MICHAEL ATIYAH
COLLECTED WORKS

VOLUME 5
Gauge Theories

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Foreword to the Chinese Edition

When Michael Atiyah was interviewed by the Mathematical Intelligencer (vol. 6, pp. 9–19, 1984), he was asked about his most admired mathematician. He answered "Well, I think that is rather easy. The person I admire most is Hermann Weyl. He had interests in group theory, representation theory, differential equations, spectral properties of differential equations, differential geometry, theoretical physics; nearly everything I have done is very much in the spirit of the sort of things he worked in. And I entirely agree with his conceptions about mathematics and his view about what are the interesting things in mathematics." We find in these Collected Papers this mathematical philosophy and spirit preserved and continued.

I would like to advise my Chinese colleagues and students to take this as an advanced "textbook". No matter how refined or improved a new account is, the original papers on a subject are usually more direct and to the point. When I was young, I was benefited by the advice to read Henri Poircare, David Hilbert, Felix Klein, Adolf Hurwitz, etc. I did better with Wilhelm Blaschke, Elie Cartan, and Heinz Hopf. This has also been in the Chinese tradition, when we were told to read Confucius, Han Yu in prose, and Tu Fu in poetry. It is my sincere hope that these Collected Papers will not be decorations on book shelves, but worn-out in the hands of young mathematicians.

陳省身

PREFACE

It appears to be increasingly fashionable to publish 'collected works' long before the author's demise. There are several clear advantages to all parties: posterity is saved the trouble of undertaking the collection, while the author can add some personal touches in the way of a commentary. There are also disadvantages: the commentary will be biased, and the author may feel that he is being pensioned off.

The initiative for these particular volumes came in fact from a different direction. A few years ago Professor Chern, who is now in active retirement trying to help China rebuild its mathematics, suggested that collections of mathematical papers made available in China would be most helpful to the younger Chinese mathematicians. Following on from this proposal the Oxford University Press agreed to publish my collected works and to make suitable arrangements to ensure their availability in China through the World Publishing Corporation in Beijing.

Essentially all my mathematical and quasi-mathematical publications are included here. The only exceptions are my textbook (with Ian Macdonald) on *Commutative algebra* and some articles which duplicate, identically or too closely, those published here. On the other hand I have included short articles, announcements of results or conference talks, which are later subsumed in larger papers. It seems to me that these still serve a useful purpose as a brief summary and introduction to the more technical papers.

There is always a problem deciding how to order papers in such a collection. The easiest course is to follow rigidly the date of publication, but this has little to commend it except inertia. The gap between submission and publication varies considerably and can run to two or three years. Also papers which have been published in several parts may not appear consecutively. Finally, any mathematical coherence can be lost in such a presentation with papers on different topics appearing all jumbled together. I have therefore tried to organize the material so that papers on related topics appear together, although the division is sometimes difficult and a bit arbitrary, for example in papers on the K-theory/Index theory boundary. Within each group I have broadly kept to a chronological order.

The commentaries I have provided are meant to fill in the mathematical background by explaining the genesis of ideas and their mutual relation. It is notorious that in mathematics the final published article, in attempting to clarify the logical presentation, usually obscures the origins and motivation. My commentaries are intended to rectify the situation in a small way. I have not hesitated to mention the names of colleagues and collaborators involved in the development of my ideas and, as far as possible, to describe their various contributions. I hope these personal touches will

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enhance the interest of the more formal material. Of course I realize that my memory may be faulty and, even worse, that by some subtle Freudian process I may have distorted the relative importance of what I have learnt from others. I apologize in advance to any who may have been unfairly treated.

I have indeed been fortunate to have had so many excellent mathematicians as my collaborators, and I thank all of them for allowing our joint papers to appear here. Above all I am indebted in many ways to my main collaborators, Raoul Bott, Fritz Hirzebruch, and Iz Singer. It has been a real pleasure to work with them over so many years.

Oxford
December 1986

M.F.A.

CURRICULUM VITAE

Born in London 22 April 1929, oldest son of Edward Atiyah and Jean Atiyah (née Levens).

Married 30 July 1955 to Lily Brown. Three sons, John, David, Robin.

Knight Bachelor 1983.

Education:

(Primary) Diocesan School, Khartoum, Sudan 1934–41.

(Secondary) Victoria College, Cairo & Alexandria, Egypt, 1941–45. Manchester Grammar School, 1945–47.

National Service R.E.M.E. 1947–49.

Trinity College, Cambridge, B.A., 1952, Ph.D. 1955. Research Fellow, 1954–58.

Commonwealth Fund Fellow, The Institute for Advanced Study, Princeton, 1955–56.

Tutorial Fellow, Pembroke College, Cambridge, 1958–61.

Assistant Lecturer, Cambridge University, 1957–58, Lecturer, 1958–61.

Reader, Oxford University and Professorial Fellow of St. Catherine's College, 1961–63.

Savilian Professor of Geometry, Oxford University and Professorial Fellow, New College, 1963–69.

Professor of Mathematics, The Institute for Advanced Study, Princeton, 1969–72.

Royal Society Research Professor, Oxford University and Professorial Fellow of St. Catherine's College, 1973–

Fellow of the Royal Society and Foreign member of: National Academy of Sciences USA, American Academy of Arts and Sciences, Academie des Sciences (France), Akademie Leopoldina, Royal Swedish Academy, Royal Irish Academy, Royal Society of Edinburgh.

Doctor honoris causa of Universities of Bonn, Warwick, Durham, St. Andrews, Dublin, Chicago, Cambridge, Edinburgh, Essex, London, Sussex, Ghent.

President, London Mathematical Society 1975–77, Mathematical Association 1981–82, Vice-President Royal Society 1984–85.

Fields Medal, Moscow, 1966.

PAPERS ON GAUGE THEORIES 94 – 124

From 1977 onwards my interests moved in the direction of gauge theories and the interaction between geometry and physics. I had for many years had a mild interest in theoretical physics, stimulated on many occasions by lengthy discussions with George Mackey. However, the stimulus in 1977 came from two other sources. On the one hand Singer told me about the Yang–Mills equations, which through the influence of Yang were just beginning to percolate into mathematical circles. During his stay in Oxford in early 1977 Singer, Hitchin and I took a serious look at the self-duality equations. We found that a simple application of the index theorem gave the formula for the number of instanton parameters, and our result appeared in the brief note [94]. At about the same time A. S. Schwarz in the Soviet Union had independently made the same discovery. From this period the index theorem began to become increasingly familiar to theoretical physicists, with far-reaching consequences.

The second stimulus from theoretical physics came from the presence in Oxford of Roger Penrose and his group. Roger and I had been research students together in Cambridge, at the time when he was an algebraic geometer. We lost touch after that but renewed contact on his arrival in Oxford as Coulson's successor. In fact, I encouraged him to come to Oxford, at a time when there were other possibilities. I also remember discussing Penrose's work with Freeman Dyson in Princeton. After describing the work on black holes which he could understand, Dyson went on to talk about twistor theory which he found mysterious. He ended by saying to me 'perhaps you will understand it'. At that time I knew nothing about twistors, but on Penrose's arrival in Oxford we had lengthy discussions which were mutually educational. The geometry of twistors was of course easy for me to understand, since it was the old Klein correspondence for lines in P_3 on which I had been brought up. The physical motivation and interpretation I had to learn.

In those days Penrose made great use of complex multiple integrals and residues, but he was searching for something more natural and I realized that sheaf cohomology groups provided the answer. He was quickly converted and this made subsequent dialogue that much easier, since we now had a common framework.

Richard Ward was at this time a student of Penrose and he gave a seminar explaining how twistors could be used to reinterpret the self-dual Yang–Mills equations. This seminar made a big impact on me. I realized that this was something significant, and spent several days thinking hard about it. Penrose and Ward were working of course with Minkowski space (or its complexification), but I was already interested (through my contacts with Singer) in the Euclidean case. Fortunately some time earlier Penrose had discussed the Euclidean version of twistor space with me, and I had discovered then its quaternionic interpretation. With this understood it was not long before I saw how to put Ward's ideas to global use for the problem of instantons. At that stage only the original 1-instanton had been explicitly constructed, and I saw how to generalize this to give k-instantons by using disjoint lines in twistor space. Almost immediately I was disappointed to receive preprints from physicists who had (by different methods) discovered the same solutions! This was to be my first experience of the different tempo of the world of theoretical physics, in which new ideas spread like wild-fire and stimulate the production of preprints on a lavish scale.

Stimulated by this competitive atmosphere, Ward and I pushed on showing how algebraic geometry gave in principle methods for the construction of all multi-instantons. Our paper [95], brought this whole subject to the attention of a wider mathematical audience, and thereby helped to increase the general interaction between physicists and geometers.

Although [95] reduced the instanton problem to one in algebraic geometry, namely the construction of suitable vector bundles on P_3, it did not really yield an explicit solution. However, vector bundles had been much studied by algebraic geometers, and various methods of construction had been developed. In particular Geoffrey Horrocks, another of my contemporaries at Cambridge, had given an interesting construction which I was trying to understand. With the help of Nigel Hitchin I finally saw how Horrocks' method gave a very satisfactory and explicit solution to the general instruction problem. I remember our final discusion one morning, when we had just seen how to fit together the last pieces of the puzzle. We broke off for lunch feeling very pleased with ourselves. On our return we found a letter from Manin (whom I had earlier corresponded with on this subject) outlining essentially the same solution to the problem and saying 'no doubt you have already realized this'! We replied at once and proposed that we should submit a joint note [96] from the four of us (Drinfeld was collaborating with Manin).

These brief publications were in due course expounded into more substantial form. In [97] Hitchin, Singer, and I gave a full account of the Penrose twistor theory in the context of Riemannian geometry and gave detailed proofs of the results announced in [94]. Our aim was in part to bring Penrose's work to the attention of differential geometers, presenting it in a purely mathematical form without reference to the physical background. In my Fermi Lectures [99] given in Pisa in 1978, I attempted to bridge the gap between algebraic geometry and the physicists view of gauge theory. As well as elaborating on my paper with Ward I also gave a fuller account of the construction of instantons in the four-author note [96]. 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 [95] and [96] in particular had caused quite a flurry of interest in the mathematics/physics interface. It is reported that Polyakov had described [96] as the first time abstract modern mathematics had been of any use! The two papers [98], [100] are examples of expository lectures given during this period (1978–80).

Besides having applications to Yang–Mills instantons the Penrose twistor theory also applied to their gravitational counterparts, self-dual Einstein manifolds. This was discovered by Penrose and represents perhaps the deepest illustration of his ideas. After Hawking and Gibbons had constructed their gravitational instantons, Hitchin showed how to rederive their results using twistor methods. This was a very elegant approach and surprisingly was closely related to Brieskorn's work on rational double points. In [101] I showed how twistor methods could be pushed one stage further to derive the Green's function for such manifolds. I found the translation of delta functions into homological algebra particularly appealing and similar ideas have come to the fore more recently in the fundamental work of Donaldson.

My paper [102] with John Jones had an unusual origin. Although we had by this time complete information on instantons, i.e. solutions of the self-dual Yang–Mills equations, it was unknown (and still is in 1986) whether (for $SU(2)$), there were any solutions of the second-order Yang–Mills equations which were not self-dual (or anti-self-dual). I then heard that this question had been settled by a physicist, but the argument depended on an assertion which I did not believe. Paper [102] developed out of my attempt to clarify this question. It was also related to the famous Gribov ambiguity which Singer and others had analysed topologically. The main result in [102] was a proof that the homology of the moduli spaces of k-instantons, as $k \to \infty$, 'contained' all the homology of the relevant function space.

I was at this time very intrigued by variational problems where the Morse theory failed, but where nevertheless the minima of the functional carried much of the homology. Graeme Segal had, in answer to a question of mine, established a remarkable theorem of this type for rational functions, as minima of the energy functional for maps $S^2 \to S^2$, and I was able to use this in my paper with Jones. I had many discussions with Segal about the role of 'particles' in topology and physics and it was interesting to see the way instantons (or pseudo-particles as they were once called) entered into the topological picture. My general ideas, and speculations, on these Morse theory questions were described in a conference report [103]. In subsequent years I also explained my ideas to Cliff Taubes who eventually produced the appropriate analytical refinement of Morse theory to explain the phenomena which had puzzled me. This refined Morse theory applies in some 'limiting exponent' cases and is very subtle. It is (in 1986) just beginning to be understood and developed.

Another example of Morse theory, but this time of a more conventional kind, was the subject of my long paper [105] with Bott, summarized in [104]. This arose in the following way. Bott was visiting Oxford for a while, having just returned from the Tata Institute in Bombay where he had been studying moduli spaces of vector bundles over Riemann surfaces with Ramanan. Meanwhile I had been engrossed with the Yang–Mills equations in dimension 4. I realized that these questoins were essentially trivial in dimension 2, but one day, walking across the University Parks with Bott it occurred to me that one might nevertheless be able to use the Yang–Mills equations to study the moduli spaces. The essential point was the theorem of Narasimhan and

Seshadri stating that stable bundles arose from unitary representations of the fundamental group. Bott and I soon became convinced that this method would work but there were many technical problems. A key idea, due to Bott, was that we should use equivariant cohomology in the Morse theory. This turned out to be very productive and later, in the hands of my student Frances Kirwan, it was put to extensive use.

A formal point which Bott and I noticed in our work was that the curvature could be viewed as a moment map for a symplectic group action (all in infinite dimensions). This was very significant in view of the role of moment maps in Mumford's geometric invariant theory (this role had just been observed by Mumford and Sternberg). After our successful treatment of the moduli space problem by these methods, Bott and I wondered whether they might also apply in some finite-dimensional situations. This was the problem which Frances Kirwan disposed of with such finality in her thesis.

In the course of writing [105] I had encountered some convexity questions, and in attempting to understand their significance I was led to the formulation described in [106]. Here I put some old results of Schur, Horn, Kostant, and others into a more general symplectic geometry context. When I visited Harvard I lectured on this material in Bott's seminar and was mystified by the looks of amusement on several faces in the audience. It transpired that Guillemen and Sternberg had almost simultaneously found the same result!

After [106] appeared I received a letter from Arnold explaining how my result could be applied to simplify a result of Koushnirenko, expressing the number of zeros of a set of polynomials in terms of the volume of the associated 'Newton polyhedron'. He raised the question of deriving Bernstein's generalization, a 'polarized form' of this result involving the Minkowski mixed volumes. I found these results quite beautiful and fascinating. They were also linked to Teissier's proof of the Alexandroff-Fenchel inequalities based on the Hodge signature thoerem. After I had understood how to prove Bernstein's result it seemed to me that this material deserved some publicity, and that it was suitable for the lecture I was asked to give at the centenary meeting of the Edinburgh Mathematical Society [107].

In my paper with Bott [105] we had already noted the appearance of the moment map in an infinite-dimensional setting. In my joint paper with Pressley [108] we extended the convexity results of [106] to the loop space of a compact group. By this time these loop groups (and their associated Lie algebras) had become intensively studied by mathematicians and physicists. I had been familiar with them for some time in view of their apperance in Bott's proof of the periodicity theorem, but my more detailed understanding of their geometry was the result of extensive discussions with Graeme Segal. He and his former student Pressley were just in the process of writing up their book on the subject (Clarendon Press, 1987).

During an *Arbeitstagung* in the early eighties I had discussed with Hans Duistermaat the problem of explaining why stationary-phase approximation sometimes gave exact results. Shortly afterwards Duistermaat and Heckman found an elegant result on these lines for Hamiltonians arising from circle actions on symplectic manifolds. When Bott

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was next in Oxford we tried together to understand one of Witten's paper where he had introduced the operator $d + i_X$ for a Killing field X. Putting these two together we saw that new insight could be obtained if one worked with a de Rham version of equivariant cohomology. In fact this involved ideas with which we had both been familiar, and which had been widely used. The new developments however sharpened our understanding and so we wrote [109], essentially as an expository paper tieing together various points of view..

A very quick survey of the role of the moment map in various contexts is given in [110].

Papers [111], and [112] are both conference lectures on gauge theories. Paper [111] is a shortened version of my Fermi lectures [99], while [112] describes Donaldson's now famous application of the Yang–Mills equations to the geometry of 4-manifolds.

In my talk at the Helsinki Congress [98] I had drawn attention to the problem of monopoles, i.e. solutions of the Bogomolny equations. While twistor methods and algebraic geometry had been very successful with instantons, the related problem of monopoles appeared more difficult. In the subsequent years this problem was attacked by many people and, due to the work notably of Ward, Hitchin, and Nahm, the general nature of the solutions was well understood. In [113] and [114] I reported on this progress, mainly from the point of view of Hitchin. During the Trieste conference, where [113] was presented, I had several discussions with Nick Manton during which he explained to me his ideas on monopole dynamics. He showed me how the dynamics of slowly moving monopoles would be described by geodesics on the parameter space of static solutions. This struck me as a very attractive idea and I tried with some of my students to find or guess the metric on the 2-monopole space, but without success. Then, a couple of years later, I learnt from Hitchin of the beautiful construction of hyper-kähler quotients and the likelihood that this would apply to the monopole spaces. The time seemed ripe therefore for another attack on the problem, and so Hitchin and I began our lengthy investigation into the geometry and dynamics of monopoles which is summarized in [115] and [116]. This has since been taken further by Gibbons and Manton who have analysed the quantization.

Around this time my former student Donaldson was taking the lead in the study of instantons and monopoles. He made a beautifully simple observation concerning the instanton construction on R^4 and he then went on to deal with the case of monopoles. The two papers [117] and [118] essentially arose out of discussions with him, although a casual breakfast conversation with Howard Garland in Berkeley had started the ball rolling. The main result of [117] relating instantons on R^4 to rational curves on the loop space interested me because it opened up a possible door to establishing the conjectures I had made with Jones on the topology of instanton moduli spaces. The idea was that the method Segal had used to study the topology of rational maps might be extended to the case of ΩG. It now looks as though this programme can in fact be carried out, while Taubes has developed his refined Morse theory which makes a direct attack also possible. All in all the Morse theory questions raised in [103] have proved very fruitful.

In 1982 I was flattered to be invited to the Solvay conference in Austin, Texas. At that meeting I heard Witten explain his mod 2 anomaly. Discussions with him, and earlier discussions in Oxford with Quillen, opened my eyes to the meaning of anomalies and their relation to the index theorem for families. In the next few years this became a very hot topic amongst physicists, leading to a typically large number of papers. Papers [119] and [121] are conference lectures where I was addressing physicists and attempting to explain the relevant mathematics, while [120] is a short note with Singer summarizing our point of view. This was intended to be written up at greater leisure, but physics moves at a different pace from mathematics. The leisurely account has now been overtaken by events and is unlikely to see the light of day.

The 1984 *Arbeitstagung* in Bonn was a special 25th anniversary occasion, so that for the first time the proceedings were published. My talk [123] was a presentation of beautiful results of Witten and Vafa. These results had impressed me because they involved topological methods to prove *inequalities* for eigenvalues. Although Witten was now very well known to mathematicians, his influential papers had been those published in journals of mathematics or mathematical physics. Papers published in regular physics journals were unlikely to be read by the mathematical community and so I felt some publicity for his ideas would be a public service. My commentary [122] on Manin's manuscript is a rare case where I put down on paper the kind of wild speculation which I usually only indulge in verbally. This is a hostage to fortune, but it may perhaps serve a useful purpose by showing that we mathematicians are not the rigorous formalists our published papers might suggest, and that we do allow our imagination a free rein.

At the Solvay Conference in Austin, on the boat trip, Witten had explained to Singer and me his beautiful ideas on the Duistermaat–Heckman formula applied to the loop space. He showed us how this led heuristically to the index theorem for the Dirac operator! Although this was not rigorous mathematics I felt it was suitable for a lecture at the Schwartz Colloquium in Paris [124]. As it happened my words fell on fertile ground, because Bismut was in the audience and he immediately turned his attention to providing rigorous proofs of Witten's ideas. Several other versions of Witten's ideas have been developed and this whole area is still in a state of great activity. The interaction between physics and mathematics in this field is quite remarkable, and I am really struck by the way most of the work which Singer and I did in the 60s and 70s has become relevant to physics.

DEFORMATIONS OF INSTANTONS

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Deformations of instantons

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Contributed by I. M. Singer, April 20, 1977

ABSTRACT **A study is made of the self-dual Yang-Mills fields in Euclidean 4-space. For SU(2) gauge theory it is rigorously shown that the solutions depend on $8k - 3$ parameters, where k is the Pontrjagin index.**

There has been considerable interest recently in the instanton or pseudo-particle solutions of the classical Yang-Mills equations in Euclidean 4-space (refs. 1, 2, and 3). In geometrical terms, these equations are the variational equations for the norm-square $\|F\|^2$ of the curvature F of a fiber-bundle with group G and connection A over R^4. In physics terminology, $\|F\|^2$ is the action, F the gauge field, A the gauge potential, and G the gauge group. The cases studied in most detail are for $G = SU(n)$ and, particularly, $G = SU(2)$.

The connection A is assumed to be asymptotically flat in an appropriate sense so that $F \to 0$ at ∞ and $\|F\|^2 < \infty$. Since the variational equations are conformally invariant with respect to change in the metric on R^4, the most natural geometrical restriction to impose on A at ∞ is that it extends to a connection for a bundle over the 4-sphere S^4. The topological type of such a bundle is then determined by a homotopy class of maps $S^3 \to G$, which is given by an integer k when $G = SU(n)$ (and more generally for any simple compact nonabelian Lie group): this is referred to by physicists as the Pontrjagin index (differing from the topologist's terminology by a factor of 2).

Using the duality $*$-operator on R^4 or S^4, we can decompose F into $F^+ \oplus F^-$, where $*F^+ = F^+$ and $*F^- = -F^-$. Clearly, $\|F\|^2 = \|F^+\|^2 + \|F^-\|^2$, while the Pontrjagin index k is given by[§]

$$k = \frac{1}{8\pi^2}\{\|F^+\|^2 - \|F^-\|^2\}.$$

Hence, $\|F^+\|^2 \geq 8\pi^2 k$, and the minimum is attained only if $F^- = 0$ or $F^+ = 0$. Solutions with $F^- = 0$ are called self-dual solutions and have been constructed for all $k \geq 0$. For $k = 0$ we have the trivial solution $F = 0$, for $k = 1$ we have the "instanton," and for $k > 1$ we have "multi-instantons." The most general explicit solutions constructed so far are those of Jackiw *et al.* (ref. 3), which depend on $5k + 4$ parameters.[¶] Our main result is that the complete set of solutions depends on $8k - 3$ parameters. This confirms some preliminary results of Jackiw and Rebbi (ref. 4) and Schwartz (ref. 5).

RESULTS

If a connection A yields a self-dual Yang-Mills field F, then so does any connection $g(A)$ where g is a bundle automorphism (or gauge transformation). The space of all solutions A modulo the action of this gauge group will, as usual in such geometric problems, be called the space of moduli. Our main result can now be formulated as a precise theorem:

THEOREM. *The space of moduli of self-dual SU(2)-Yang-Mills fields over S^4, with Pontrjagin index $k \geq 1$, is a manifold of dimension $8k - 3$.*

The standard deformation theory approach to such problems is to consider the linearized equations modulo the infinitesimal gauge transformations. Here this leads to a three-step elliptic complex

$$0 \to \mathcal{G} \xrightarrow{D_0} \mathcal{G}\otimes\Omega^1 \xrightarrow{D_1} \mathcal{G}\otimes\Omega^2_- \to 0$$

where Ω^1 denotes 1-forms, Ω^2_- denotes anti-self-dual 2-forms on S^4, and $\mathcal{G}$ is the Lie algebra of G. The operator D_0 is the covariant derivative and D_1 is the anti-self-dual part of the covariant derivative ($D_1D_0 = 0$ because we are using a self-dual connection). The index theorem of Atiyah-Singer (ref. 6) yields the alternating sum formula $h^0 - h^1 + h^2 = 3 - 8k$. Here h^0 is just the dimension of the null space of D_0, and this is zero unless the SU(2)-bundle is trivial (which is excluded for $k \geq 1$). h^1 is the potential number of moduli and h^2 is the dimension of the null space of D_1^*. Fortunately, in our case, a Bochner type vanishing theorem works very well and we find $h^2 = 0$. This gives $h^1 = 8k - 3$, showing that this is the dimension of the solutions of the linearized problem.

Now we appeal to the general theorem of Kuranishi (ref. 7), which, when $h^2 = 0$, guarantees that the infinitesimal variations really integrate to give genuine local variations. Moreover, the Kuranishi theorem asserts that the family of solutions thus obtained is (locally) complete and effective (non-redundant). This leads to the theorem as stated above.

Note. The theorem does not assert that, for each k, the space of moduli is connected: In principle it may have several components. For $k = 1$ it is in fact connected and is the hyperbolic 5-space. See Yang (ref. 8) for a different discussion of the case $k = 1$.

FURTHER REMARKS

The above arguments apply equally to SU(n), provided we have an irreducible connection (so that $h^0 = 0$), i.e., one that does not come trivially from SU($n - 1$). We then find $h^1 = 4nk - n^2 + 1$. Moreover, the existence of irreducible SU(n) solutions can be deduced from this formula provided $k \geq (n - 1)/2$. In the opposite direction one can deduce nonexistence for $k < n/4$.

Similar methods, i.e., index theorem plus vanishing theorem, yield a formula for the dimension d of the space of zero-eigenvalue fermions (harmonic spinors): one finds $d = k$.

The problem of explicitly constructing the $(8k - 3)$-parameter families of solutions, whose existence is asserted by our theorem, can be treated by converting it into a problem in algebraic geometry (M. F. Atiyah and R. Ward, unpublished).

The 4-sphere can be replaced by other 4-manifolds M (compact, oriented, Riemannian). If M is also a spin-manifold, then we have the two spin SU(2)-bundles, P^+, P^-. One can show that P^+ (with the Riemannian connection) is self-dual if

[§] Physicists use a different norm and get a factor $1/(16\pi^2)$.
[¶] The formula is a little different for $k = 1,2$.

and only if the metric on M is an Einstein metric ($R_{ij} = \lambda g_{ij}$) and that P^- is then anti-self-dual. The deformation theory above can then be applied to P^+ to obtain the number of moduli. The vanishing theorem for a self-dual field ($h^2 = 0$) still applies provided that the conformal Weyl tensor of M is self-dual and that the scalar curvature is positive.

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