ASIAN J. MATH.
Vol. 3, No. 1, pp. lxi–lxiv, March 1999

MICHAEL ATIYAH AND THE PHYSICS/GEOMETRY INTERFACE

EDWARD WITTEN*

The first time that I met Michael Atiyah was in the spring of 1977, when he was visiting Roman Jackiw at Harvard. I was a postdoctoral fellow at Harvard, having received my Ph.D. the year before.

It is a bit hard now to recapture the spirit of the time. Theoretical physics had made major advances in the previous decade, with the nonabelian gauge theory revolution, and in a sense had caught up with experiment, though (largely because some of the weak interaction experiments were in an inconclusive state) this was not yet clear. Theoretical physicists had certainly not yet realized that the gauge theory revolution had created a situation in which it would be necessary and worthwhile to develop a greater mathematical sophistication than we were accustomed to. It took a long time to realize this. Michael Atiyah and other mathematicians who became interested in what physicists were doing in quantum gauge theory played an important role in the process.

The first major turning point, out of many, had come in 1976. The so-called $U(1)$ problem, which had been identified by Murray Gell-Mann and Steve Weinberg, among others, as the main remaining flaw in the theory of the strong interactions, was suddenly solved (in work with various contributions by Gerard 't Hooft; Claudio Rebbi and Roman Jackiw; and Roger Dashen, Curt Callan, and David Gross), using instantons. Soon afterwards, Albert Schwarz showed that some of the ingredients in the solution were best understood in terms of the Atiyah-Singer index theorem. Few of us knew what to make of this, as in the theoretical physics environment of those days, the index theorem was way beyond the prevailing level of mathematical sophistication. In fact, it seemed incredibly esoteric and obscure. But things were soon to get much more esoteric.

Much of Atiyah's visit to MIT was devoted to explaining his work with Ward applying the Penrose twistor transform to solve the instanton equations on $\mathbf{R}^4$. Solving those equations was something that many of us had been extremely interested in for the preceding year, largely because of A. M. Polyakov's speculations about the dynamics of gauge theories. The twistor approach, on the other hand, involved things that I and most of my physics colleagues had never heard of – complex manifolds, sheaf cohomology, and fiber bundles.

Atiyah invited me to visit Oxford for a few weeks – perhaps I seemed like a promising student, though I certainly had a lot of catching up to do, as I have just indicated. By the time I arrived (which was in January, 1978), the twistor transform of the instantons had been further elaborated to give the much more precise ADHM construction of instantons. Atiyah lectured on it at the Maths Institute during my visit. I remember him beginning the first lecture explaining that the trouble with working on problems posed by physicists is that once the problem is solved, one might be told that the problem wasn't quite the right one. This must have been at least partly a response to my impatience, at the time, with anything that didn't shed light on *quantum* behavior of gauge theories.

In hindsight, my focus in that period seems shortsightedly narrow to me. I also

*Institute for Advanced Study, School of Natural Science, Princeton, NJ 08540, U.S.A. (witten@sns.ias.edu).

had an impatience for quick results that must have seemed jarring to mathematicians. (I recall one of the Oxford mathematicians commenting on it in January, 1978.) At any rate, I've had no choice over the years but to learn to be a bit more patient. Learning about the ADHM construction has served me well repeatedly – especially in 1995 when it helped in understanding the problem of small instantons in the heterotic string, and the behavior of Type I fivebranes.

Towards the end of this visit, Atiyah showed me a paper by David Olive and Claus Montonen on duality in four-dimensional gauge theories. The paper was new to me, and my initial reaction was skeptical. Their conjecture was very wild, the evidence they offerred was striking but limited, and it was easy to state technical objections to the conjecture in the form in which they originally stated it. I don't know whether he was motivated in part by exhaustion from all our discussions, but at any rate Atiyah urged me to travel down to London to discuss the question with Olive. It turned out to be a very fruitful trip. By the end of the day, Olive and I had understood that the Montonen-Olive conjecture really made most sense in the supersymmetric case, and had formulated a few of the ideas that eventually (fifteen years later) were useful in understanding it better.

My recollections of discussions with Atiyah in the next few years are varied, and I will here mention only a couple of highlights. There was a conference in New Hampshire, just after Simon Donaldson's first breakthrough in four-manifold topology, where I was educated about Donaldson theory for the first time; and a conference in Texas at which Atiyah and Is Singer began to educate us about the topological meaning of perturbative gauge anomalies. That was where we physicists began to learn for the first time that we should think of the determinant of the chiral Dirac operator as a section of a complex line bundle. After 1984, string theory as well as gauge theory was prominent in all the math/physics discussions, and the two subjects have influenced each other very much; but I won't try to describe that side of things here.

In the spring of 1987, Atiyah visited the Institute for Advanced Study and was more excited than I could remember. What he was so excited about was Floer theory, which he felt should be interpreted as the Hamiltonian formulation of a quantum field theory. Atiyah hoped that a quantum field theory with Donaldson polynomials as the correlation functions and Floer groups as the Hilbert spaces could somehow be constructed by physics methods. The idea was clearly tantalizing, but I had a variety of technical objections. For example, the fermionic symmetries in Floer theory were of spin zero, as opposed to the half-integral spin of spacetime supersymmetries as studied by physicists. Even if one were willing to abandon the spin-statistics theorem, the fermions required by Floer theory, if one were to treat it as a Lagrangian field theory, did not seem to form representations of the Lorentz group. Because of these and a few other difficulties, I was skeptical, and though the idea was intriguing, I did not pursue it until I was reminded of the question during another visit by Atiyah to the Institute at the end of 1987. This time I dropped some of my prejudices and had the good luck to notice that a simple twisting of $N = 2$ supersymmetric Yang-Mills theory would give a theory with the properties that Atiyah had wanted.

The other problem that Atiyah recommended for physicists in the years 1987-8 was to understand the Jones knot polynomial via quantum field theory. It was from him that I first heard of the Jones polynomial. There followed other clues in 1987-8 about the Jones polynomial and physics. For example, A. Tsuchiya and Y. Kanie had connected some braid representations that arise in conformal field theory with the ones studied by Vaughn Jones. I didn't understand too much of this paper, which I perhaps had been shown by Dan Friedan and Steve Shenker, but I tried to pay attention to it because of Atiyah's suggestion. The nature and relation to physics of the braid representations was greatly clarified in conformal field theory work in 1987-8 by Erik Verlinde and then Greg Moore and Nathan Seiberg. I was lucky that much of this work was done at the Institute (by Moore and Seiberg) which made it much easier to follow what was going on.

In the summer of 1988, the International Congress of Mathematical Physicists was scheduled in Swansea. I knew that Atiyah, Graeme Segal, and other mathematicians interested in the Jones polynomial would be there, and I knew in particular that Atiyah considered it a major piece of unfinished business to understand the Jones polynomial in terms of quantum field theory. So by way of preparation I sat down in the week before departure with a whole pile of papers on the Jones polynomial and its generalizations that various mathematicians had sent me. It was discouraging, since the papers seemed very deep, and it looked like it would take a lifetime to understand all that. Another paper I saw in the week before the meeting – without connecting it at the time with the papers on the Jones polynomial – was one by Polyakov attempting to use abelian Chern-Simons theory in three dimensions to understand high temperature superconductors.

At any rate, the meeting at Swansea turned out well for me. Atiyah and Segal reminded me of the right clues (in particular Segal reminded me of some points that I think he'd actually explained the year before), and my mind wandered back to Chern-Simons theory during the lecture that Albert Schwarz was supposed to give. (With the Soviet Union nearing collapse, he was the one speaker not permitted to attend the meeting; his lecture was read by Igor Krichever.) Some important points fell in place during a memorable dinner at Annie's restaurant with Atiyah and Segal.

In many ways, this work relating the Jones polynomial to Chern-Simons theory was a turning point in my career. For one thing, I learned that while it might indeed take a lifetime to master all the learnedness in that pile of papers that I had been looking at, the piece of the story that I was suited for personally did not require all that. It required focussing on the right questions and, at times, listening to the right advice.

Going back to Donaldson theory, the quantum field theory formulation of this subject did not lead to any immediate progress. I felt in the years 1988-90 that the Lagrangian representation of the theory would make it possible to perform computations by purely formal, short distance or weak coupling methods. It took me several years to become convinced that this would not work. In fact, Atiyah and other mathematicians helped me on several occasions in understanding that the sort of results I could get that way were more or less along the lines of what mathematicians were anyway doing by more standard (and of course rigorous) mathematical methods. Thus to get somewhere it would be necessary to supply some more physical ingredient.

Though I was extremely reluctant to accept this, it eventually became obvious that the missing ingredient would have to be a knowledge of what physicists call the dynamics of the $N = 2$ quantum field theory. I think that, although he might not have expressed the point in exactly those words, this is essentially what Atiyah was hoping for during those years. Anyway, Seiberg and I had the great good fortune of understanding the $N = 2$ dynamics in the spring of 1994. The most interesting aspects of the dynamics were described in terms of an effective $U(1)$ gauge theory with "monopoles." The monopoles in question are the same ones that star in the book *The Geometry and Dynamics Of Magnetic Monopoles,* by Atiyah and Nigel Hitchin, except that they and other mathematicians (and physicists) have studied monopoles as classical solutions of a nonlinear PDE, while to understand the $N = 2$ dynamics, it is necessary to understand how the monopoles behave in a region of parameters where the quantum effects are big and the classical PDE is not a good approximation. Nevertheless, many features of monopoles described in the Atiyah-Hitchin book are relevant to $N = 2$ dynamics and were later used in checking features of the quantum behavior.

It was fairly evident that the work on $N = 2$ dynamics with Seiberg should lead to a new description of Donaldson theory. To actually elaborate the new description still took some time. Yet another visit to the Institute by Atiyah – in the late spring of 1994 – helped sharpen my ideas about this.

I have tried to recount a few of the highlights of my scientific interactions with Michael Atiyah, and to convey a little of the role he played in encouraging us to study quantum field theory from new points of view. We had to learn a lot of lessons before taking these new perspectives seriously. Atiyah, along with colleagues such as Raoul Bott and Is Singer, played an important role in teaching some of these lessons to the physics world. Atiyah has always believed intuitively that the study of quantum field theory as a tool in geometry had to be integrated with the study of more "physical" aspects of quantum field theory. This was one of the hardest lessons for me personally to learn.