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SEARCHES FOR NEW PHYSICS | OPINION

Witten reflects

21 December 2021

Edward Witten has spent almost 50 years at the forefront of theoretical and mathematical physics. Here he describes how the LHC and other recent results have impacted his view on nature, and asks whether naturalness is still a useful guide for the field.

Trendsetter Edward Witten, Charles Simonyi professor at the Institute for Advanced Study, Princeton, on the occasion of the award of the 2012 Breakthrough Prize in Fundamental Physics. Credit: B Lacombe/Breakthrough Prize

How has the discovery of a Standard Model-like Higgs boson changed your view of nature?

The discovery of a Standard Model-like Higgs boson was a great triumph for renormalisable field theory, and really for simplicity. By the time the LHC was operating, attempts to make the Standard Model (SM) work without an elementary Higgs field – using a dynamical mechanism instead – had become rather convoluted. It turned out that, as far as one can judge from what we have learned so far, the original idea of an elementary Higgs particle was correct. This also means that nature takes advantage of all the possible building blocks of renormalisable field theory – fields of spin 0, 1/2 and 1 – and the flexibility that that allows.

The other key fact is that the Higgs particle has appeared by itself, and without any sign of a mechanism that would account for the smallness of the energy scale of weak interactions compared to the much larger presumed energy scales of gravity, grand unification and cosmic inflation. From the perspective that my generation of particle physicists grew up with (and not only my generation, I would say), this is quite a shock. Of course, we lived through a somewhat similar shock a little over 20 years ago with the discovery that the expansion of the universe is accelerating – something that is most simply interpreted in terms of a very small but positive cosmological constant, the energy density of the vacuum. It seems that the ideas of naturalness that we grew up with are failing us in at least these two cases.

What about new approaches to the fine-tuning problem such as the relaxion or "Nnaturalness"?

Unfortunately, it has been very hard to find a conventional natural explanation of the dark energy and hierarchy problems. Reluctantly, I think we have to take seriously the anthropic alternative, according to which we live in a universe that has a "landscape"of possibilities, which are realised in different regions of space or maybe in different portions of the quantum mechanical wavefunction, and we inevitably live where we can. I have no idea if this interpretation is correct, but it provides a yardstick against which to measure other proposals. Twenty years ago, I used to find the anthropic interpretation of the universe upsetting, in part because of the difficulty it might present in understanding physics. Over the years I have mellowed. I suppose I reluctantly came to accept that the universe was not created for our convenience in understanding it.

Which experimental paths should physicists prioritise at this time?

It is extremely important to probe the twin mysteries of the cosmic acceleration and the smallness of the electroweak scale as thoroughly as possible, in order to determine whether we are interpreting the facts correctly and possibly to discover a new layer of structure. In the case of the cosmic acceleration, this means measuring as precisely as we can the parameter w (the ratio of pressure and energy), which equals –1 if the acceleration of the expansion is governed by a simple cosmological constant, but would be greater than –1 in most alternative models. In particle physics, we would like to probe for further structure as precisely as we can both indirectly, for example with precision studies of the Higgs particle, and hopefully directly by going to higher energies than are available at the LHC.

What might be lurking at energies beyond the LHC?

If it is eventually possible to go to higher energies, I can imagine several possible outcomes. It might become rather clear that the traditional idea of naturalness is not the whole story and that we have on our hands a "bare" Higgs particle, without a mechanism that would account for its mass scale. Alternatively, we might find out that the apparent failure of naturalness was an illusion and that additional particles and forces that provide an explanation for the electroweak scale are just beyond our current experimental reach. There is also an intermediate possibility that I find fascinating. This is that the electroweak scale is not natural in the customary sense, but additional particles and forces that would help us understand what is going on exist at an energy not too much above LHC energies. A fascinating theory of this type is the "split supersymmetry" that has been proposed by Nima Arkani-Hamed and others.

It seems that the ideas of naturalness that we grew up with are now failing us

There is an obvious catch, however. It is easy enough to say "such-and-such will happen at an energy not too much above LHC energies". But for practical purposes, it makes a world of difference whether this means three times LHC energies, six times LHC energies, 25 times LHC energies, or more. In theories such as split supersymmetry, the clues that we have are not sufficient to enable a real answer. A dream would be to get a concrete clue from experiment about what is the energy scale for new physics beyond the Higgs particle.

Could the flavour anomalies be one such clue?

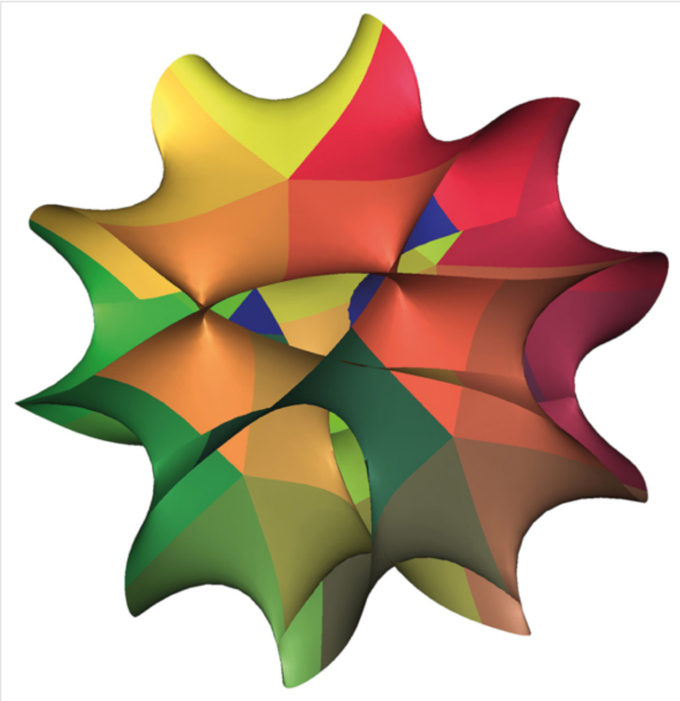
There are multiple places that new clues could come from. The possible anomalies in b physics observed at CERN are extremely significant if they hold up. The search for an electric dipole moment of the electron or neutron is also very important and could possibly give a signal of something new happening at energies close to those that we have already probed. Another possibility is the slight reported discrepancy between the magnetic moment of the muon and the SM prediction. Here, I think it is very important to improve the lattice gauge theory estimates of the hadronic contribution to the muon moment, in order to clarify whether the fantastically precise measurements that are now available are really in disagreement with the SM. Of course, there are multiple other places that experiment could pinpoint the next energy scale at which the SM needs to be revised, ranging from precision studies of the Higgs particle to searches for muon decay modes that are absent in the SM.

Which current developments in theory are you most excited about?

The new ideas about gravity and quantum mechanics that go under the rough title "It from qubit" are really exciting. Black-hole thermodynamics was discovered in the 1970s through the work of Jacob Bekenstein, Stephen Hawking and others. These results were fascinating, but for several decades it seemed to me – rightly or wrongly – that this field was evolving only slowly compared to other areas of theoretical physics. In the past decade or so, that is clearly no longer the case. In large part the change has come from thinking about "entropy" as microscopic or fine-grained von Neumann entropy, as opposed to the thermodynamic entropy that Bekenstein and others considered. A formulation in terms of fine-grained entropy has made possible new statements and more general statements which reduce to the traditional ones when thermodynamics is valid. All this has been accelerated by the insights that come from holographic duality between gravity and gauge theory.

How different does the field look today compared to when you entered it?

It is really hard to exaggerate how the field has changed. I started graduate school at Princeton in September 1973. Asymptotic freedom of non-abelian gauge theory had just been discovered a few months earlier by David Gross, Frank Wilzcek and David Politzer. This was the last key ingredient that was needed to make possible the SM as we know it today. Since then there has been a revolution in our experimental knowledge of the SM. Several key ingredients (new quarks, leptons and the Higgs particle) were unknown in 1973. Jets in hadronic processes were still in the future, even as an idea, let alone an experimental reality, and almost nothing was known about CP violation or about scaling violations in high-energy hadronic processes, just to mention two areas that developed later in an impressive way.



Higher plane 6D Calabi–Yau manifolds, on which the extra dimensions of string theory are conjectured to be "compactified", exhibit powerful symmetries that Witten helped uncover. Credit: A J Hanson

Not only is our experimental knowledge of the SM so much richer than it was in 1973, but the same is really true of our theoretical understanding as well. Quantum field theory is understood much better today than was the case in 1973. There really is no comparison.

Perhaps equally dramatic has been the change in our understanding of cosmology. In 1973, the state of cosmological knowledge could be summarised fairly well in a couple of numbers – notably the cosmic-microwave temperature and the Hubble constant – and of these only the first was measured with any reasonable precision. In the intervening years, cosmology became a precision science and also a much more ambitious science, as cosmologists have learned to grapple with the complex processes of the formation of structure in the universe. In the inhomogeneities of the microwave background, we have observed what appear to be the seeds of structure formation. And the theory of cosmic inflation, which developed starting around 1980, seems to be a real advance over the framework in which cosmology was understood in 1973, though it is certainly still incomplete.

Exploring the string-theory framework has led to a remarkable series of discoveries

Finally, 50 years ago the gulf between particle physics and gravity seemed unbridgeably wide. There is still a wide gap today. But the emergence in string theory of a sensible framework to study gravity unified with particle forces has changed the picture. This framework has turned out to be very powerful, even if one is not motivated by gravity and one is just searching for new understanding of ordinary quantum field theory. We do not understand today in detail how to unify the forces and obtain the particles and interactions that we see in the real world. But we certainly do have a general idea of how it can work, and this is quite a change from where we were in 1973. Exploring the string-theory framework has led to a remarkable series of discoveries. This well has not run dry, and that is one of the reasons that I am optimistic about the future.

Which of the numerous contributions you have made to particle and mathematical physics are you most proud of?

I am most satisfied with the work that I did in 1994 with Nathan Seiberg on electric-magnetic duality in quantum field theory, and also the work that I did the following year in helping to develop an analogous picture for string theory.

Who knows, maybe I will have the good fortune to do something equally significant again in the future.

Interview by **Matthew Chalmers**.