The Physics Landscape After the LHC

and Open Issues

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Summary

Covering the future physics landscape in a short talk is hard, so I'll talk about two limited topics:

- **The hierarchy problem**, which motivates new energy-frontier colliders. We should talk more clearly about its importance.

- **A quick summary of some possible discoveries in the coming years**, and how they might relate to the case for new energy-frontier colliders. In particular, we'll look at **electric dipole moments**, where experiment is progressing at an exciting rate.

Some big questions about small numbers

- Hierarchy problem: why is $m_W^2/M_{\rm Pl}^2 \approx 10^{-33}$?

- Strong CP problem: why is $|\bar{\theta}| \lesssim 10^{-10}$?

- Flavor: why the wide range of Yukawa couplings and of mixings, e.g. $y_e \approx 3 \times 10^{-6}$ but $y_t \approx 0.95$?

- Neutrino masses: why so small? $m_\nu \sim \frac{v^2}{10^{15}\ {\rm GeV}}$ or $m_\nu \sim 10^{-13} v$?

- Cosmological constant problem: why $\rho_\Lambda \sim 10^{-120} M_{\rm Pl}^4$?

- Matter/antimatter asymmetry: why $(n_B - n_{\bar{B}})/n_\gamma \sim 10^{-9}$?

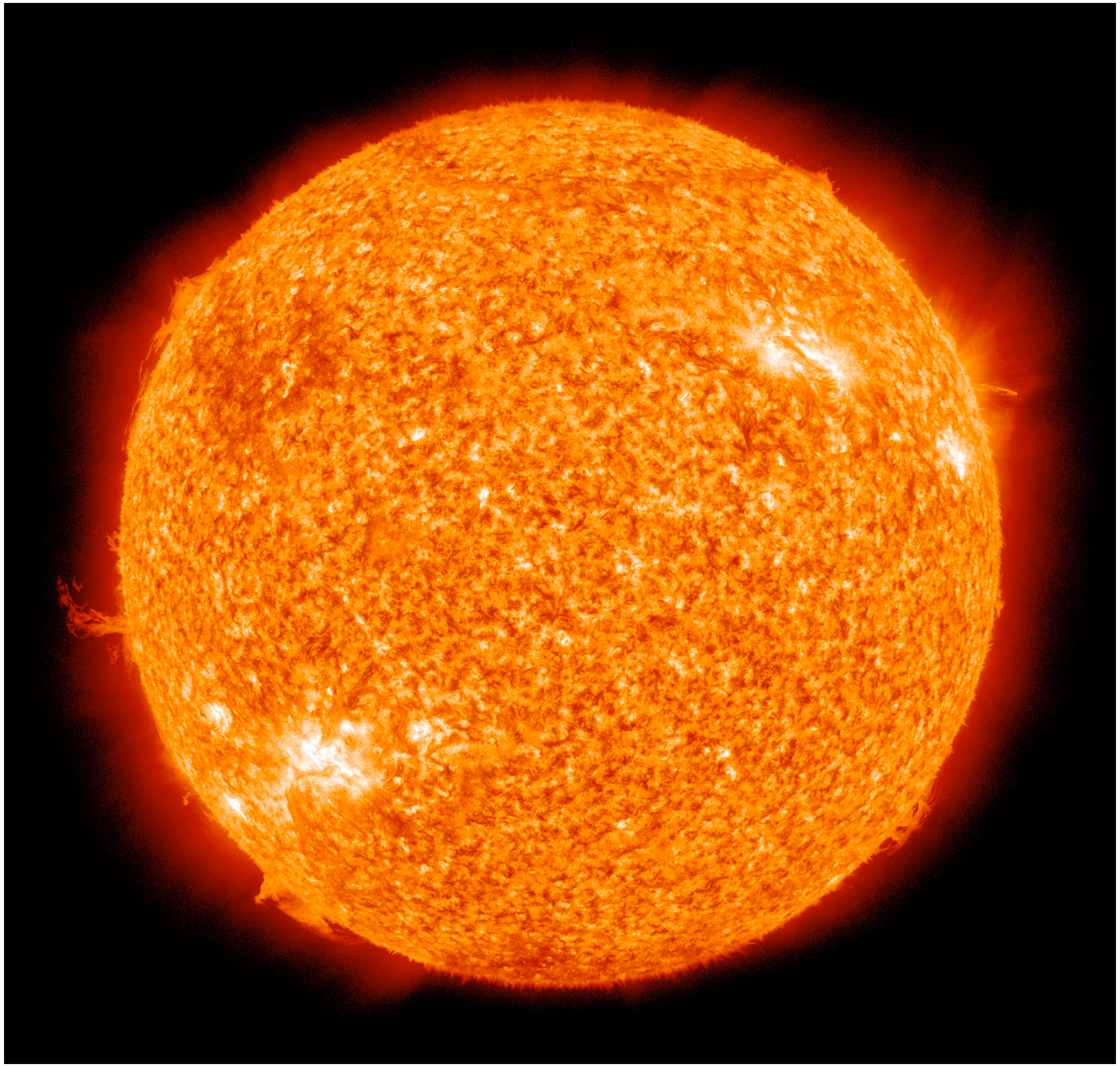
- Dark matter abundance: why $n_{\rm DM}/n_\gamma \sim 10^{-12} m_{\rm DM}/{\rm TeV}$?

- Primordial density perturbations: why $\delta\rho/\rho \sim 10^{-5}$?

Common theme: when we see small numbers, we're not satisfied until we can *explain* them in terms of some underlying mechanism.

The electroweak hierarchy

The mysterious number that best motivates new colliders is the electroweak hierarchy. We should not lose sight of how important it is to understand.



The electroweak hierarchy is not just an obscure fact about high-energy physics. It is crucial for the existence of large objects like stars and planets.

$$M_\odot \approx 2 \times 10^{30}\ \mathrm{kg}$$

$$\approx 1.1 \times 10^{57}\ \mathrm{GeV}$$

$$\approx 0.6 \left(\frac{M_{\mathrm{Pl,unred}}}{m_{\mathrm{proton}}}\right)^3 m_{\mathrm{proton}}$$

The electroweak hierarchy

A more detailed estimate, assuming a ball of hydrogen gas that is hot enough for nuclear fusion to work despite Coulomb repulsion, leads to a scaling like:

$$\frac{M_{\text{star}}}{m_{\text{proton}}} \gtrsim \left(\frac{M_{\text{Pl}}}{m_{\text{proton}}}\right)^{3} \left(\frac{m_{\text{proton}}}{m_{\text{electron}}}\right)^{3/4} \alpha^{3/2}$$

In fact, a star also cannot be too much heavier than this without collapsing.

Similar reasoning reveals that the maximum mass of a rocky planet scales like

$$\frac{M_{\text{rocky planet}}}{m_{\text{proton}}} \lesssim \left(\frac{M_{\text{Pl}}}{m_{\text{proton}}}\right)^{3} \alpha^{3/2}$$

If the Higgs VEV were near the Planck scale, the Universe would be a very different place!

[Details: V. Weisskopf, *Science* **187**(4177):605–612 (1975); Burrows and Ostriker, *PNAS* 111 (7):2409-2416 (2014).]

What is the hierarchy problem?

A good solution to the hierarchy problem should leave us feeling like we *understand* the origin of a scale in terms of some more fundamental physics.

A good example comes from QCD: we can ***compute*** the QCD scale from the gauge coupling measured at some higher energy, and it comes out exponentially small in a robust manner:

$$\Lambda_{\mathrm{QCD}} \sim M\,\mathrm{e}^{-8\pi^2/(bg^2)}$$

Or BCS superconductivity: Cooper pairing from similar running of marginal interaction. (Shankar, Polchinski)

We want something similar for the EW hierarchy. Not literally the same, but same qualitative character of allowing us to compute the scale from something more microscopic.

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At the most fundamental level, the question we want to ask is really:

where did the weak scale come from?

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Various refinements of this question, or related questions, are:

- Can we *explain* or *compute* the weak scale in terms of a more fundamental theory beyond the Standard Model?
- Are there microscopic *dynamics* that tell us why electroweak symmetry breaking happened, or that make it more likely?
- What is the shape of the Higgs potential? (Strong motivation for measuring the Higgs self-coupling.)
- Is the Higgs boson a fundamental particle, or is it composite?
- What would happen if we heated up the universe above the weak scale?

What is the hierarchy problem NOT?

The question is ***NOT***

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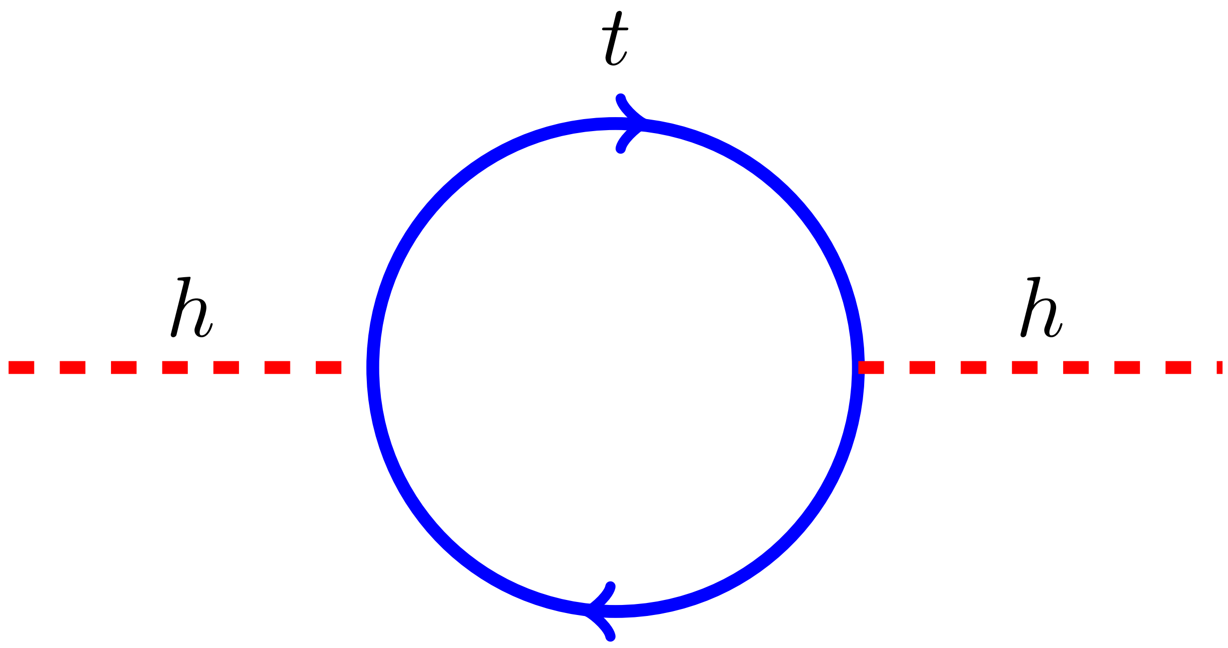
how do I regulate a loop diagram?

- The problem will *not* go away just because you like to use dimensional regularization, which has no power divergences.
- The problem will *not* go away simply because you like a different choice of “fine-tuning measure.”
- The fact that you can measure Standard Model parameters and do calculations to high precision that match data at the weak scale does *not* mean there is nothing to explain.

What not to say

Like many other people, I have given talks where, due to lack of time or wanting to focus on other points, I have just said things like:



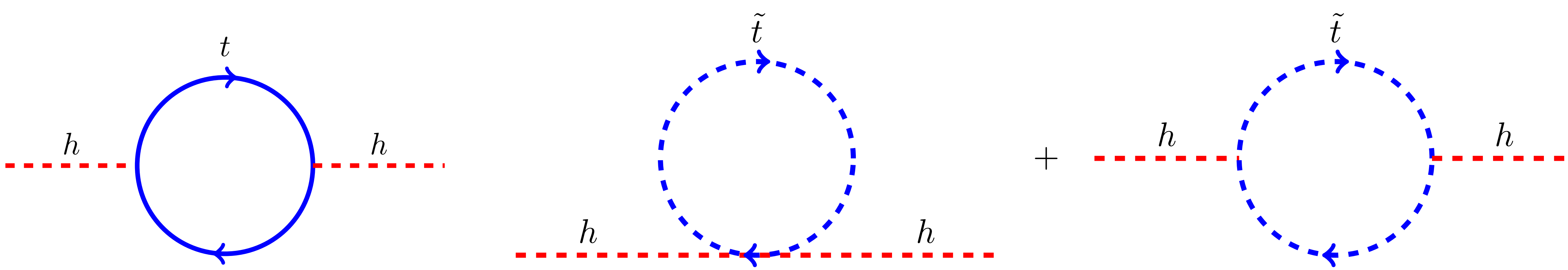
$$\delta m_H^2 \sim \frac{y_t^2}{16\pi^2}\Lambda_{\mathrm{UV}}^2$$

This diagram is quadratically divergent, so the weak scale is quadratically sensitive to UV scales. We need a low cutoff or a cancelation of this divergence.

Because then…

Some people respond "power divergences are unphysical" or "when you use the renormalized mass in a calculation, there is no problem" or any number of other things you've probably heard before.

Or maybe we are a little more careful and we say something like:



$$\delta m_{H_u}^2 = -\frac{3}{8\pi^2} y_t^2 \left(m_{\tilde{t}_L}^2 + m_{\tilde{t}_R}^2 + |A_t|^2 \right) \log \frac{\Lambda}{\mathrm{TeV}}.$$

What we have is *quadratic sensitivity* to *physical scales*.

Can we do better?

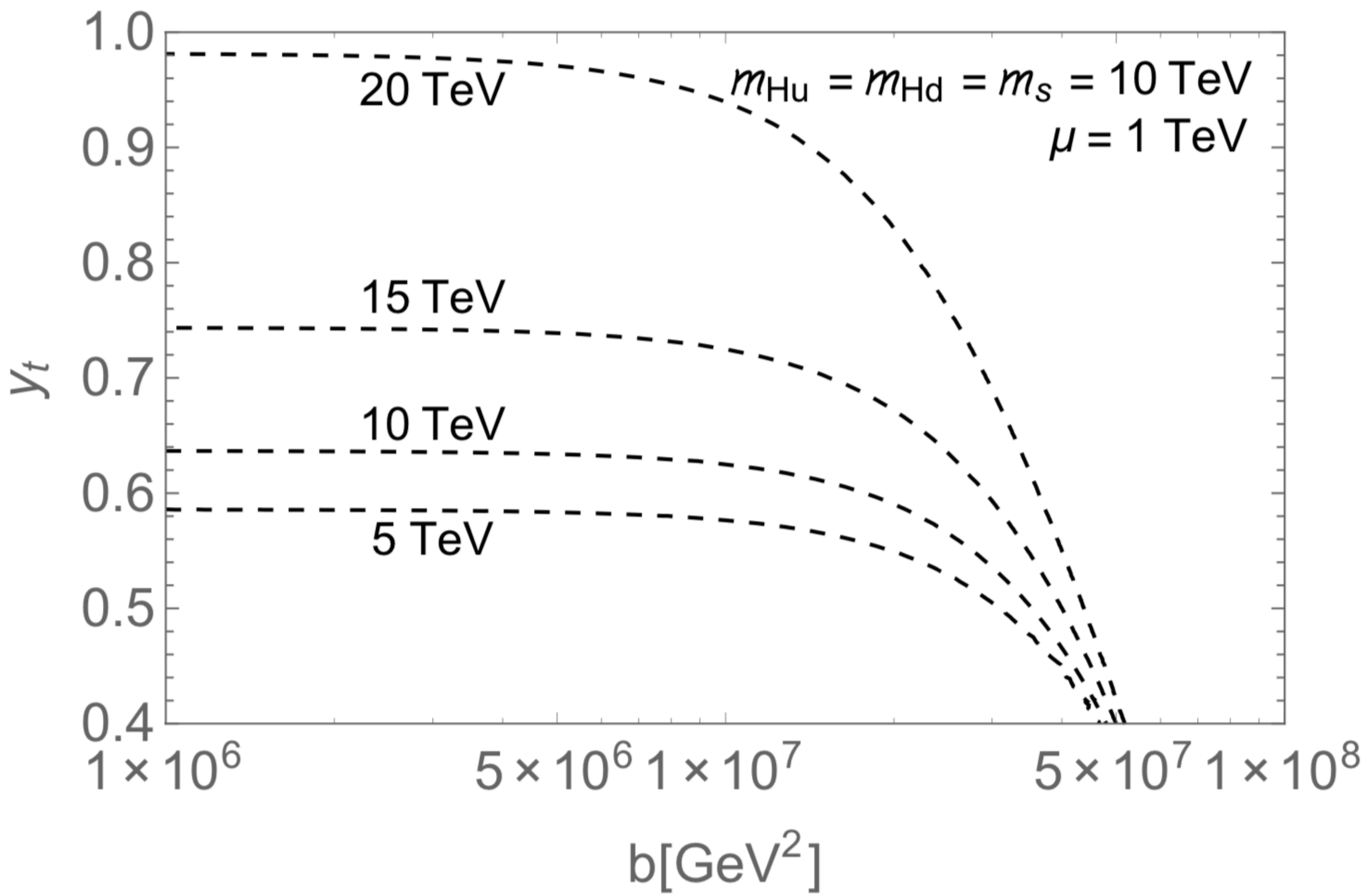
The better way to frame the problem, and the role of fine-tuning, is that we are seeking a theory that explains the *origin* of the EW scale.

If, within that theory, the EW scale is extremely sensitive to input parameters, it's not a very good explanation. The theory does not generically describe a universe like the one we live in.

If moving around in parameter space just produces modest changes in the low-energy physics, that's a compelling theory that predicts a world like ours.

SUSY and the hierarchy problem

When we say that weak-scale SUSY solves the hierarchy problem, we mean something simple:



The weak scale can be ***computed*** from input parameters, and is typically *of order* the SUSY breaking parameters.

Recasting the hierarchy problem

Many known solutions to the hierarchy problem really *recast* the problem into a different problem:

- Scale of supersymmetry breaking?
- Compositeness scale?
- Volume of extra dimensions?
- Why does the relaxion field have such an extremely flat potential?

They allow the electroweak scale to be **computed** from other inputs, but explaining the origin of those inputs is a new problem.

By changing the character of the problem, they allow for new kinds of solutions—often dimensional transmutation.

Scenarios: where will particle physics stand in 10-20 years?

LHC: no BSM discovery *or*
partial BSM discovery (e.g. some superpartners) *or*
confusing BSM status (e.g. lepton flavor violation but no correlated particles; "who ordered that?" signals)

Dark matter: no discoveries (direct detection down to ν floor) *or*
positive signals (direct detection, GCE confirmed in dwarfs, …?)

Precision tests of SM: no discoveries *or*
positive discovery beyond SM (e.g. electron electric dipole moment)

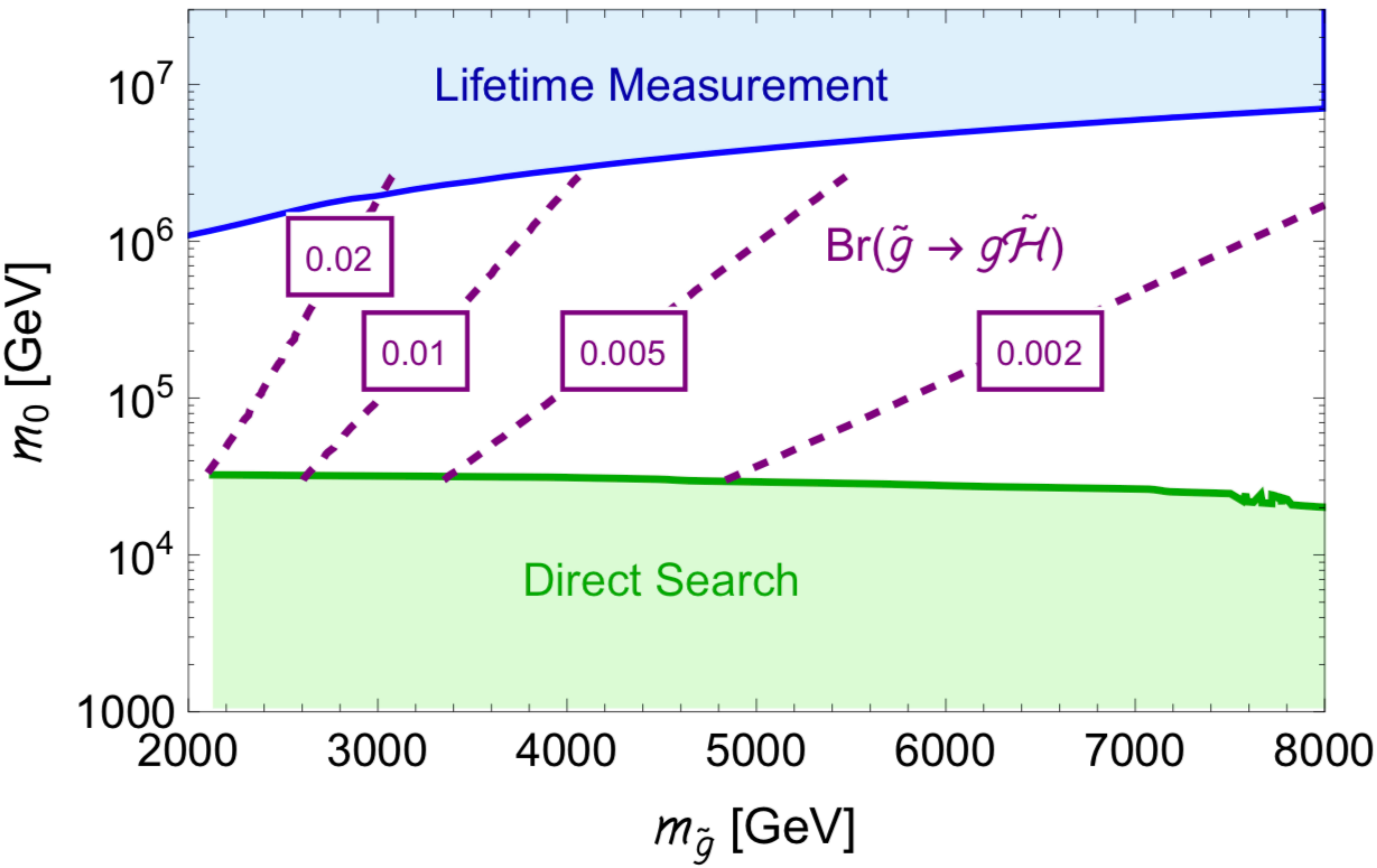
Other new physics: e.g. Hubble tension confirmed by GW standard sirens?

Some may be irrelevant to TeV scale (axion, Hubble tension?)

But I can imagine *no* scenario in which I *don't* want a high-energy collider for further exploration and assessment!

LHC partial discovery scenario

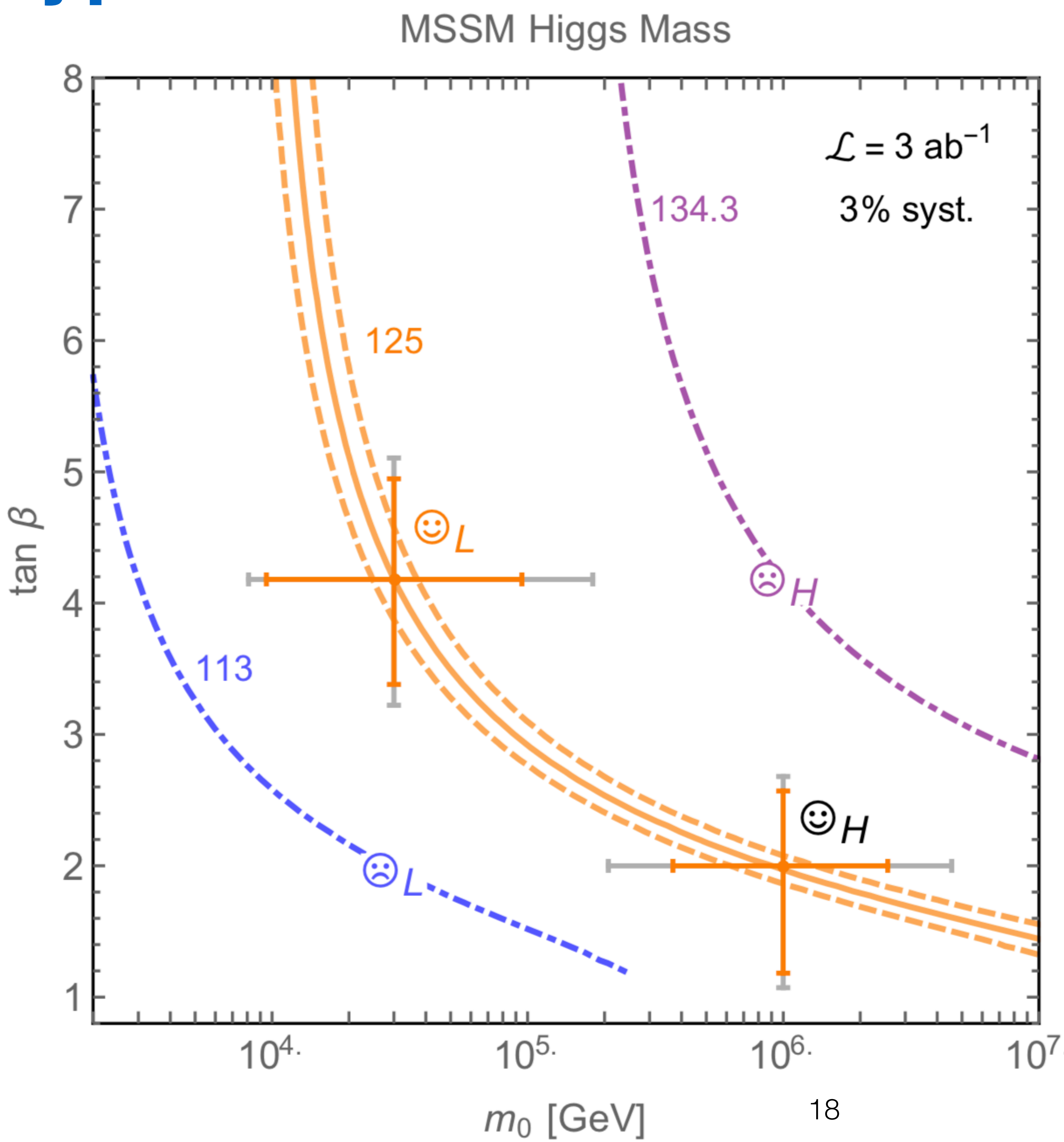
We may get only *part* of the spectrum. **Example: split SUSY**. Even at a 100 TeV collider, can't make the scalar superpartners. But the 100 TeV collider would be a **gluino factory.**



Can we learn where the scalars are?

Agrawal, Fan, MR, Xue '17

Testing the MSSM Higgs mass hypothesis

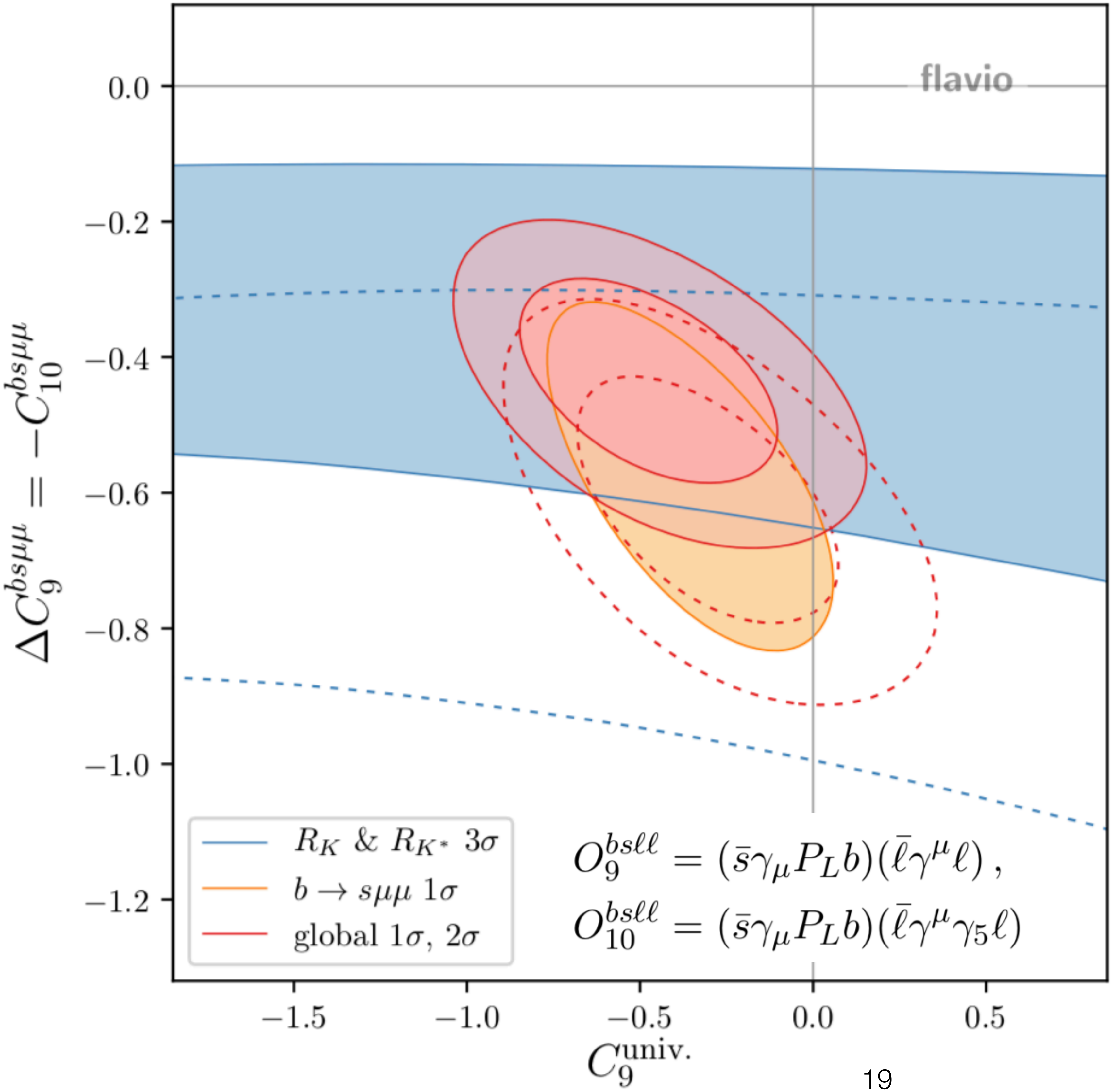


Precision physics @ future hadron collider could answer **big question**: where does the Higgs mass come from?

Would open up **new big question**: where does the SUSY breaking scale come from?

Agrawal, Fan, MR, Xue '17

Flavor?



Interesting discrepancies persist in the data, for instance in lepton flavor universality tests.

Aebischer, Altmannshofer, Guadagnoli, Reboud, Stangl, Straub 1903.10434

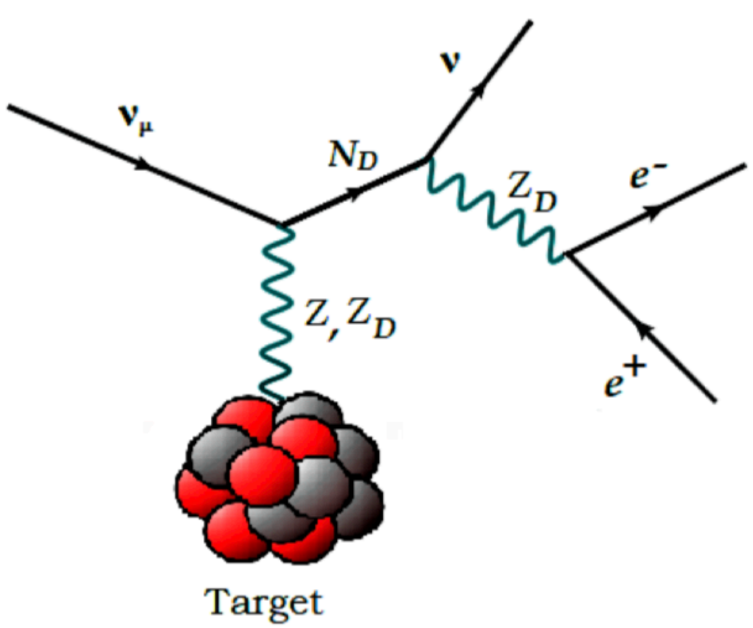
Will it hold up? More data on the way (Belle II).

If real, a new collider can reveal the underlying structure (leptoquarks? new, flavor-dependent forces?)

Neutrinos?

Many interesting anomalies (LSND, MiniBoone, reactor, gallium) might be interpreted as pointing to **sterile neutrinos** but this is **incompatible with other disappearance results**.

If we take all the data seriously, we need a more exotic model, with not just new mixings but **new forces** (e.g. Bertuzzo, Jana, Machado, Zukanovich Funchal ’18).



Again, new mass scales, new interactions beyond the SM—we would really need new collider experiments to pin down what is happening!

Will become much more clear if anomalies hold up in coming years.

arXiv:1807.09877 Bertuzzo et al. (but see Jordan et al., 1810.07185)

Dark matter: the "WIMP miracle"?

Should DM affect how we think about future colliders, one way or the other?

Thermal freezeout gives

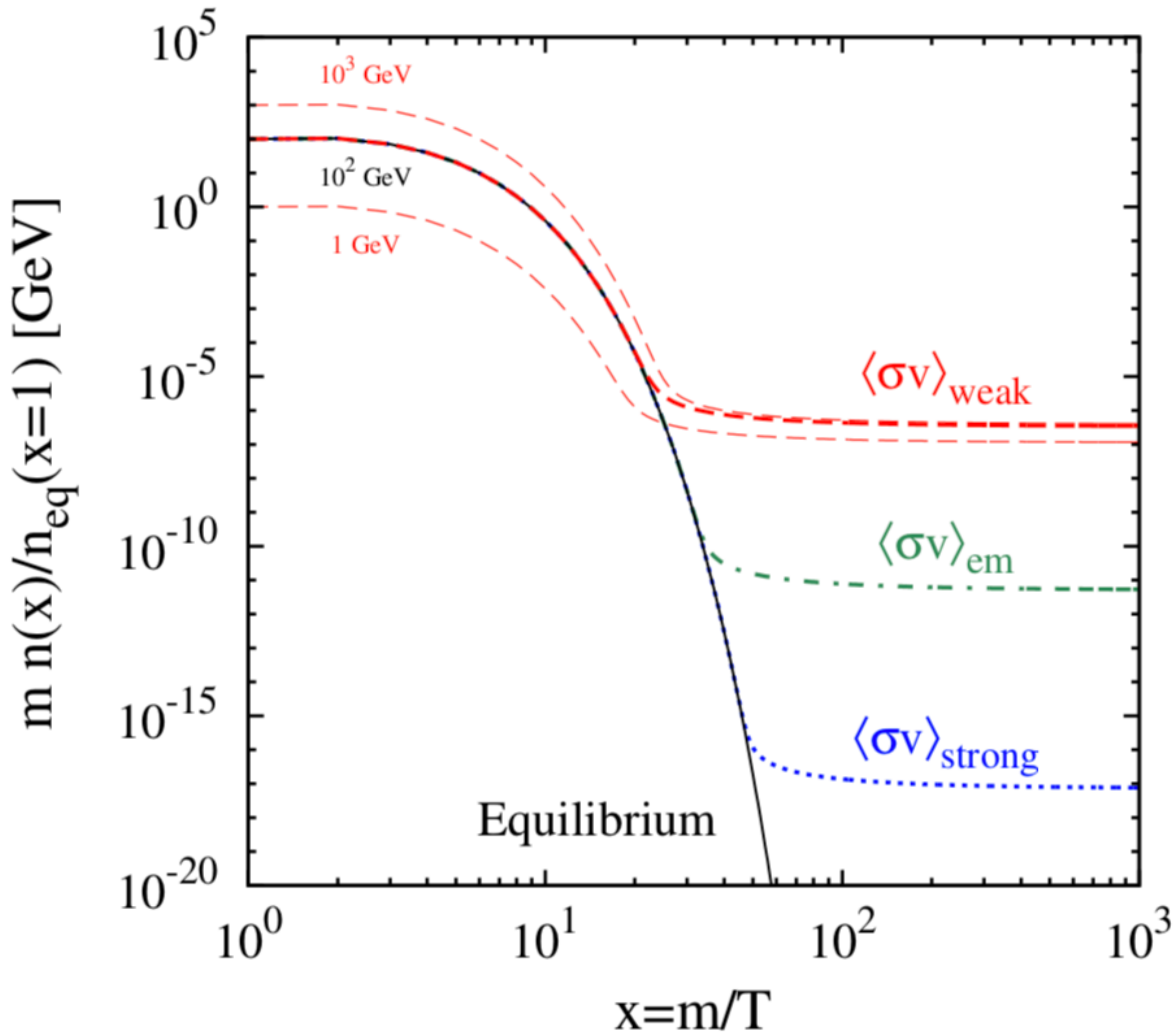
$$\Omega_{\rm DM}h^2 \approx 0.1\left(\frac{2\times 10^{-26}{\rm cm}^3/{\rm s}}{\langle\sigma v\rangle}\right)$$

$$\approx 0.1\times 0.2\left(\frac{\alpha^2/m_W^2}{\langle\sigma v\rangle}\hbar^2/c\right)$$

Parametrically, this is the mysterious coincidence

$$\left(\frac{\Omega_{\rm rad,0}}{\Omega_{\rm DM,0}}\right)\frac{1}{T_{\rm CMB,0}M_{\rm Pl}} \sim \frac{\alpha^2}{m_W^2}$$

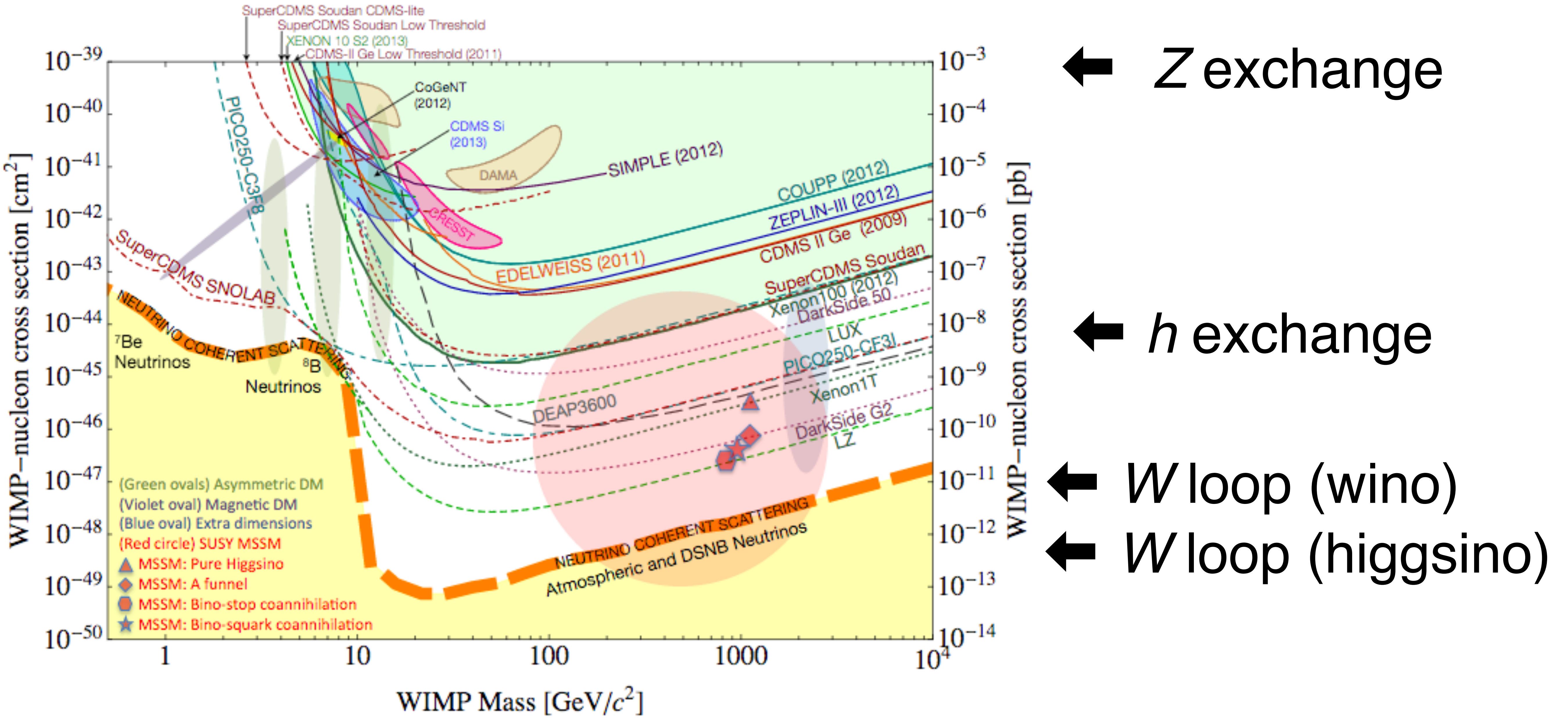
Needs both DM mass and interactions near the weak scale. Chiral neutrino long ruled out. Less miraculous?



Thermal relic abundance: figure from Beacom, Dasgupta, Steigman 1204.3622

Dark matter direct detection in the future

Snowmass: Cushman et al. 1310.8327



SU(2) multiplets dominantly scattering through loops are a real challenge, beyond the next generation of experiments.

Other dark matter possibilities?

In recent years there has been a proliferation of proposals for novel ways to look for dark matter, especially at lower masses. Unlike electroweak WIMPs, not such a clear weak-scale connection.

What if we get a discovery, e.g. DM in MeV to 10 GeV mass range?

New light particles interacting through *kinetic mixing* portal (dark photon) ⇒

collider searches for dark photon plus **new big question**: what sets the dark photon mass scale? Is it related to the EW scale?

Such a DM discovery ⇒ stronger case for a new collider.

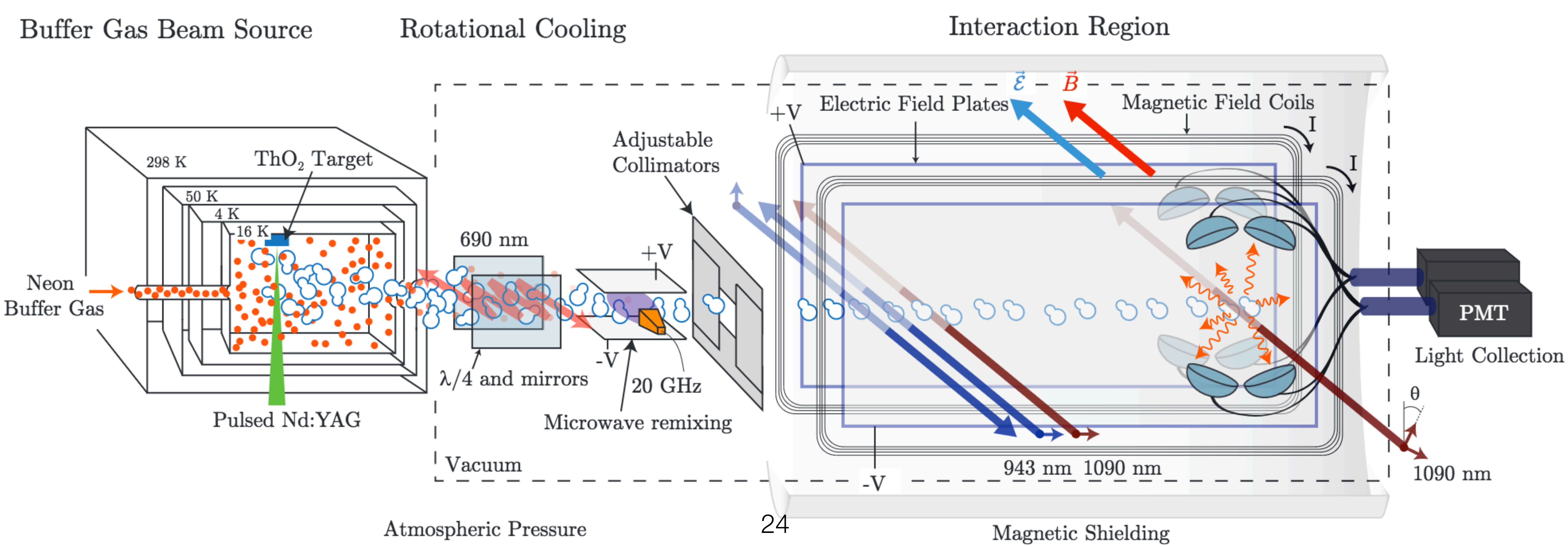
Even a discovery of axions would offer some encouragement that there is *not a desert* to the GUT scale, though f_a may be too large to probe directly.

Atomic Physics Testing the Standard Model

Colliders may not be our first sign of new physics!

Recent dramatic progress in AMO physics: searches for the electron EDM.

ACME 2 (source: electronedm.org) DeMille, Doyle, Gabrielse and collaborators. New result last year.



Electron EDM

The 2018 bound from ACME is: $|d_e| \lesssim 1.1 \times 10^{-29}\, e\,\mathrm{cm}$

This improves on the previous, 2013, ACME bound by about an order of magnitude.

EDMs violate chirality, so putting in the electron mass a spurion, we expect an effect of order:

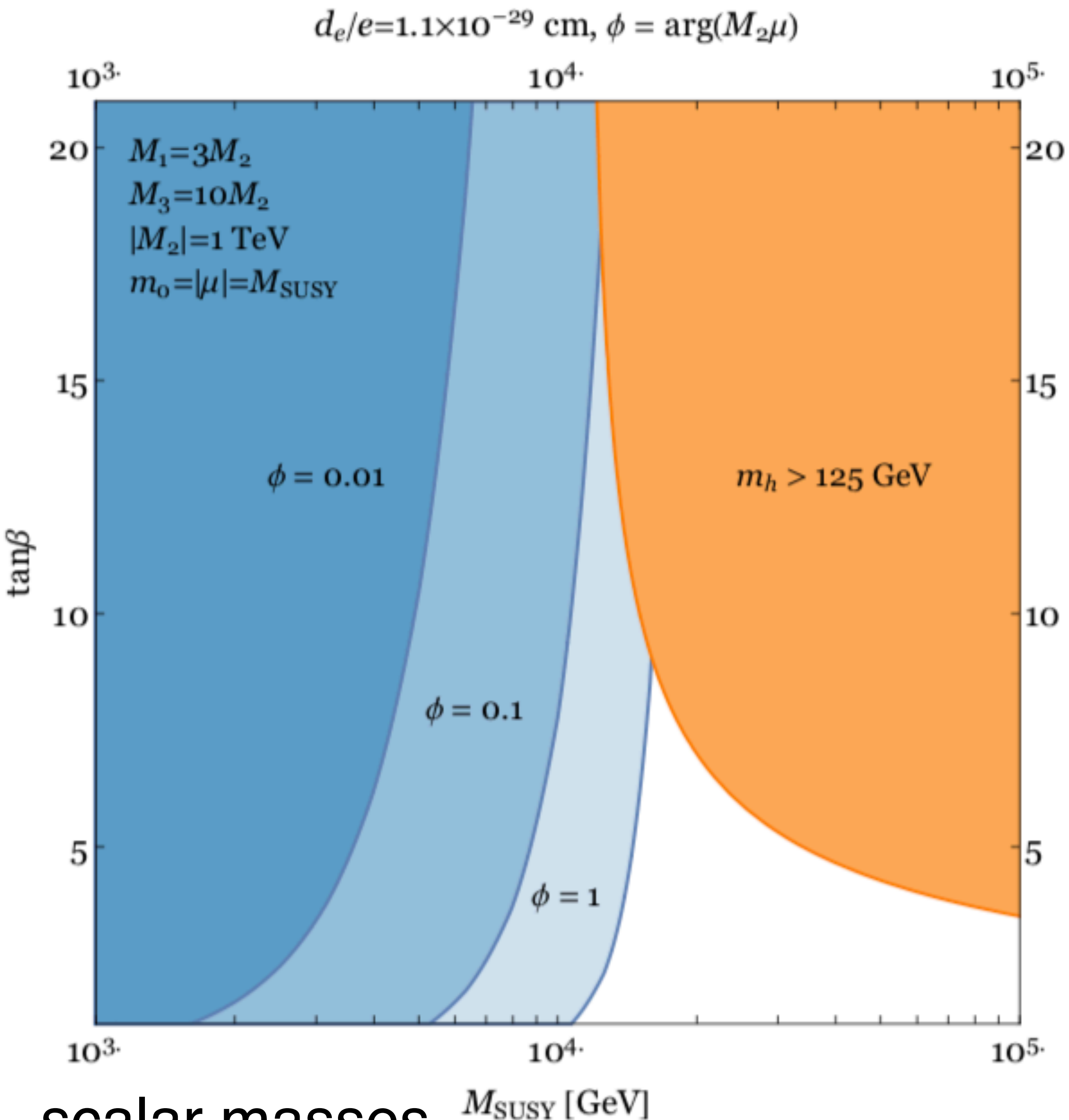
$$d_e \sim \delta_{\mathrm{CPV}} \left(\frac{\lambda}{16\pi^2}\right)^k \frac{m_e}{M^2}$$

Then dimensional analysis tells us that the experiment probes masses

0-loop	1-loop	2-loop
1000 TeV	50 TeV	3 TeV

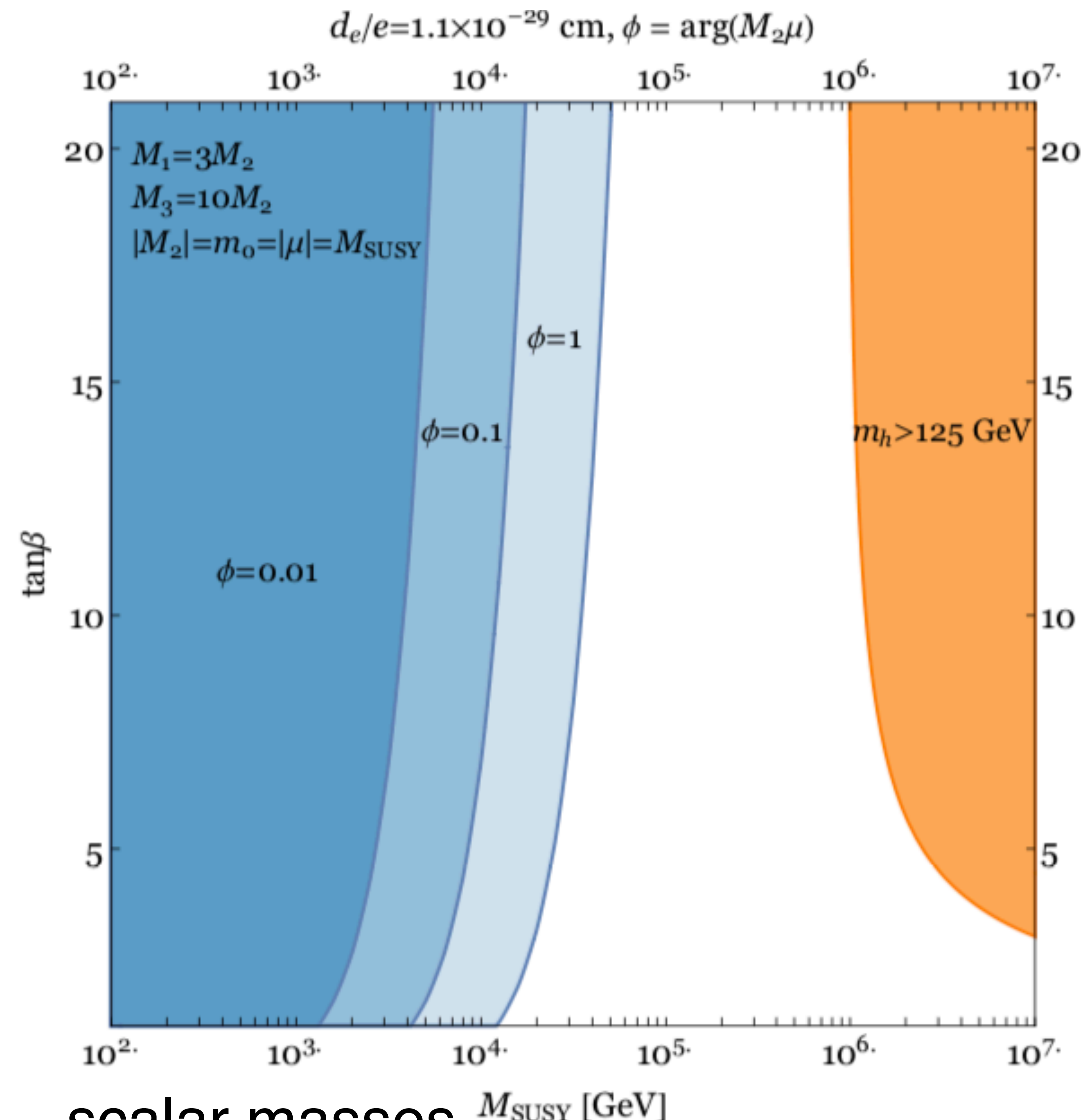
for order-one CPV phases this often exceeds LHC reach!

Electron EDM vs. MSSM



scalar masses

Split SUSY



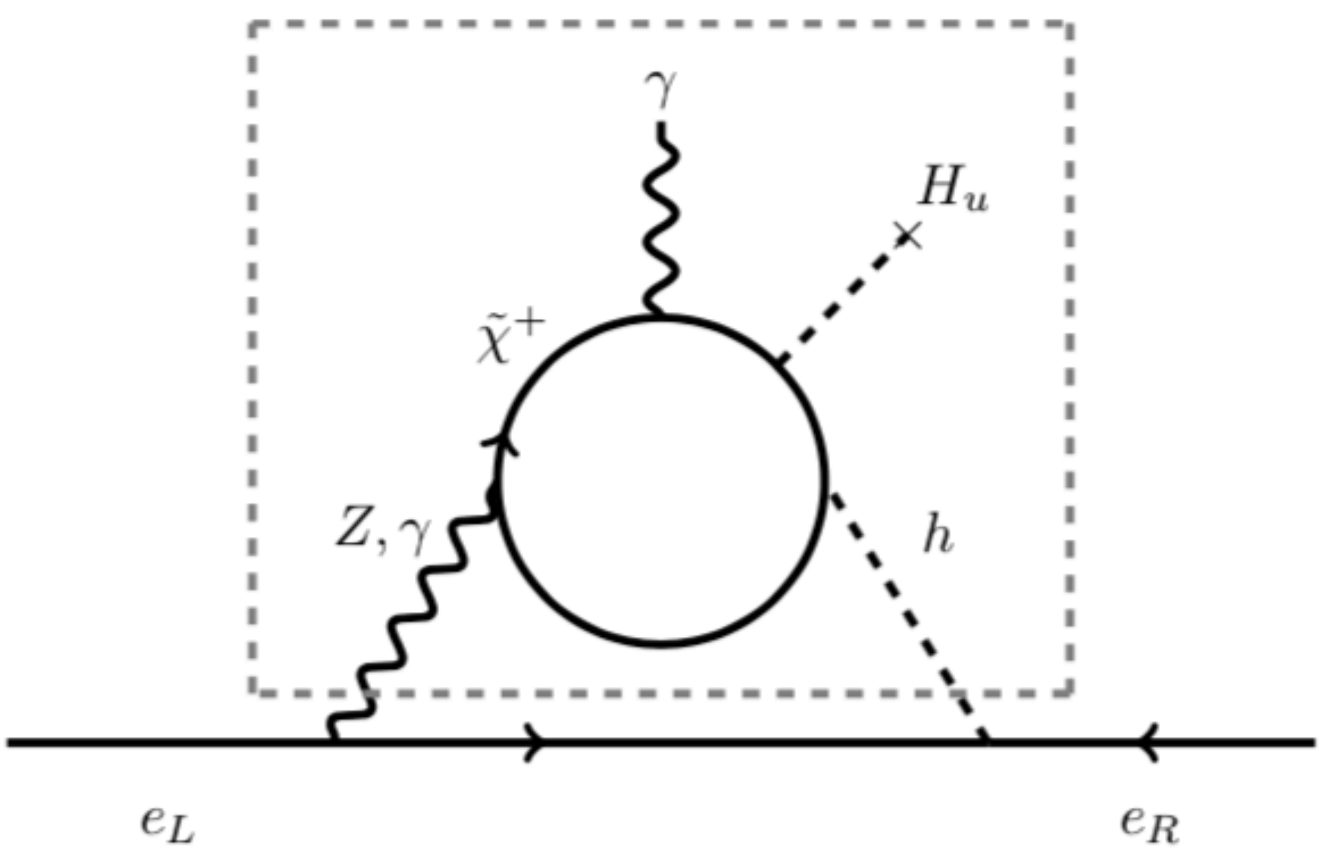
scalar masses

High-Scale SUSY

One-loop effects: Cari Cesarotti, Qianshu Lu, Yuichiro Nakai, Aditya Parikh, MR, ’18

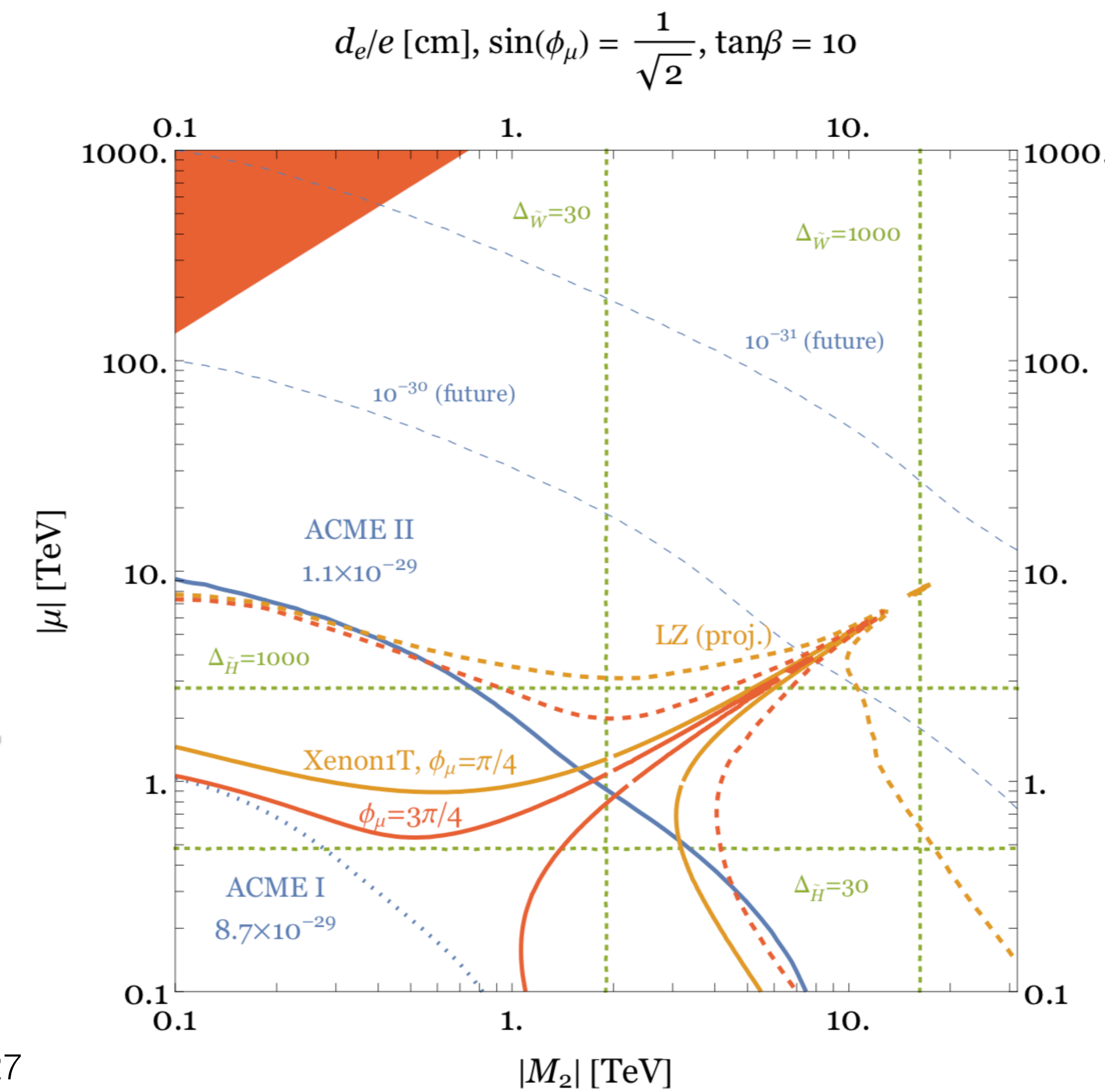
Electron EDM vs. Electroweak Physics

Quite generally, electroweak new physics coupling to the Higgs boson gives rise to an electron EDM (Barr-Zee).



[Cesarotti, Lu, Nakai, Parikh, MR, '18]

Powerful split SUSY electroweakino constraints from ACME 2!



And Others…

EDMs are not the only precision measurements that could potentially hint at physics beyond the Standard Model.

$\mu \to e\gamma$	Mu2e
$K \to \pi\nu\bar{\nu}$	NA62, KOTO
$(g-2)_\mu$	Fermilab
τ_{neutron}	existing tension between "beam" and "bottle"
[Your favorite here]	

We should keep looking for opportunities for smaller scale experiments that can very precisely test the SM in new regimes.

How Convincing Can Precision Null Results Be?

New physics ***discoveries*** or strong hints would strengthen the motivation for future energy-frontier colliders.

Converse: can a strong ***null result*** ever convince you that we *should not* build a collider? Usually not—still many possibilities remain.

Generic new physics with EW interactions allows for new CP phases and hence an electron EDM. So orders-of-magnitude stronger null results in EDM experiments would be mysterious if there is TeV-scale new physics.

Possibility: **CP** is a ***spontaneously broken*** symmetry. If all the breaking is *correlated* with flavor breaking, this could explain why the CP phase in the CKM is large but the CP phase in the EDM is small. (Nir & Rattazzi, 1996) Could be timely to revisit this idea—how small can phases naturally be?

Outlook, 1

The LHC has discovered what appears to be an elementary, spin-0 boson.

So far, it acts just like a Standard Model Higgs boson.

This is a *big deal*! We have learned something important about nature. But ***we do not understand the weak scale***, and we still have every reason to think *the answer is most likely to lie at nearby energies*.

We, as a field, are failing to convey this to younger physicists and possibly to funding agencies. A larger fraction of the field is spending time on *unmotivated* models that do not confront fundamental questions.

I don't know the answer, but I think that we need to talk *more* but also *more clearly* about why the big questions matter.

Outlook, 2

There are many smaller scale, non-collider experiments that are happening, and many more proposed for the future.

These include dark matter, flavor, EDMs, ….

Our first discovery of physics beyond the Standard Model could come from these experiments, but only a collider will allow us to directly characterize the properties of the new particles and interactions.

These experiments have the potential to strengthen our arguments for future colliders. However, in most cases null results at these experiments will not *weaken* the case, because a wide range of collider-accessible, motivated new physics can evade them.

Particle physics needs new energy frontier colliders!