},
\]

it would require a large scalar top mass, which would feed into the radiative correction to the Higgs mass-squared term

\[
\Delta m_{h_i}^2 \simeq -12 \frac{h_1^2}{16\pi^2} m_{\tilde{t}}^2 \log \frac{\Delta_{UV}}{\mu_{IR}}.
\]

Therefore, a larger physical Higgs mass in the MSSM indirectly implies exponentially worse fine-tuning in \( m_{\tilde{t}} \) between the bare parameter in the Lagrangian and the radiative correction above.

This can be avoided if there is an additional contribution to the Higgs self-coupling, such as in the massive NMSSM

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4 SuMIRe (http://sumire.ipmu.jp/en/).
5 SDSS (www.sdss.org).
This can be prevented for a large soft mass $m_S^2 \gg M^2$ as a non-decoupling effect [30], but $m_S^2$ then feeds into $\Delta m_{h_u}^2 = \left[ \frac{1}{16\pi^2} \right] \frac{\alpha_S}{\pi} \frac{M^2}{m_S^2}$, re-introducing the fine-tuning.

Together with Xiaochuan Lu, Josh Ruderman and Kohsaku Tobioka, we have come up with an idea that we call **semi-soft supersymmetry breaking** [31]. Using the NMSSM of Dirac type, $W = \lambda S H_u H_d + \frac{1}{2} M S^2$, the singlet field $S$ couples to the rest of the model only through a dimensionful coupling $M$. It can then be proven that the limit $m_S^2 \to \infty$ does not re-introduce fine-tuning (figure A.1) even though it looks like a hard breaking of supersymmetry, hence semi-soft.

The second problem with supersymmetry is its non-observation in direct searches. It is well known that a quasi-degenerate spectrum among supersymmetric particles makes the search difficult because of small $Q$-values in decays and hence small $E_T$ (see e.g. [32]). However, such a spectrum lacked theoretical motivation: in particular, why should scalars and gauginos be degenerate?

Together with Yasunori Nomura, Satoshi Shirai and Kohsaku Tobioka, I proposed that supersymmetry broken by boundary conditions in extra dimensions would automatically give the same mass to all gauginos and sfermions at the tree-level split only by loop effects [33], similar to the universal extra dimension (UED) [34]. Correspondingly, the experimental limit is weaker. A dedicated search with initial state radiation should improve the limit as in the UED case [35].

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