CELEBRATIO MATHEMATICA

Michael F. Atiyah

NIGEL HITCHIN

MATHEMATICS AND CULTURE: GEOMETRY IN OXFORD 1960–1990

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Background

When J. J. Sylvester, the Savilian Professor of Geometry at Oxford University, died in 1897, research in geometry virtually stopped. His successor, W. Esson, was more concerned with University business and the reform of teaching methods, and when G. H. Hardy took the position in 1920 all was analysis, continued by E. C. Titchmarsh who held the post from 1932 to 1963. Titchmarsh's letter of application stipulated that he would only accept if he did not have to teach geometry, and, though Hardy did indeed engage in his undergraduate geometrical duties, this was at least consistent with his view: "I do not claim to know any geometry, but I do claim to understand quite clearly what geometry is" [Atiyah 2000].

It fell to J. H. C. Whitehead, who became the Waynflete Professor in 1947, to wear the mantle, though it was geometry in the guise of topology which changed the face of research in the University. Whitehead, born in Madras on November 11, 1904, had studied with O. Veblen in Princeton from 1929 until 1932 and obtained his Ph.D. there in 1930. Together, they wrote a foundational book on differential geometry [Veblen and Whitehead 1932], where they notably gave a proper definition of a manifold, including the Hausdorff property. It is worth remembering that Cartan, even in 1946, considered that "the general notion of manifold is quite difficult to define with precision," and that various authors, Hodge and Weyl included, laboured over it at this time. Under the influence of Marston Morse in Princeton, Whitehead tackled global questions concerning the differential geometry of manifolds, including the now-standard proof of the existence of convex neighborhoods. He took up a Fellowship at Balliol College, Oxford, in 1932, but his interests by then had moved on to algebraic topology. Differential geometry in Oxford at the time was driven by the study of cosmological models, with E. A. Milne and A. G. Walker, and this work was essentially local.

Whitehead's interest in topology began also in Princeton, under the influence of S. Lefschetz. They wrote a joint paper together, and this set him off in a direction which was to occupy his mathematical life until his death in 1960. It was a failed attempt on the Poincaré conjecture in 1934 that committed him to the subject, and over the years he developed rigorous combinatorial and algebraic methods for attacking problems of homotopy equivalence; his name is now attached to many fundamental constructions and concepts. He gathered many students to work with him on these projects. Perhaps his ebullient character

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helped; P. J. Hilton asked him "What is algebraic topology, Henry?" and he answered "Don't worry, Peter. You'll love it!" [Hilton 2000]. A further attraction was the widely known fact that his interests were equally divided between beer, cricket and mathematics [Atiyah 2000].

During the Second World War, like many of his contemporaries, Whitehead worked as a codebreaker at Bletchley Park, but, after the war and particularly after being appointed to the Chair, he gathered around him a group of algebraic topologists, so that Oxford was an important centre for the subject. Hilton and I. M. James were two of his early students. In 1957 James was appointed as Reader, and a few years later M. Barratt and C. T. C. Wall had short-term positions there. A reflection of the strength of the subject is Whitehead's approach in the late 1950s to Robert Maxwell, chairman of Pergamon Press, to start a new Oxford-based journal to be called *Topology* [James 1989], still the leader in the field.

Michael Atiyah

When Whitehead died suddenly in 1960 at the age of 55, there were 12 graduate students in algebraic topology in Oxford, both his and those of Ioan James. The vacant Waynflete Chair was soon filled by one of Whitehead's former students, but one whose interests were now group-theoretical rather than topological — Graham Higman. There was one unsuccessful applicant, the 32-year-old Cambridge mathematician M. F. Atiyah, who was nevertheless offered a Readership as a consolation, which he accepted partially to be relieved of his college teaching in Cambridge. Within two years, Titchmarsh had died and Michael Atiyah became the Savilian Professor of Geometry, restoring the Chair to its original purpose.

Atiyah's background at the time was in algebraic geometry. He had been a student of W. V. D. Hodge in Cambridge, who pushed him to learn about vector bundles and characteristic classes, together with all the new points of view and techniques that were emerging in those postwar years. There was the work of Serre, and Kodaira and Spencer, on sheaf cohomology; Chern, Allendoerfer and Weil, on characteristic classes and curvature; and Hodge's older work on harmonic integrals, given a new rigour by the analytical methods of Weyl and Kodaira. He had then spent time at the Institute for Advanced Study in Princeton, where he met R. Bott, F. Hirzebruch and I. M. Singer, J.-P. Serre and A. Borel. Until 1959, most of his papers were in algebraic geometry, but then became more topological. Topology was all around, going through a big period, and, although he didn't regard himself as a topologist, he "just picked it up" [MFA 2005].

It was the work of Hirzebruch that shifted his attention from algebraic geometry to topology, for he had linked the Riemann–Roch theorem to the topological Chern numbers, and was producing, via the cobordism theory of R. Thom, combinations of characteristic numbers for differentiable manifolds, not just projective varieties, which were integers. Bott's periodicity theorem, together with questions of James in topology and A. Grothendieck's work on the general Riemann–Roch theorem, all conspired to give birth to K-theory [MFA 2005]:

> I saw that by mixing all these things together you ended up with some interesting topological consequences, and because of that we then thought it would be useful to introduce the topological K-group as a formal apparatus in which to carry this out.

It was a bold approach, drawing inspiration from the generalities of Grothendieck, but it introduced many new features, in particular, the odd groups which do not appear in algebraic geometry.

Before moving to Oxford, Atiyah had already, with his topological hat on, given seminars which had generated great interest from Whitehead and James, and he arrived there essentially as a topologist. By 1963 he had nine research students, most of whom were considered as algebraic topologists, though one was described as a differential geometer — the Australian mathematician G. B. Segal, sent from Cambridge by Hodge. He has described the atmosphere around Atiyah at the time [Segal 2003]:

> He would direct his abounding energy at each of us in turn. I remember how inspired I felt after each meeting, but on the whole we students used to hide from him, for if he ran into us in the corridor and found that we hadn't made much progress with yesterday's suggestions he would pour forth a torrent of new lines for us to try. At the same time he always left us feeling there was something worthwhile we could do; however wrong were the ideas we came up with, he never crushed us, but made our muddle seem like steps in the right direction.

The index theorem

The most important work that Atiyah did, when he arrived in Oxford, was the formulation and proof of the index theorem jointly with Singer, then at MIT. This contributed to his Fields Medal in 1966, and was recognized by the award of the 2004 Abel Prize to Atiyah and Singer. In one sense, it grew naturally out of his work on K-theory [MFA 2005]:

> There was a kind of zigzag pattern backwards and forwards where different versions of the proof of the index theorem and bits of K-theory helped each other along, until in the end they were so mixed up I didn't know whether I was going from left to right or right to left.

It was not only topology and analysis that were linked by the theorem, but ultimately algebraic geometry, number theory, differential geometry and theoretical physics!

A linear operator on a Hilbert space is called *Fredholm* if it has a finite-dimensional kernel and cokernel. The difference of these dimensions is called the *index*, and, although the individual dimensions may jump in a continuous family, the difference is unchanged, and is thus a topological invariant. The problem of calculating this was posed by mathematicians in the school of I. Gelfand, though Atiyah was at first unaware of this. The Russians had calculated some simple examples in terms of the topological degree of a certain map.

For Atiyah, by contrast, the problem arose from his visits to Bonn to talk with Hirzebruch. Hirzebruch's work had shown that certain combinations of characteristic numbers were necessarily integers. These had originally come from algebraic geometry where, with Hirzebruch's talent for calculations, he had interpreted them as alternating sums of dimensions of certain sheaf cohomology groups. Yet the numbers were still integers for almost-complex manifolds — the purely topological version of algebraic geometry where sheaf cohomology made no sense. Not only that, but there were similar results for ordinary smooth manifolds. In particular, Hirzebruch had shown that, for a manifold whose Stiefel–Whitney class $w_2(X)$ vanishes, the particular combination of characteristic classes called the $\hat{A}$ genus is an integer. At the time, these subtle integrality theorems sat very naturally in the language of K-theory, but there was no genuine explanation for why they were true. As Atiyah commented, "We had the answer; we didn't know what the problem was" [MFA 2005].

When Atiyah arrived in Oxford, despite his prestigious chair, there was very little money to invite people to stay. The few pounds necessary to invite Serre across the Channel were only begrudgingly granted. But in January 1962, Atiyah's first academic year in Oxford, Singer decided to spend time there. It was family considerations that forced him to change his previous sabbatical plans, but he remembered their friendship from the Institute in Princeton in 1955 and called to see if he could come on his own money. He was of course welcome, and then [Singer 2003]:

> ...on my second day at the Maths Institute you walked up to the fourth floor office where I was warming myself by the electric heater. After the usual formalities, you asked "Why is the genus an integer for spin manifolds?"
>
> "What's up, Michael? You know the answer much better than I."
>
> "There's a deeper reason," you said.

By March, they had found a candidate for what the answer must be — the index of the Dirac operator. This was an elliptic operator only defined on manifolds which satisfied the spin condition $w_2(X) = 0$, and so fitted in with Hirzebruch's theorem. In a way, they had rediscovered the operator since physicists were already familiar with it, and in fact Atiyah had attended Dirac's lectures in Cambridge as a student but did not follow the work very closely. In any case, there was a huge difference between the Euclidean signature of Riemannian geometry and the Lorentzian signature of relativity. Nowadays physicists are quite happy to work in the Riemannian domain, but this was not the case in the 1950s.

The easiest example of the index theorem is the Gauss–Bonnet theorem, which expresses the Euler characteristic of a compact manifold as a curvature integral which realizes a topological characteristic class, the Euler class of the tangent bundle. The Fredholm operator here is $d + d^*$, where d is the usual exterior derivative and d^* its formal adjoint, and one considers it mapping even forms to odd ones. The kernel consists of the harmonic even forms, and the cokernel, which is the kernel of its adjoint, of the harmonic odd forms. Using Hodge's theorem, the index is the sum of the even Betti numbers minus the sum of the odd ones, which is the Euler characteristic. Of course, $d + d^*$ is an unbounded operator, so a considerable amount of analysis is needed even to rephrase this theorem rigorously as an index problem.

Another example is the signature operator on differential forms. Instead of decomposing the differential forms into odd and even ones, there is another way to do it in $4k$ dimensions, using the Hodge star operator of a Riemannian metric. The same operator $d + d^*$ maps one space to another, and now the index is the *signature* of the manifold: in $4k$ dimensions, the intersection form on the middle-dimensional cohomology is symmetric, and the index is the signature of this form. Hirzebruch had expressed this invariant as a particular combination of Pontryagin numbers in his 1953 paper.

Singer brought analytical expertise into play, as well as differential geometry know-how, but they also had a lead when S. Smale passed through Oxford on his way back from Moscow. He told the pair about Gelfand's work on indices of operators, and following up on this Atiyah and Singer were able to consult the analytical papers of M. S. Agranovich, A. S. Dynin and R. Seeley, and also rely on friends such as L. Nirenberg and L. Hörmander. As Atiyah subsequently admitted [MFA 2005],

> I did actually make attempts to … read a few serious books. They were the first books I'd actually tried to read since I was a student. After you've ceased being a student you don't usually read textbooks; you learn what you need to on the hoof.

The advantage they had over the Russians was that they were concentrating on a particular operator, the Dirac operator, and they knew what the answer should be. They also knew the answer for related operators, such as the signature operator and the Dolbeault operators on a complex manifold. The index theorem for each of these cases would give new proofs of the Hirzebruch signature theorem and the Riemann–Roch theorem, respectively. Perhaps more importantly, they had seen the problem in the context of K-theory, and that was where the link really lay — the index of an elliptic operator only depends on its highest-order term, the principal symbol, and this immediately defines a K-theory class.

Atiyah and Singer discussed the problem intensively in Oxford, walking together in the University Parks, or up the River Thames to the Trout Inn. A proof was finally completed in the Autumn of the same year as Atiyah visited Harvard, and the results were presented in a seminar run by Bott and Singer, subsequently expounded in detail in the Princeton seminar [Palais 1965]. The proof was based on Hirzebruch's 1953 proof of the signature theorem: both the signature and the characteristic number it is supposed to equal are the same for two oriented manifolds which form the boundary of a third — such a pair is said to be *cobordant*. So, proving the theorem can be reduced to the evaluation of special cases which generate the cobordism classes. For the index problem, one has to describe its change under cobordism, and this required an extension of elliptic boundary-value techniques to singular integral operators.

The proof was not ideal, though [MFA 2005]:

> What was wrong with the first proof, besides being … conceptually a bit unattractive, because you verify things, was that it didn't include some generalizations that we had in mind.

These included the equivariant case, where a group acts on the manifold, and the case of real operators rather than complex ones. In these cases, the index is not simply an integer — it takes values in the representation ring of the group in the first case, and is an integer mod 2 in the second. The case of families of elliptic operators was not dealt with in the first proof, and here the index is an element in the K-theory of the space parametrizing the family.

So, over the next five years, Atiyah and Singer produced new proofs, this time based on Grothendieck's proof of the Riemann–Roch theorem using embeddings and projections, and in a series of *Annals* papers they dealt with all the other ramifications, including fixed-point theorems, with many applications. Their work also fed backwards into the purely topological world — it became immediately apparent that the K-theory of a space X was actually the set of homotopy classes of maps from X to the space of Fredholm operators. At another stage, the authors arrived at Bott's periodicity theorem for the homotopy groups of the classical groups, without being aware that they had used it, and thereby discovered an elementary proof of this result. And then, the fixed-point formula gave a proof of the Weyl character formula for representations of a compact Lie group. By focusing initially on that single problem of the Dirac operator, they had opened up a theory which lay at the crossroads of an enormous number of disciplines. In fact, it turned out that, for the index theorem, the Dirac operator is fundamental, and coupling it to an arbitrary vector bundle yields all possible indices.

The Abel Prize citation for this work says it all:

> Atiyah and Singer are receiving the prize for having discovered and proved the index theorem, which links together topology, geometry and analysis, and for playing an extraordinary role in building new bridges between mathematics and theoretical physics. The index theorem was proved in the early 1960s and is one of the most important mathematical results of the twentieth century. It has had an enormous impact on the further development of topology, differential geometry and theoretical physics. The theorem also provides us with a glimpse of the beauty of mathematical theory, in that it explicitly demonstrates a deep connection between mathematical disciplines that appear to be completely separate. Sir Michael Atiyah and Isadore Singer have demonstrated mathematics at its very best and are worthy winners of the Abel Prize.

The five *Annals* papers began to appear in 1968, but by 1969 Atiyah had left Oxford for Princeton, to become a permanent member at the Institute for Advanced Study. However, in 1973, and holding a Royal Society Research Professorship which freed him from administrative duties, he returned. That same year, another Chair was filled in Oxford — Roger Penrose became the Rouse Ball Professor of Mathematics.

Roger Penrose

Roger Penrose, born on August 8, 1931, had been an undergraduate at University College, London, but was a contemporary of Michael Atiyah as a graduate student in Cambridge. For one year, they had shared the same supervisor, Hodge, but then Penrose moved over to work with J. A. Todd, Atiyah's director of undergraduate studies. So, while Atiyah was learning about harmonic forms and differential geometry, Penrose's work was concerned with classical invariant theory. He invented a diagrammatic way of keeping track of indices in tensorial calculations, not far removed from Feynman diagrams. In fact, this pictorial analysis of mathematics is a constant feature of his work: some of the illustrations for his latest book were exhibited at the Royal Academy Summer Exhibition in London in 2004!

Whereas geometry was his formal topic of study (and continued to the extent that he wrote, with Whitehead and E. C. Zeeman in 1960, a paper about embedding manifolds in Euclidean space), under the tutelage of D. Sciama in Cambridge he became more and more interested in physics. By the time he went to Oxford, he had made his name with S. W. Hawking on singularity theorems in relativity — in 1965, using topological methods, he found conditions, which he called the existence of a trapped surface, that proved that a singularity must occur in a gravitational collapse. But he was also developing an approach to the equations of mathematical physics which few understood and was what he termed *twistor theory*.

Giving a talk at Princeton University, just before both he and Atiyah moved to Oxford, he spoke to his former classmate, downplaying the topological aspects of the singularity theorems. It seemed as if they would have little in common in Oxford. But Freeman Dyson had heard of twistors, and in Princeton he had told Atiyah: "Twistors are a mystery ... but perhaps you will understand them" [Atiyah 1998].

In Oxford, both Atiyah and Penrose quickly accumulated graduate students — something that Atiyah missed in Princeton — and continued their research, apparently pursuing different themes. Fairly soon, however, they got talking about twistors. The Klein correspondence between lines in three-dimensional projective space and points of a four-dimensional quadric, they both knew from their Cambridge days, but Penrose's use of complicated contour integrals to produce solutions to zero rest-mass field equations was very new to Atiyah. Penrose patiently explained the rules whereby one could change the integrand or the contour. And then [Atiyah 1998],

> It was not long before it dawned on me that Roger was essentially struggling with sheaf cohomology, but did not realize it. Once this was pointed out, Roger and his students became fervent converts. After a few private seminars in my study they really took off. Within a short period of time Roger's group were more expert with sheaf cohomology than I had ever been.

Penrose's new viewpoint was motivated by the desire to put the complex numbers into the theory at the very beginning. There were a number of reasons: one was that quantum theory demands the use of complex numbers; another was the observation that the celestial sphere is in fact a complex object. It is naturally the Riemann sphere — the complex numbers together with a point at infinity — but, more importantly, two relativistic observers relate their views of the celestial sphere by a holomorphic transformation, or complex Möbius transformation. Also, he hoped that replacing the points of spacetime by something more fundamental might be a gateway to nonlocality.

To describe twistor theory, one begins with a complex four-dimensional vector space T with a Hermitian form of signature $(2, 2)$. The vectors in T are the *twistors*. The two-dimensional subspaces of T, or equivalently the lines in the three-dimensional projective space $P(T)$, are classically parametrized by the points of a four-dimensional projective quadric. The unitary group $SU(2, 2)$ of the Hermitian form acts on the quadric as the group $SO(4, 2)$, which is the conformal group of Minkowski space. In fact, the equation of the quadric has real coefficients, and the real points can be identified with a conformal compactification of Minkowski space.

What is achieved here is that the points of spacetime are described by complex projective lines in complex projective three-space, and the miracle of twistor theory is that the objects and constructions that one naturally studies in algebraic geometry transform into some of the well-known equations of mathematical physics. This is where those sheaf cohomology groups come in. The Hermitian form divides $P(T)$ into two halves, $P(T)^{\pm}$, and an element of the sheaf cohomology group $H^1(P(T)^+, \mathbb{O}(-n))$ defines a solution to a field equation of spin $(n-2)/2$. The cases $n = 3$ or $n = 1$ give the Dirac equations with the two possible chiralities — the Riemannian version of which was so fundamental to Atiyah's work on the index theorem. The case $n = 2$ is the wave equation.

The simplest case is the static solution to the wave equation — a harmonic function in three dimensions. Here, Penrose's contour integral involves a holomorphic function $f(w, z)$ of two variables, where $z \neq 0$. This may be changed by adding on a holomorphic function of w and z, or z^{-2} times a holomorphic function of w and z^{-1}. This process is essentially the Čech definition of a sheaf cohomology group. To obtain the value of the corresponding harmonic function φ, the contour integral is:

$$\varphi(x_1, x_2, x_3) = \int_C f\big((x_1 + ix_2)z^2 + 2x_3 z - (x_1 - ix_2),\ z\big)\, dz.$$

This formula was actually given by E. T. Whittaker in 1903, and the wave-equation equivalent a year later by H. Bateman, but the new, general point of view was to generate means of solving equations far more difficult than these.

This use of complex analysis was very attractive to Penrose [Garcia Prada 2000]:

> I feel strongly that complex numbers and complex analytic structures are fundamental for the way that the physical world behaves. I suppose that part of my reason for this goes way back to my mathematical training. When I first learnt about complex analysis at university in London I was totally gob-smacked — it just seemed to me an incredible subject.

The fact that Penrose's students were able to absorb the techniques of homological algebra — exact cohomology sequences, spectral sequences and so on — which the sheaf-cohomology interpretation of twistor integrals opened up, was due to the cohesive nature of the twistor-theory group. There were many graduate students, but they shared a common ethos. Regular all-day meetings were held on Fridays, there was a privately circulated hand-written *Twistor newsletter*, and a "Problem Library" where students could take out a question for two weeks and return it if they couldn't solve it in the allotted time. New ideas very quickly spread through the group this way.

However successful the contour integrals were, Penrose's hopes for twistor theory were more in General Relativity and Einstein's equations. By 1975, he had partially achieved, with Atiyah's help, this goal. His idea was to take an open set in the standard three-dimensional projective space $P(T)$ and deform it as a complex manifold, retaining the projective lines. Since the intersection properties of the lines in projective space determined the conformal structure of Minkowski space, one should get a deformed conformal structure. The key mathematical ingredient was the assertion that the deformation would keep the projective lines, and for this Atiyah provided the references to Kodaira's papers of the early 1960s which gave sheaf-theoretic conditions for this to work. In the paper [1976], Penrose produces a construction of complexified solutions to the Einstein vacuum equations this way.

In terms of the ultimate goal, there was a catch. The metrics have self-dual Weyl tensor, but in a Lorentzian spacetime the two halves of the Weyl tensor are complex-conjugate, so self-duality implies flatness: only Minkowski space fits the bill, and the obvious link with relativity was lost. There was one use of this in General Relativity, though, using the H-space point of view of E. T. Newman on asymptotic spacetimes, but in many respects Penrose's "nonlinear graviton" seemed a purely mathematical construction with no application. It was nevertheless a solution to a highly nonlinear differential equation achieved by geometrical means, and twistor theory would provide more of these.

Instantons

By 1976, it was not just Penrose's work which led Atiyah to pay more attention to what physicists were doing. Through Singer, he had learnt that their work on the index theorem for the Dirac operator was intimately tied up with questions about anomalies in quantum field theories. In fact, there had been a parallel development: theoretical physicists across the corridor from Singer's office in MIT had been essentially re-deriving the index theorem, but in the opposite direction from the mathematicians. Atiyah and Singer had started with the integer index and worked through different analytical approaches, the cobordism proof, the Riemann–Roch proof and then, in the early 1970s, the heat-equation proof (to deal with nonlocal boundary-value problems); the physicists had started at the other end.

By the end of 1976, Singer was aware of the interest of physicists in the Yang–Mills equations and, in particular, the finite-action solutions on $\mathbb{R}^4$, called Yang–Mills *instantons*. These were equations for connections on vector bundles, something that differential geometers had been familiar with for years, but not these equations nor the notion of gauge equivalence. In early 1977, Singer came to Oxford again for several months, and began to give weekly lectures on Yang–Mills theory: the basic problem, the examples of G. 't Hooft and R. Jackiw, the group of gauge transformations, conformal invariance and translating the problem to the sphere.

At the same time, Richard Ward, a South African student of Penrose, had been looking at these same equations from the twistor point of view. He was scheduled to give a seminar on his work; Penrose suggested to Atiyah that he might be interested, and indeed [Atiyah 2000]:

> ... at the last minute I decided to go. You know, you go to a lot of seminars, and I suppose ninety percent of the time you get something out of them, and sometimes it's a bit boring, but every now and then something really interesting happens ... I was really terribly excited by what I had heard, and wanted to understand it in my own language, and then I went away and spent a hard weekend trying to follow through the implications ... and then I saw how one could use it for getting global solutions on the four-sphere.

Ward's interpretation of the self-dual Yang–Mills equations was that complex solutions corresponded to holomorphic vector bundles on projective twistor space. Atiyah's excitement stemmed from various sources. One was that his first paper was about holomorphic bundles, and his student R. Schwarzenberger had written his thesis about such bundles on projective space. But he was also aware that there was considerable current work by G. Horrocks in Newcastle and W. Barth in Erlangen in this area. Another link related to a discussion Atiyah had had some time earlier, with students of Penrose, on what twistor theory meant if you replaced Minkowski space by Euclidean space, whose conformal compactification was the four-sphere. The picture he had found was very simple: it meant considering the four-dimensional complex vector space T as a two-dimensional vector space over the quaternions. A one-dimensional complex subspace then generates a one-dimensional quaternionic one, and so there is a projection map from $P(T)$ to the quaternionic projective line, which is the four-sphere.

These facts fitted the instanton problem very well, since the Yang–Mills connection extends by conformal invariance of the equations to the sphere, and defines directly a holomorphic structure on its pullback to the projective space. Conversely, by Ward's theorem, this gives back the connection. The finite-action boundary conditions translate very neatly into the existence of a global holomorphic bundle on projective space. Together with Ward, Atiyah used a construction of bundles due to Serre to get solutions to the equations, but it was still not clear how general these examples were.

Singer's seminar each week absorbed these new points of view, and also one of the writer, concerning the infinitesimal deformations of the instanton equations. This was a simple application of the index theorem, coupled to a differential-geometric vanishing theorem dependent on the positive scalar curvature of the sphere. It gave the expected dimension of the moduli space for instantons with topological charge k

as $8k-3$. As Singer then pointed out, an adaptation of Kuranishi's arguments for moduli of complex structures showed that there was a genuine smooth moduli space of this dimension. The dimension had also been calculated independently by the Russian physicist A. S. Schwartz. Since this dimension was bigger than the algebraic-geometric constructions to date, there clearly had to be another way to obtain all instantons.

The algebraic geometers had already found, in a different language, such a way, for the previous summer Horrocks had produced a linear-algebra construction of certain holomorphic bundles on projective spaces, and Barth had used this to show that, if E was any rank-2 holomorphic bundle with vanishing first Chern class on projective three-space, and satisfied a certain sheaf-cohomological condition, then one could construct it using Horrocks' concrete techniques. The condition was the vanishing of the sheaf cohomology group written as $H^1(P^3, E(-2))$. For the holomorphic bundle coming from an instanton, this space can be interpreted, by a modification of Penrose's original contour integrals, as the space of solutions to a certain Laplace-type equation on the sphere, and again the positivity of the curvature of the sphere forces it to vanish. Thus, using Barth's result, *every* solution to the self-dual Yang–Mills equations on the sphere can be expressed from linear-algebraic data.

These pieces of the jigsaw puzzle were finally assembled by Atiyah and the writer before going off to have lunch at St. Catherine's College on November 22, 1977. On our return to the Mathematical Institute, we found a letter from Yu. Manin giving essentially the same construction, with V. G. Drinfeld. A joint paper was published and the method became known as the ADHM construction of instantons.

This interaction between mathematics and physics was a significant event, especially for Atiyah [Atiyah 1988]:

> Around this time I was in fact giving lectures in many parts of the world on the geometry of gauge theories. I think it is fair to say that the papers had caused quite a flurry of interest on the mathematics/physics interface. It is reported that Polyakov had described (ADHM) as the first time abstract modern mathematics had been of any use!

In time, most concepts in mathematics become obvious. Nowadays, the ADHM construction can be seen, in the formalism of hyperkähler moment maps, as a sort of Fourier transform of the original Yang–Mills equations, and fits into a number of such constructions. And here is a physicist's view [Dorey et al. 2000]: "The arcane ADHM construction of Yang–Mills instantons can be very naturally understood in the framework of D-brane dynamics in string theory." Or, as E. Witten has written [2003],

> Learning about the ADHM construction has served me well repeatedly — especially in 1995 when it helped in understanding the problem of small instantons, and the behaviour of Type I fivebranes.

Atiyah had met Witten, then a Junior Fellow at Harvard, in the Spring of 1977, and his subsequent interactions with physics were virtually always fed by conversations between the two.

Simon Donaldson

In October 1980, after a recommendation that he was "the best student in ten years," Simon Donaldson began his mathematical life in Oxford; six years later, he had won a Fields Medal.

He was born on August 20, 1957, and obtained his first degree at Pembroke College, Cambridge. Of his experiences in Oxford at this time, he has said [Donaldson 2003]:

> The early 1980s was a golden age for geometry in Oxford, or at least it seems so to me and probably to all who were lucky to be a part of the group led by Atiyah at that time. This was a sizeable group — among the faculty were Graeme Segal, Nigel Hitchin, Brian Steer, Glenys Luke, George Wilson and (somewhat later) Simon Salamon and Dan Quillen. Research students included Frances Kirwan, Michael Murray, Michael Pennington, Jacques Hurtubise, John Roe (contemporaries of the writer) and a little later, Yat-Sun Poon, Henrik Pedersen, Peter Kronheimer and Peter Braam — with interweaving research interests. There were also many interactions with the equally large and active group of mathematical physicists working with Roger Penrose. For us research students the weeks (at least during the short Oxford terms) revolved around Atiyah's "Geometry and Analysis" seminar, which met each Monday at 3pm … The most memorable of these seminars were those given by Atiyah himself, which were invariably virtuoso performances.

Since the flurry of interest over instantons, research in geometry was following a number of different paths. The year 1977 saw another visitor to Oxford, Raoul Bott, on his way back from a stay at the Tata Institute in Bombay. He had been exposed to work on the moduli spaces of stable bundles on Riemann surfaces, a subject virtually invented there some years earlier. Talking to Atiyah, he wondered if the magical Yang–Mills theory would do anything for this problem. The solutions to the Yang–Mills equations were essentially trivial in two dimensions — flat connections or connections with constant curvature — but what they wanted to do was derive the known formulas for the cohomology of the moduli space of stable bundles by Morse-theoretical means using the Yang–Mills functional. A key observation was the moment map interpretation, in an infinite-dimensional setting. The space of *all* connections on a bundle over a closed surface is formally a symplectic manifold, and the action of the group of gauge transformations preserves the symplectic form. It has a moment map, which is the curvature of the connection, and so the moduli space of flat connections is formally the quotient of the zero-set of the moment map by the action of the group — a symplectic quotient, or reduced-phase space, which inherits a symplectic form.

With a choice of complex structure on the surface, this viewpoint becomes an infinite-dimensional analogue of the action of a group on a projective variety, and there it was known that stability of points under the action of the complex group was related to the moment map for its maximal compact subgroup. The famous theorem of Narasimhan and Seshadri from 1964, that a stable holomorphic bundle on a Riemann surface has a natural flat connection, could now be seen in a very natural light through the use of moment maps. So, Atiyah, Bott and some of Atiyah's students, notably Frances Kirwan, were applying themselves actively to finite- and infinite-dimensional moment-map problems.

The writer, by contrast, was interested in developing further the manifestations of twistor theory in Riemannian signature. Hawking and G. W. Gibbons had in 1976 given simple constructions of complete solutions to Einstein's equations in positive-definite signature, and these turned out to be self-dual, which meant that Penrose's twistor methods could be applied to them — General Relativity could not fully benefit from twistor theory, but Riemannian geometers could. The twistor spaces of Hawking's metrics were easy to construct, and so what came into being seemed at the time like a coherent class of objects, with plenty of examples: one should look at four-manifolds with a self-dual conformal structure and self-dual connections over them. These were some sort of quaternionic analogues of Riemann surfaces and holomorphic line bundles. That Penrose's twistor theory applied to these meant that ultimately one was doing complex-analytic geometry, which put the area on a solid footing, together with a range of available techniques.

There were, however, indications in other directions. One was the result of C. Taubes [1982] about the existence of self-dual connections on non-self-dual manifolds. The impact of this was in some ways comparable to the Hirzebruch integrality theorems that motivated the general index theorem. Taubes' analytical existence theorem came out in 1982, but preliminary versions were circulating much earlier than that. He showed that, on any four-manifold with positive-definite intersection form, one could find self-dual $SU(2)$ connections. His construction yielded solutions of topological charge k, whose curvature was concentrated around k points.

Another aspect which pointed away from self-dual spaces was the role of special connections on holomorphic bundles over a Kähler manifold. A choice of Hermitian metric on a holomorphic bundle naturally defines a connection, and one is interested in such metrics where the curvature of the connection, which is a matrix of two-forms, is orthogonal to the Kähler form. In two complex dimensions, this is the same as the anti-self-dual Yang–Mills equations. It was conjectured by the writer at a Taniguchi Symposium in 1979 that such connections should exist on stable holomorphic bundles, a conjecture also advanced by others, in particular S. Kobayashi. There was evidence for the conjecture; the Narasimhan–Seshadri theorem was one. There were also concrete examples: the instanton connections on the four-sphere, pulled back to projective space.

It seemed then that self-dual spaces did not have to be the unique setting for studying the Yang–Mills equations.

Simon Donaldson became the writer's research student in 1980, and was given the task of attempting to prove the conjecture about stability. He absorbed various analytical approaches during his first year, studying Eells and Sampson's work on harmonic maps and Yau's work on the Calabi conjecture. After observing that there was a moment-map description of these equations, he transferred to Atiyah's group and enlisted in the moment-map activity there. His first success was a new, more analytical but more elementary, proof of the theorem of Narasimhan and Seshadri, using the moment-map formalism; but then, in the Autumn of 1981, he had a radically new idea which turned the subject of instantons on its head.

As Donaldson recalls [1997],

> I studied Taubes' paper in detail in 1980–81: it fitted in with my thinking about Hitchin's problem in the following way. In studying the nonlinear heat equation mentioned above the essential thing was to obtain analytical compactness theorems which would allow one to get some kind of limit. This is closely related to understanding the compactness of instanton moduli

> spaces: now the algebro-geometric literature contained various examples of moduli spaces of stable bundles, and one can observe in these examples that the moduli spaces have natural compactifications in which one adjoins points "at infinity," made up of configurations of points in the underlying complex surface. It was therefore natural to make the hypothesis, assuming the conjectured relation between instantons and holomorphic bundles, that instanton moduli spaces over general 4-manifolds should be compactified by adjoining configurations of points.

This hypothesis was supported by Taubes' construction — his instantons were near the boundary of the moduli space. The standard example of a moduli space of instantons was the case of the four-sphere with charge 1. In this case, the $8k-3=5$-dimensional moduli space is an orbit of the conformal group of the sphere, $SO(5,1)$ — it is naturally hyperbolic 5-space, and its boundary is the sphere itself. Moreover, the explicit energy densities of the instantons can easily be seen to converge to a Dirac delta function at a point as one approaches the boundary, so this model fitted in with Donaldson's ideas — the four-manifold bounds the moduli space. In the case where the intersection form of a manifold X is positive definite, the index theorem predicts the dimension of the charge-one moduli space to be 5 again, so it looked as if the boundary of the moduli space would be the four-manifold itself in this case. But Donaldson knew enough about cobordism theory to realize that, for example, the complex projective plane did not bound a smooth manifold and he realized that it was the reducible connections — those with holonomy $U(1)$ and not the full $SU(2)$ — which gave singular points in the interior of the moduli space. The manifold did not itself bound, but was given an explicit cobordism to a disjoint union of complex projective spaces.

His conclusion was that when the intersection matrix of X is positive definite it must be reduced over the integers to the standard diagonal form. As he admits [Donaldson 1997],

> The point I wish to make is that the chain of reasoning was to a large extent a product of naiveté, the initial impetus being the desire to test the compactification hypothesis for instanton moduli spaces. Moreover, it was not immediately clear to me what use the argument should be put to: I did not even know that there were any non-standard quadratic forms!

At the time, his supervisor Atiyah went to visit the Institut des Hautes Études Scientifiques in Paris, and met Michael Freedman who had been working on the topological theory of four-manifolds. He began to explain Donaldson's work [Atiyah 2000]:

> I asked him "Would this sort of result be of interest? How would it fit into…"
>
> "Oh," he said, "that would be spectacular, totally unbelievable."
>
> And it just so happened that, because Freedman had finished off the problem on the topological side, he expected that with a little bit more work he would finish off the smooth case, and to be told that exactly the opposite was true was really, you know, a bit of a shock to him.

Freedman had shown that any unimodular quadratic form could appear as the intersection matrix of a *topological* four-manifold. He was awarded a Fields Medal at the same time as Donaldson in 1986. By that time, their results had shown that $\mathbb{R}^4$ itself has infinitely many different differentiable structures.

After the remarkable achievement of his thesis, Donaldson returned to the stability conjecture, and soon succeeded in proving it for Kähler surfaces (it was followed up in more generality by S.-T. Yau and K. Uhlenbeck). He needed it for algebraic surfaces, to begin to apply his four-manifold theory further, since there are concrete ways of constructing stable holomorphic bundles on some of these, in particular, elliptic fibrations. Such fibrations were also a source of interesting conjectures — Kodaira had in 1970 produced examples of complex surfaces which were homotopy equivalent to a $K3$ surface but not obviously diffeomorphic to one.

One feature in all this work is the fact, mentioned earlier, that the dimension of the kernel of an elliptic operator can change in a smooth family. In producing a smooth moduli space, Donaldson needed to prove the vanishing of the null space of a particular operator. For the sphere, this came from positivity of curvature, and in the early days of Donaldson's proof it was not clear whether he was proving something about just a manifold or a manifold with a metric satisfying a curvature condition. The same was true of early versions of Taubes' existence theorem. Donaldson got around this for his theorem by deforming the self-duality equations, though subsequently it was shown that one can deform the metric. This jumping in dimension was made to work positively a little later. Fixing a surface in the four-manifold, one may study the Dirac operator on the surface coupled to the self-dual connection. The set of connections where the dimension of the null space jumps defines a cycle in the moduli space and, although it is not compact, intersection numbers can be defined. These gave rise to the Donaldson polynomials in 1988, which gave an algebraic structure to his results and enabled powerful theorems about four-manifolds to be proved.

In 1985, at the age of 28, Donaldson was appointed to the Wallis Chair of Mathematics in Oxford, and he rapidly built up a large number of graduate students, dealing with them ultimately in a manner not unlike that of Penrose.

Geometry and physics

The story of the instanton helped to cement the ties between mathematics in Oxford and the new developments in theoretical physics around the world. The obvious next problem was to study magnetic monopoles in Euclidean three-space — these were static solutions to the Yang–Mills–Higgs equations, and cousins of the instantons. Ward, by then at Trinity College, Dublin, had produced some interesting examples using elliptic functions in 1981, but the writer began to develop the theory using the twistor approach, which led to the study of holomorphic line bundles on an algebraic curve. The physicist W. Nahm had produced an analogue of the ADHM construction in this case, which led to a system of ordinary differential equations which were algebraically integrable in the same sense that the equations of a spinning top are integrable, and led again to the same curve. Nahm's approach also allowed Donaldson to prove that the moduli space of $SU(2)$ monopoles of charge k had a far more concrete description than the instanton case — it was the space of rational functions $f(z) = p(z)/q(z)$ of degree k mapping infinity to zero.

At the same time, the Riemannian version of twistor theory was being extended to higher dimensions and, in particular, to the theory of hyperkähler manifolds — Riemannian structures based on the quaternions. The same hyperkähler geometry was being used by physicists in the supersymmetric sigma-model and, for a while, twistor theory and supersymmetry were running side by side. The mathematical outcome of a collaboration between the writer and the physicists M. Roček, U. Lindström and A. Karlhede was the hyperkähler quotient construction. This fitted in well with Atiyah's moment-map philosophy, and it became clear that the self-duality equations on $\mathbb{R}^4$ and the monopole equations on $\mathbb{R}^3$ could both be expressed as zeros of a hyperkähler moment map. This meant that the moduli spaces (in the instanton case, an $SU(2)$ bundle over the $(8k-3)$-dimensional space) had a hyperkähler structure. Using this fact and the evaluation of the metric in the charge-2 case, Atiyah and the writer investigated the scattering properties of two-monopoles.

These were all interactions between geometry and physics, but was it genuine physics? Witten recalled hearing Atiyah give a lecture during his first visit to Oxford in 1978 [Witten 2003]:

> 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* behaviour of gauge theories.

There was one geometer in Oxford, however, who had thought a lot about quantum theory — Graeme Segal. Segal had been one of Atiyah's first students, working on equivariant K-theory, and then other equivariant generalized cohomology theories. He was a collaborator on the second of the *Annals* papers on the index theorem. Well known as an algebraic topologist, he arrived in Moscow in the early 1970s to give some lectures and met S. Novikov, who told him, "So you are a topologist? Here we think that algebraic topology is dead." Novikov had won a Fields Medal in 1970, largely for his work in topology, but was at the time involved in solitons and integrable systems.

Segal's work after that changed course and, in particular, he wrote with his student A. Pressley a highly influential book on loop groups — the group of maps from a circle to a Lie group — and their representations. With G. Wilson, he used the loop-group properties to give a rigorous version of Miwa and Sato's approach to the KdV equation.

In the mid-1980s, Segal produced a preprint, "The definition of conformal field theory," which was only privately circulated but nevertheless became quite influential. Its motivation was to link up what the physicists were doing with a number of pure-mathematical topics, such as the Griess–Fischer Monster group, representations of the diffeomorphism group of the circle, representations of loop groups, and moduli of Riemann surfaces. He chose to do this by defining a conformal field theory in an axiomatic way, as a functor from a certain category to the category of Hilbert spaces, satisfying a list of properties. The work finally appeared, still in a relatively informal mode, in [Segal 2004].

To many mathematicians, a category was a huge thing: the category whose objects are sets and morphisms maps between them; or the one whose objects are finite-dimensional vector spaces and morphisms, linear maps. Segal had earlier been interested in smaller ones, a group for example (which is a category with one object, the morphisms being elements of the group); he had discussed the homotopy theory of classifying spaces of such small categories.

The objects of Segal's category are oriented disjoint unions of circles, and the morphisms, Riemann surfaces with boundary. So, the Riemann surface has an ingoing boundary component and an outgoing one, and it represents a morphism from one to the other. Morphisms are composed by gluing the Riemann surfaces together, extending the conformal structure. Segal's point of view was that a conformal field theory is a functor from this category to the category of projective infinite-dimensional Hilbert spaces.

Meanwhile, in 1982, Witten had applied quantum-theoretical ideas to a simple geometrical problem, to produce a radical new viewpoint [Witten 1982]. The geometric problem was Morse theory — the study of a compact manifold with a smooth function f on it, with nondegenerate critical points. Witten viewed the even forms on the manifold as a bosonic Hilbert space, and the odd forms as a fermionic one, with the operators $d + d^*$ and $i(d - d^*)$ as supersymmetry operators going from the bosonic space to the fermionic one. Conjugating by e^{tf}, he argued that the eigenfunctions of the Hamiltonian $dd^* + d^*d$ should, for large t, be concentrated around the critical points; but there was "tunneling" from one state to another, given by "instantons." This language reflected the original role of the Yang–Mills instantons in quantum field theory. The instantons in this case are the gradient flow lines of the function f, going from one critical point to another. He saw this as a quantum-mechanical system in one-space/one-time dimensions.

This paper introduced a different category, whose the objects are the critical points of the function f, and the gradient flow lines between them are the morphisms. Here, the objects are zero-dimensional manifolds, and the morphisms one-dimensional ones, but with no additional structure like the conformal structure of the Riemann surface in Segal's case. There was clearly a general concept here, of a *topological* quantum field theory — the objects d-manifolds, the morphisms $(d+1)$-cobordisms, and the theory a functor to finite-dimensional Hilbert spaces.

Atiyah and Segal axiomatically formalized this [Atiyah 2003]:

> Because mathematicians are frightened by the Feynman integral and are unfamiliar with all the jargon of physicists, there seemed to me to be a need to explain to mathematicians what a topological quantum field theory really was, in user-friendly terms. I gave a simple axiomatic treatment (something mathematicians love) and listed the examples that arise from physics. The task of the mathematician is then to construct, by any method possible, a theory that fits the axioms. I like to think of this as analogous to the Eilenberg–Steenrod axioms of cohomology, where one can use simplicial, Čech or de Rham methods to construct the theory.

The axioms require for each oriented d-dimensional manifold Σ a complex vector space $Z(\Sigma)$; for each $(d+1)$-dimensional manifold with boundary Σ, there is a distinguished vector in $Z(\Sigma)$. The nomenclature reflects the partition function of quantum field theory. With the opposite orientation, one gets the dual space; for the empty set, $Z(\varnothing) = \mathbb{C}$. The vector space for the disjoint union of Σ_1 and Σ_2 is the tensor product $Z(\Sigma_1) \otimes Z(\Sigma_2)$. There are two more axioms: one is associativity for composing cobordisms, and the other, a homotopy condition that the morphism defined by $\Sigma \times I$ is the identity.

Thus did Atiyah respond to Witten's challenge to move upwards to the quantum level in doing geometry. He conjectured that Floer's new homology groups should be the Hilbert spaces of a TQFT, that Donaldson's theory should be one where $d = 3$, and that the new knot polynomials of Vaughan Jones should be another for $d = 2$.

In the summer of 1988, the International Congress of Mathematical Physicists was held in Swansea, and at dinner in Annie's restaurant (an event now immortalized by a plaque) Witten talked to Atiyah and Segal about Segal's ideas on conformal field theory and "modular functors." He then realized that "the right theory to get the Jones polynomials was a TQFT whose Hilbert space is the finite-dimensional space of conformal blocks of a two-dimensional WZW theory, and the Lagrangian of this theory was the Chern–Simons functional."

The conformal blocks are essentially the space of sections of a certain holomorphic line bundle on the moduli space of stable bundles over a Riemann surface. Somewhat later, Witten would give also an interpretation of Donaldson theory as a TQFT.

Atiyah's comparison of his TQFT axioms with the axiomatic approach to cohomology suggests that there should be many different ways of establishing this TQFT, but at the time of writing (2007) there is only one — a combinatorial one — which truly works. Other approaches, using more algebraic geometry, are close to working and the writer in 1990 made a contribution to this by defining a projectively flat connection designed to identify the Hilbert space for two different conformal structures.

But other events took place in 1990. Witten won a Fields Medal for his work on the Jones polynomial, and Atiyah and Segal both left Oxford for Cambridge. Atiyah became Master of Trinity College, Director of the newly formed Isaac Newton Institute for Mathematical Sciences, and President of the Royal Society. Segal had been elected to the Lowndean Chair of Astronomy and Geometry after the death of the topologist J. F. Adams. This writer too left in that year, to take a Chair at the University of Warwick. The quantization of geometry was to be pursued elsewhere.

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