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WILL M THEORY UNIFY MATHEMATICS AND PHYSICS ?

by MICHAEL R. DOUGLAS

Mathematics and theoretical physics have always enjoyed a symbiotic relationship; sometimes close, sometimes not. It is fascinating to look back on the history of the two fields and see how every possible variation has appeared. A physicist is led by his intuition of reality to ask a mathematical question; he finds it was long before anticipated and answered (take Einstein and Riemann [12]). Or, he finds it was not anticipated at all. A mathematician takes up a problem posed in physics and solves it by methods which (in Arthur C. Clarke's phrase) "seem indistinguishable from magic". (The construction of instantons by Atiyah, Drinfeld, Hitchin and Manin was regarded this way at first; as described in [4]). Or, a theory which would seem directly based on a physical formalism is ignored by the vast majority of physicists (as is still the case for the theory of operator algebras and quantum mechanics).

Despite the many points of contact and the fact that many have worked in both fields, most would agree that mathematicians and physicists have rather different ideas about what is important. Even developments of revolutionary importance in one field can go unappreciated in the other. For example, the foundations of mathematics laid in the late 19th and early 20th century such as set theory, the definition of the real numbers, and measure theory, are still ignored by most theoretical physicists, who tend to feel (despite Kronecker's dictum) that God intended us to measure distances in space using real numbers and (more recently) amplitudes in quantum mechanics using complex numbers, and that all one needs to know about them can be understood through working out real predictions one can test in the lab.

In truly revolutionary periods, this attitude is sometimes taken to extremes. The best remark on this was made by Res Jost [14]: "In the thirties, under the demoralizing influence of quantum-theoretic perturbation theory, the mathematics required of a theoretical physicist was reduced to a rudimentary knowledge of the Latin and Greek alphabets."

The passage from quantum mechanics to quantum field theory did not improve this situation. This general attitude towards mathematics was exemplified by the inventor of the most beautiful form of quantum-theoretic perturbation theory, Richard P. Feynman. Those few readers who have not heard of his opinions can find them in [8] (at least, the printable ones). As a grad student at Caltech in the mid-eighties, I had the good fortune to talk with him about this and many other subjects, and I can at least report that he knew his opinions were an oversimplification. After hearing a tirade on axiomatic quantum field theory in which he complained that nothing had ever been proven first by its practitioners, I had the temerity to mention the CPT theorem, and to my surprise he admitted right away that – yes – ONE theorem had been proven first.

Although the theory of Lie groups was appreciated early (indeed indispensable), in my opinion the real assault on this barrier began in the mid-seventies with the recognition of the importance of topology in studying gauge theory, solitons and especially instantons. As is well-known this was a true exchange initiated by physicists including Wu and Yang and mathematicians including Simons and Singer, and leading on the math side to Donaldson's spectacular work, and on the physics side to 't Hooft's resolution of the U(1) problem, an observable effect due to instantons in QCD [20], [4].

All this could not yet be said to foreshadow any "unification" of math and physics. An interesting theory had been found with important applications on both sides, but by and large the questions being asked were completely different, not to mention the approach to answering them.

Perhaps the same could be said for supersymmetry in the beginning – an idea with dramatic but strikingly different applications on both sides. It is here that we might find the beginnings of a "unification", in the work of Witten during the early 1980's. More than anyone else Witten brought supersymmetry to the mathematicians; he also gave the physicists their first clues of how ideas from mathematics could help to go beyond perturbation theory [18].

A constant obstacle to closer contact has been the frustrating (to both mathematicians and physicists) inability of the physicists to precisely define the theories they talk about. To the extent that quantum field theory could be precisely defined, it became needlessly ugly (as with lattice gauge theory). On the other hand, the most symmetric and beautiful examples of quantum field theories, the eleven-dimensional supergravity of Cremmer, Julia and Scherk [5] and the ten-dimensional super Yang-Mills theory [9], for a long time were not taken seriously by most physicists, the problems of definition were so extreme.

In 1984 the work of Green and Schwarz convinced a sizable minority of both physicists and mathematicians that superstring theory was sensible enough, beautiful enough, even (in Bohr's words) crazy enough to be worth overcoming these obstacles. It would not be long before deep and subtle mathematical results were playing a central role in physicists' discussions of the theory.

The most important of these was Yau's proof of the Calabi conjecture [21], as applied in the work of Candelas, Horowitz, Strominger and Witten in 1985 on Calabi-Yau compactification of string theory [3]. This work and the general ideas leading up to it are well discussed in [9]. Much attention had been focused on Kaluza-Klein models of observed physics, and it was learned that a pure application of their ideas could not be combined with the known supersymmetric theories: one needed to start with a theory containing both gauge and gravitational interactions in higher dimensions. String theory finally provided this; the next known difficulty was to reproduce the large ratio between the Planck scale (10^{19} GeV) and the observed scales of particle physics (such as the weak scale of 100 GeV), and the chiral nature of the observed interactions. Plausible arguments had been given that this could be accomplished with a theory with N = 1 supersymmetry in four dimensions at energies just below the Planck scale; thus the problem was to find consistent compactifications of superstring theory preserving N = 1 supersymmetry in four dimensions.

The problem of supersymmetric compactification turns out to be solved by understanding the possibilities for reduced holonomy groups in the compact manifold. The supersymmetries of heterotic string theory are parameterized by a ten-dimensional spinor; this transforms under $SO(6) \times SO(3,1)$ as (complex) 4×2 and so compactification on a flat six-dimensional M (say T^6) will have four supersymmetries or N = 4. On a curved M, the unbroken supersymmetries correspond to covariantly constant spinors on M; these will exist only if the holonomy of M is $H \subset SO(6)$ in which the 4 contains singlets. If we want N = 1, we seek a manifold of $SU(3)$ holonomy.

Happily, according to Yau such manifolds exist and are guaranteed to be Ricci-flat, thus providing solutions to Einstein's equations and consistent solutions of superstring theory. A remarkable similarity with observed physics can be obtained by starting with the $E_8 \times E_8$ heterotic string, for the gauge group of the Standard model can naturally be embedded in E_8.

Physicists were thus led to ask mathematical questions such as "Classify all the Calabi-Yau manifolds, topologically and then with a choice of complex structure" and "Can all the Calabi-Yau manifolds be connected through simple operations." The first problem remains unsolved, though extensive tables have been made [6]. The second problem was solved in a "mathematical" sense before being solved in a "physical" sense, meaning that such operations were found, but it took some time to find sensible physical interpretations for them.

This brings us to the "second superstring revolution" of 1994-95. Good reviews of these developments now exist [15], [16], [13], [7] and I will confine myself here to answering the question: what is M theory ? Superstring theory came in several different versions (type I, type II, heterotic of various types) and all were defined as perturbative expansions. M theory is the theory which admits these expansions, each in a different regime of parameter space. The argument which convinced physicists that such a theory must exist is sometimes called the "BPS" argument and is at the heart of duality: in theories with enough supersymmetry, one can derive exact formulas for the mass of certain solitons, which show that in a limit of strong coupling (where perturbative techniques must fail) the solitons become the fundamental states of the theory. During 1995 it was shown that each superstring theory contains every other type of superstring as a soliton, allowing this argument to link them all, along with eleven-dimensional supergravity, which is not a string theory at all.

Besides linking different string theories, different compactifications of string theory were also linked by these arguments. A famous example is the topology change of Calabi-Yaus described by Strominger [17]. It was a known mathematical fact that a conifold (or node) singularity in a Calabi-Yau had several distinct resolutions, producing smooth manifolds with either two-cycles or three-cycles at the singular locus. Thus, if one could show that string physics allows either resolution, topology change would be possible. Indeed it does – for example, if we start with the resolution with the three-cycle and take its volume to zero, "three-branes" (solitons with three-dimensional spatial extent) wrapping the three-cycle provide new fundamental states which take on precisely the properties expected from a manifold with the two-cycle resolution. These mechanisms have been generalized to link all known Calabi-Yaus.

At present we have a good "macroscopic" picture of M theory – at least in compactifications with enough supersymmetry (which, sadly, means at least N = 2 in four dimensions). The question of "What are all the Calabi-Yau manifolds" has broadened into "What is the complete connected moduli space of Calabi-Yau compactifications". It seems quite likely that the answer to this question is simpler than that for any one Calabi-Yau, which will ultimately allow us to solve the classification problem. Much work at present focuses on "microscopic" pictures of M theory and the attempts to isolate its truly fundamental degrees of freedom. It appears that complete definitions of M theory (in certain regimes) can be made in terms of large N limits of gauge theory and even quantum mechanics [1], [10], which may finally lead to definitions both mathematicians and physicists can be happy with.

Algebraic geometry has become an essential tool for many problems in this area. Many of us have had the following type of experience. One studies a particular class of theory – an example I was involved with is five-dimensional N = 1 supersymmetric SU(2) gauge theory [11]. Physical arguments can be used to determine properties which the moduli space and effective action must satisfy, and one then wants to know the possible examples. After talking to the right mathematicians, we learn that the same properties and moduli spaces are shared by a class of mathematical objects (say, del Pezzo surfaces) we had never heard of. Usually after a little thought, M theory tells us why this should be – in this case, M theory compactified on a degenerating Calabi-Yau leads to five-dimensional gauge theory, and the condition for the gauge group to have rank one corresponds to the condition for a del Pezzo to appear at the degeneration.

Calabi-Yau compactification of the heterotic string now appears to be just one of many possibilities. In the broadest terms the possibilities correspond to Berger's classification of the possible holonomy groups of the compact space [2]. Intriguingly, the most interesting new possibilities are the "exceptional" ones. Starting from eleven dimensional M theory, the natural choice is to use a seven dimensional manifold of G_2 holonomy, which again leads to N = 1 supersymmetry in four dimensions. Even more exotic is to use an eight dimensional manifold of *Spin*(7) holonomy; although the resulting three-dimensional space-time might seem useless for physics, Witten has argued (by analogy to other dualities) that in certain limits this space might look four-dimensional, with a naturally vanishing cosmological constant [19].

So is all this leading to a unification of mathematics and physics? It has certainly led to a new breed of mathematical physicist who finds that modern mathematics gives the best answers to his physical questions. I would also say that some of the mathematician's ways of thinking have taken hold among them – for example, the idea that a question can be simplified by finding the right generalization or the right class of objects to pose it for.

It certainly has led to far-reaching unifications of structures which mathematicians and physicists thought were unrelated. We simply have to marvel before this amazing structure, "M theory," whatever it is. Surely we have a great deal more to learn from it.

Whether it really is the particle theorists' "Theory of Everything" is too early to tell – at this point, besides the argument that it is the only theory we know which has a chance to be right (an argument from ignorance after all) there is little concrete evidence that it is right.

Still, many physicists believe it could be right, and hope that some day one will be able to derive the Standard Model with all of its idiosyncracies as one of (perhaps a few) preferred vacuum states of M theory. If that day ever comes, we will certainly witness the true unification of mathematics with physics, for the theoretical particle physicists will have to invent their own universes to continue to explore. Whether this enterprise will be called "physical mathematics", "postmodern physics", or something else, we can thankfully leave to the unforeseeable future to decide.

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