The state of high-energy particle physics: a view from a neighboring field

Peter Woit

Mathematics Department
Columbia University

US Naval Observatory Colloquium
December 6, 2018

Two cultures

Physics

1979: B.A./M.A. in physics, Harvard
1984: Ph.D. in particle theory, Princeton
1984-87: Postdoc ITP Stony Brook

Mathematics

1988: adjunct Calculus instructor, Tufts math department
1988-9: Postdoc, Mathematical Sciences Research Institute, Berkeley
1989-93: Asst. professor, Columbia math department (non-tenure track)
1993-current: Permanent non-tenured position at Columbia, now Senior Lecturer

Neighboring fields, but very different language and culture. Remniscent of moving between US and France as a child (lived in Paris age 8-13).

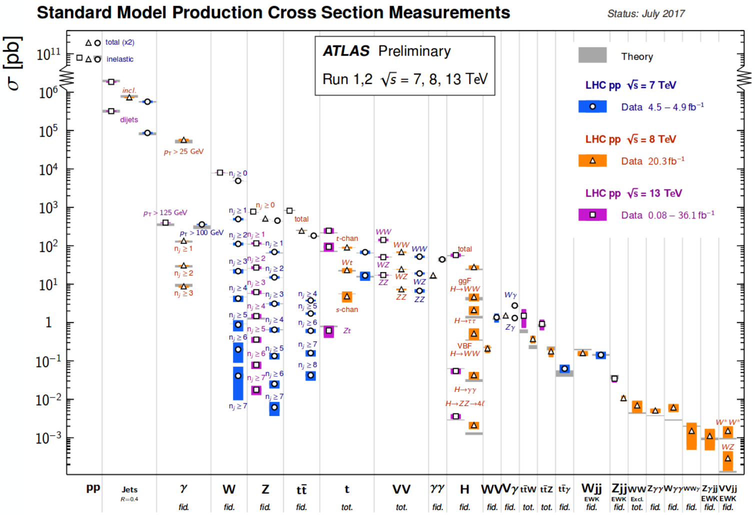
The Standard Model, some history

- 1973: SU(3) x SU(2) X U(1) gauge theory of strong, weak and electromagnetic forces
- 1983: Discovery of W/Z bosons
- 1995: Discovery of the top quark
- 1998: Discovery of non-zero neutrino masses (an extension of the original SM)
- 2012: Discovery of the Higgs at the LHC

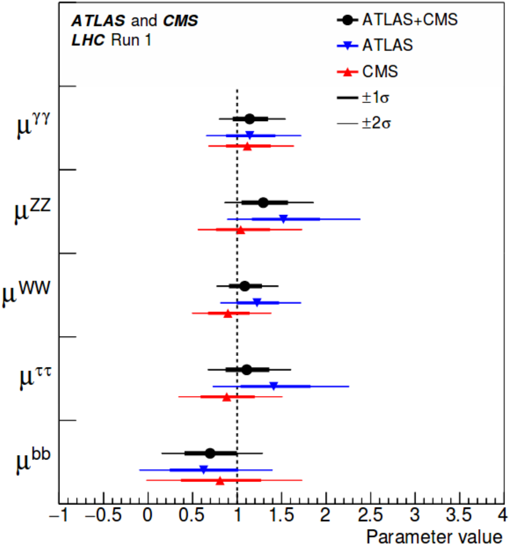
Current situation

All high energy accelerator experiments consistent with the SM. Ongoing experiments at the LHC at 13 TeV center of mass energy.

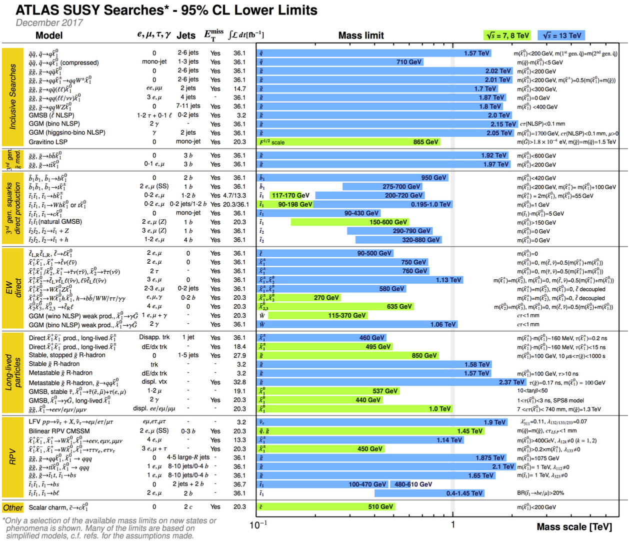
LHC cross-section measurements



Higgs coupling measurements



SUSY exclusions



Gluino mass limits

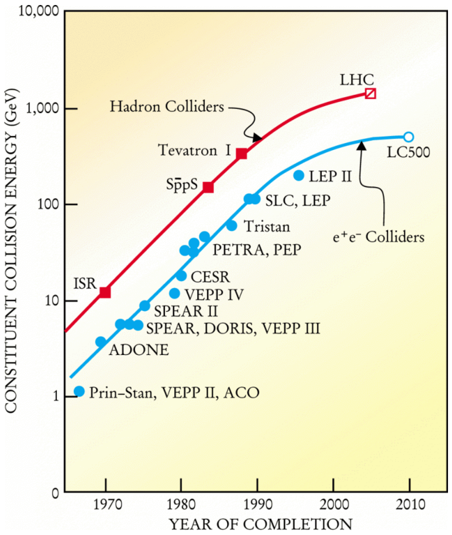
Tevatron: 300 GeV

LHC(7 TeV): 1.2 TeV

LHC(13 TeV): 2 TeV

LHC 14 TeV reach: $\sim$3 TeV

History and future of collider energies



Proposed (affordable) machines

- e^+e^-: 250 GeV, 2030s (ILC Japan)
- pp: 27 TeV, 2040s? (HE-LHC CERN)

Higher energies likely prohibitively expensive ($20 billion and up).

Technological limits at the high energy frontier

p-p colliders

$E \propto RB$
To double energy need to double circumference or double magnetic fields.
LHC magnets: B=8 Tesla
Proposed HE-LHC magnets: B=16 Tesla

$e^+ - e^-$ colliders

- Circular colliders:
  Synchrotron radiation losses $\propto E^4/R$
  LEP (209 GeV) power consumption = 40% city of Geneva
- Linear colliders:
  For given acceleration technology, fixed energy gradient, $E \propto L$
  Large power demand since beam dumped after acceleration, not stored.

Unfinished business

Open questions

- Why these particles, forces?
- Why these parameters?
- What about gravity?

Other experimental directions

- Neutrino physics
- Precision measurements
- Dark matter: astrophysics
- Cosmology

Experimental study of quantum gravity seems out of reach.

Enlarging fundamental symmetries

The fundamental symmetries of the Standard Model are

- SU(3) x SU(2) x U(1) internal gauge symmetries
- Poincaré group of spacetime symmetries

Grand Unified Theories (GUTs), 1974

Extend internal symmetry to a larger group (such as SU(5) or SO(10)) which includes SU(3) x SU(2) x U(1) as subgroup.
Problem: new symmetry generators imply interactions that allow protons to decay (quarks decay to leptons), conflict with proton decay experiments.

Supersymmetry (SUSY), 1977

Extend Poincaré group to a larger "super"-group (allowing anticommuting variables). **Problem**: new symmetry generators imply "super-partner" states for all know elementary particles, but these have not been seen (e.g. table above of SUSY exclusions).

Supergravity unification

Can use SUSY as a gauge symmetry, and get "super-gravity" theories incorporating gravity as well as the Standard Model.
1980: Hawking inaugural lecture as Lucasian professor was about unification of all interactions using supergravity GUTs:
"*Is the End in Sight for Theoretical Physics?*"

Problems

- No experimental evidence: need to explain why symmetries not visible at accessible energies
- Theoretical problems: supergravity may not be renormalizable, may not be able to get correct electroweak force properties

String unification: the vision

1984-5: Proposal to take as fundamental not quantized particles, but quantized strings, in a supersymmetric version, the "superstring". Consistency requires 10 space-time dimensions.

The vision

Take 10d superstring as fundamental, compactify 6 dimensions using a special type of manifold (Calabi-Yau).
Get effective supergravity theory in 4d at low energy, unified theory of SM + quantum gravity.

7 known families of Calabi-Yaus, each one parametrized by moduli spaces of various dimensions from 36 to 203.
The plan: pick family, find dynamics that fixes the moduli, get the SM.

String unification: the problem

- Collaboration with mathematicians: more and more families of Calabi-Yaus. Currently unknown if the number of families is finite.
- Better understanding of the theory: more and more possibilities for dealing with extra 6 dimensions (e.g. branes). More and more possible "string vacua" (currently $10^{272,000}$ for each family).

Research has steadily moved in the wrong direction, away from the vision

Better understanding the theory just keeps making the problem worse. More and more possible "approximate string vacua"

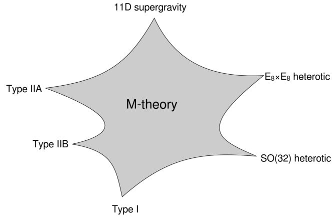
Fundamental problem

It appears that you can get just about any low energy physics you want, depending what you do with the extra dimensions. No predictions about observable physics.

String unification: the source of the problem

- String theory is a generalization of single-particle quantum theory, not of many-particle quantum field theory.
- Can get an analog of single-particle interactions from the geometry of the string. Can get an analog of a Feynman diagram expansion.
- Don't get the phenomena of QFT: non-trivial vacuum, non-perturbative behavior. Need a "non-perturbative string" or "string field" theory to get true, not approximate, "string vacua".

M-theory conjecture



1995: M-theory

Conjectural non-perturbative theory

1997: AdS/CFT

New ideas about non-perturbative string theory, but no help with the "too many string vacua" problem

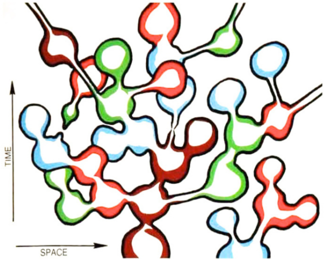
Current situation: "string theory" is not a theory, but a conjecture there is a theory

Typical summary talk by David Gross, Strings 20XX. "The big open questions are: What is string theory? What are the underlying symmetries of string theory?"

Fallout from string unification failure: the Multiverse

Where string theory unification vision has ended up

- Conjectured features of string theory imply if one "string vacuum" is consistent, so are an exponentially large number of them
- Can get essentially any low energy physics by choice of "string vacuum"
- Inflationary cosmology is invoked to create multiple universes and populate the possible "string vacua".



The end of the search for unification in physics?

Claims are now being made that evidence for inflation implies evidence for universes with different physics. However:

There are no testable predictions of this idea because there is no actual theory, no theory which could describe the metastable "vacua" and how they are populated by an inflationary big bang model.

A real and present danger

- There is no actual theory, just a hope that a theory exists. This can't be tested and isn't science.
- Will string theory enter the textbooks, with the multiverse explaining why it can't be tested?
- Promotion of this to the public damages understanding of what science is.
- Promotion of this to students and young researchers discourages them from working on the unification problem.

Mathematics: a non-empirical science

There is one science that does not rely on input from experiment to make progress: **Mathematics**.

Mathematics suffers from some of the same inherent difficulties as theoretical physics: great successes during the 20th century were based on the discovery of sophisticated and powerful new theoretical frameworks. It is increasingly difficult to do better, as the easier problems get solved.

But abstract mathematics is in a very healthy state, with recent solutions of long-standing problems:

1994: Fermat's Last Theorem (Taylor-Wiles)

2003: Poincaré Conjecture (Perelman)

A historical precedent

Can abstract mathematics instead of experiment inspire theoretical physics progress? A precedent:

Einstein's 1912 breakthrough towards General Relativity

"This problem remained insoluble to me until 1912, when I suddenly realized that Gauss's theory of surfaces holds the key for unlocking this mystery... I realized that the foundations of geometry have physical significance. My dear friend the mathematician Grossmann was there when I returned from Prague to Zurich. From him I learned for the first time about Ricci and later about Riemann."

Einstein's 1915 discovery of the field equations

Einstein lectured on his work in Gottingen in 1915, entered into discussions with Hilbert. They both later in the year came up (independently?) with the final form of the field equations.

Two cultures

Caricatures

Mathematicians

Physicists give arguments with obvious holes and ill-defined concepts, often it's completely unclear what they are actually claiming or what the argument is supposed to be.

(Many of my colleagues tell me they started out in physics, moved to math after being unable to follow a quantum mechanics class).

Physicists

Mathematicians devote their time to pedantic $\epsilon - \delta$ arguments or empty abstraction, ignoring what is interesting. They write unmotivated papers in an unreadable style.

(This is what I often thought in the first stage of my career).

The culture of mathematics

Some things valued highly in mathematics, less so in physics:

- Always be extremely clear about precise assumptions
- Always pay close attention to the logic of an argument: at each step, does the conclusion really follow?
- Where precisely is the boundary between what is understood and what isn't?

Paying close attention to these concerns carries a big cost, danger of getting lost in technicalities. Best mathematics avoids this, less good mathematics doesn't.

Physics has never really needed to pay close attention to these issues. Experiment could be relied upon to sooner or later help sort out which calculations/arguments work, which don't. String theory and the multiverse provide an extreme example of where arguments are often made based on unclear assumptions.

Mathematics and physics: a deep unity

Historically, deep new ideas about mathematics and physics have turned out to be closely related

Mathematics	Physics
Riemannian geometry (1867 -)	General relativity (1915 -)
Lie group representations (1925 -)	Quantum mechanics (1925 -)
Index theorem (1960 -)	Dirac equation (1928 -)
Ehresmann connections (1950 -)	Yang-Mills theory (1954 -)

This continues to the present day (an example: topological quantum field theories).

Mathematics and physics: surprising connections

Deep ideas in mathematics with no connection to physics sometimes turn out to involve the same mathematical structures as the Standard Model.

An example: topology and gauge theory

The observables of a variant of the Standard Model QFT (‘twisted” $N = 2$ SUSY gauge theory), formulated on an arbitrary 4-dimensional space, turned out to be important topological invariants of the space (Donaldson invariants).

Ways forward in absence of experimental hints

Two possible lessons to draw:

One should pay close attention to what we don't understand precisely about the Standard Model, even if the standard prejudice is "that's a hard technical problem, whose solution won't tell us anything interesting." For instance:

- Non-perturbative electroweak theory (including non-perturbative BRST treatment of gauge symmetry)
- Wick rotation of spinor quantum fields

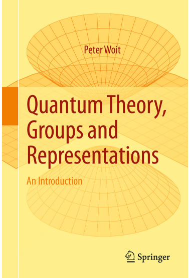
Exploit the unity of mathematics and fundamental physics: better understand the mathematical structures behind the Standard Model and what mathematicians know about them.

The rest of this talk: two advertisements.

Advertisement 1: Group representations are fundamental to the structure of quantum mechanics

A group and its representation theory govern the basic structure of quantum mechanics, not just symmetries of a Hamiltonian

For a detailed development of QM and QFT from this point of view, see



Based on a year-long course for advanced undergraduates and graduate students, taught 2012-3 and 2014-5.

http://www.math.columbia.edu/~woit/QMbook

Advertisement 2: Dirac cohomology, and the ubiquity of the Dirac operator

The Dirac operator plays a central role in a new set of ideas about representation theory that have appeared in the last 10-20 years: "Dirac cohomology"

Work in progress: a paper explaining the relation of Dirac cohomology to the BRST formalism, and possible applications to new ways to exploit group representation theory in physics.

Summary

Some intentionally provocative claims:

- Popular speculative ideas for how to go beyond the Standard Model (GUTs, SUSY, strings) have failed. The use of untestable multiverse scenarios to excuse this failure is a significant danger to science.
- Technological barriers are starting to make it impossible to make progress on HEP physics as before. HEP theorists might want to look to mathematics for some guidance, both methodological and substantive.
- The concept of a representation of a group is both a unifying theme in mathematics, and at the basis of the axioms of quantum mechanics.
- The Standard Model may be closer to a true unified theory than people expect, but with new ideas about its underlying structure needed. Inspiration for such new ideas might be found in modern mathematics, in particular in advances in representation theory, with a hint the central role of the Dirac operator.

Thanks for your attention!