***FOUNDATIONS OF MATHEMATICS AND PHYSICS ONE CENTURY AFTER HILBERT: NEW PERSPECTIVES* EDITED BY JOSEPH KOUNEIHER**

JOHN C. BAEZ

The title of this book recalls Hilbert's attempts to provide foundations for mathematics and physics, and raises the question of how far we have come since then—and what we should do now. This is a good time to think about those questions.

The foundations of mathematics are growing happily. Higher category theory and homotopy type theory are boldly expanding the scope of traditional set-theoretic foundations. The connection between logic and computation is growing ever deeper, and significant proofs are starting to be fully formalized using software. There is a lot of ferment, but there are clear payoffs in sight and a clear strategy to achieve them, so the mood is optimistic.

This volume, however, focuses mainly on the foundations of physics. In recent decades fundamental physics has entered a winter of discontent. The great triumphs of the 20th century—general relativity and the Standard Model of particle physics—continue to fit almost all data from terrestrial experiments, except perhaps some anomalies here and there. On the other hand, nobody knows how to reconcile these theories, nor do we know if the Standard Model is mathematically consistent. Worse, these theories are insufficient to explain what we see in the heavens: for example, we need something new to account for the formation and structure of galaxies.

But the problem is not that physics is unfinished: it has always been that way. The problem is that progress, extremely rapid during most of the 20th century, has greatly slowed since the 1970s. While theories developed since then have yielded a huge harvest of exciting mathematics, their predictions have not been confirmed by experiment. The discovery of the Higgs boson, for example, merely confirms a theory that particle physicists proposed in the 1960s. So far the Large Hadron Collider has not found anything new.

Thus, it is fascinating to see Joseph Kouneiher's attempt to bring together some of the best mathematicians and physicists and let them speak on what has happened in last century. The result illustrates the frustrating situation fundamental physics finds itself in now.

The elephant in the room is string theory. After the rise of the Standard Model, some of the best minds in physics turned to the project of unifying all particles and forces, including gravity. After a variety of inconclusive attempts, by about 1986 they settled on string theory as the most promising candidate. The main new principles were supersymmetry and the use of higher-dimensional extended objects—notably strings—to model particles. The mathematical sophistication required to do string theory far exceeded previous standards in particle physics, and a new cast of characters came to the fore, the most prominent being Edward Witten. By around 1995, he and others found clues that all the various kinds of string theory were limits of a single 11-dimensional theory, now called M-theory, even though the precise formulation of this theory remains elusive. In the process, higher-dimensional membranes of various kinds became important in string theory. Still later, in 1997, Juan Maldacena found evidence for an interesting isomorphism or 'duality' between certain supersymmetric gauge theories and string theories; this is called the AdS-CFT correspondence.

These ideas, and their many spinoffs, have transformed mathematics in ways that could not have been imagined at the start. An intricate web of new connections has become visible. We now know, for example, that the Monster group (the largest sporadic finite simple group) is connected to the j-function (the most basic invariant of elliptic curves) via a quantum field theory built using the Leech lattice. Similarly, but still in its embryonic stages, there is now a promising line of thought that attempts to connect Khovanov homology (a sophisticated knot invariant) to the geometric Langlands program (a Riemann surface analogue of the more famous program in number theory) using a mysterious quantum field theory in 6-dimensional spacetime. The almost psychedelic nature of these connections means that new surprises are bound to emerge as we dig deeper toward simple explanations of what we know so far. For example, higher categories are starting to play a significant role [4].

Given its remarkable impact on mathematics, it is natural to ask what string theory has achieved toward its original goal: becoming a true theory of *physics*, one that makes experimental predictions we can test. The volume under review does not address this. It does include a paper by Witten, entitled "What every physicist should know about string theory". This is clear and worth reading—but it could have been written decades ago, except for a sentence about dualities between gauge theory and gravity. It does not tackle the question of where string theory stands today. Elsewhere [3], Witten has said:

> I actually believe that string / M-theory is on the right track toward a deeper explanation. But at a very fundamental level it's not well understood. And I'm not even confident that we have a good concept of what sort of thing is missing or where to find it.

Some other authors in this volume echo this sense of frustration. Roger Penrose gives a lucid and fascinating account of his work on twistors, an approach to physics based on complex projective geometry. This is one of the high points of the book. Twistor theory has led to many interesting ideas in geometry and representation theory, and starting around 2004 it has combined forces with string theory and produced some surprising new techniques for computing scattering amplitudes. However, it is still far from a theory of real-world physics. At the end of his paper, after sketching some new ideas, Penrose writes:

> If all these procedures (or something like them) indeed work roughly as intended [...] then there could appear to be possible openings for twistor theory applicable to basic physics generally. [....] Yet, much work needs to be done to decide whether or not the ideas outlined here can really be made to hang together, and if they do not, then we need to know what might replace them.

The physicist Lee Smolin asks what we may be missing in our quest for a theory of quantum gravity, and gives some answers. Pointedly, he notes:

> Perhaps we might, for a moment, consider that the approaches so far pursued are not really theories, in the sense quantum mechanics, general relativity, and Newtonian mechanics are theories. For those are based on principles and perhaps we can agree that we don't yet know the principles of quantum gravity.

The most optimistic of the papers in this volume are those by Alain Connes and his collaborator Ali Chamseddine, who have been developing an approach to particle physics and more recently quantum gravity based on noncommutative geometry. One interesting thing about this work is that it seeks to understand the details of particle physics as arising naturally from the geometry of spacetime—a kind of geometry that is noncommutative in certain directions. String theory also has its extra dimensions, but it has found a vast wilderness of possible theories, often called the "landscape", without enough principles to choose among them. This is one reason for the current pessimism in particle physics. Chamseddine and Connes, on the other hand, are seeking elegant principles that lead to the Standard Model or closely connected theories [1, 2]. It is too early to pass judgement on their attempt, but it is exciting to see work that takes the details of the Standard Model seriously and tries to explain them using deep mathematics.

I have only mentioned a few of the papers in this volume, those most engaged with current struggles in fundamental physics. There are also interesting papers on the history of general relativity, the work of Grothendieck, and other topics. Two drawbacks of the book are a plethora of typos and a paper by Michael Atiyah falsely claiming to prove a famous open conjecture: namely, that there exists no complex structure on the 6-sphere. Better editing and refereeing could have caught these problems.

References

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Department of Mathematics, University of California, Riverside CA, 92521, USA.

Centre for Quantum Technologies, National University of Singapore, 117543, Singapore.

E-mail address: baez@math.ucr.edu