MICHAEL ATIYAH
COLLECTED WORKS

VOLUME 4
Index Theory: 2

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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 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 INDEX THEORY
56 – 93a† (1963 – 84)

In the Spring of 1962, my first year at Oxford, Singer decided to spend part of his sabbatical there. This turned out to be particularly fortunate for both of us and led to our long collaboration on the index theory of elliptic operators. This had its origins in my work on K-theory with Hirzebruch and the attempt to extend the Hirzebruch–Riemann–Roch theorem into differential geometry. We had already shown that the integrality of the Todd genus of an almost complex manifold and the $\hat{A}$-genus of a spin-manifold could be elegantly explained in terms of K-theory. For an algebraic variety the Hirzebruch–Riemann–Roch theorem went one step further and identified the Todd genus with the arithmetic genus or Euler characteristic of the sheaf cohomology. Also the L-genus of a differential manifold, as proved by Hirzebruch, gave the signature of the quadratic form of middle dimensional cohomology and, by Hodge theory, this was the difference between the dimensions of the relevant spaces of harmonic forms. As Hirzebruch himself had realized it was natural therefore to look for a similar analytical interpretation of the $\hat{A}$-genus. The cohomological formula and the associated character formula clearly indicated that one should use the spin representations. I was struggling with this problem when Singer arrived. Fortunately Singer's strengths were precisely in differential geometry and analysis, the areas where I was weakest. With his help we soon rediscovered the Dirac operator! My knowledge of physics was very slim, despite having attended a course on Quantum Mechanics by Dirac himself, Singer had a better background in the area but in any case we were dealing with Riemannian manifolds and not Minkowski space, so that physics seemed far away. In a sense history was repeating itself because Hodge, in developing his theory of harmonic forms, had been strongly motivated by Maxwell's equations. Singer and I were just going one step further in pursuing the Riemannian version of the Dirac equation. Also, as with Hodge, our starting point was really algebraic geometry. However, spinors were (and remain) more mysterious than differential forms, and they had never before been used in differential geometry.

Once we had grasped the significance of spinors and the Dirac equation it became evident that the $\hat{A}$-genus had to be the difference of the dimensions of positive and negative harmonic spinors. Proving this then became our main objective. By good fortune Smale passed through Oxford at this time and, when we explained our ideas to him, he drew our attention to a paper of Gelfand on the general problem of computing the index of elliptic operators. Following up this paper we discovered a

† Papers 56–78 form Volume 3; papers 79–93a form Volume 4

number of papers by analysts (Agranovic, Dynin, Seeley) devoted to the index problem. In particular we learnt the relevance of pseudo-differential operators to the problem. In all this Singer was a great help. My analytical background was very weak and I remember having to be instructed on the significance of Fourier transforms.

Singer and I had some great advantage over the analysts investigating the problem. We were investigating a particular case, the Dirac operator, and we already 'knew' the answer. Also, this case, existing in all (even) dimensions, encompassed all the global topological complications. Moreover, we had arrived at the problem starting from K-theory and this turned out to be just the right tool to study the index problem. Finally the Dirac operator was in a sense the most general case, all others being essentially deformable to it.

With considerable help from our analytical friends, such as Louis Nirenberg, Singer and I eventually produced a proof of the general index theorem during my stay at Harvard in the Fall in 1962. This was based on the use of boundary value problems and follows the cobordism approach of Hirzebruch's proof of the signature theorem. We announced our results in the *Bulletin* note [56]. I realized at the time the significance of the index theorem and that it represented the high-point of my work, but it would have been hard to predict that the subject would continue to occupy me in various forms for the next twenty years. I would also have been extremely surprised if I had been told that this work would in due course become important in theoretical physics.

The proof of the index theorem was only briefly sketched in [56]. The full details were presented in the seminar which I ran with Bott and Singer in the Fall of 1962. In due course a more comprehensive version of this appeared as the *Annals of Mathematics Study*, based on the Seminar at the Institute for Advanced Study run by Borel and Palais. I wrote an Appendix [58] for this describing the extension of the index problem for manifolds with boundary. This was joint work with Bott and is also described in [57]. In fact understanding the role of boundary conditions for elliptic operators was by no means routine. Bott and I struggled with the problem for some time, before we saw the light. It was clear, for topological reasons, that a good boundary condition should somehow 'trivialize' the symbol of the operator at the boundary, so as to define a relative K-theory class. Deriving this trivialization from the boundary conditions turned out to involve a very thorough understanding of the Bott periodicity theorem, and this led as a by-product to our elementary proof [40].

The opening of the new Courant Institute building and the International Congress at Moscow in 1966 provided opportunities for expository survey talks [59], [60] on the index theorem and its relation to the topology of the linear groups (i.e. to K-theory).

At the Woods Hole conference in the summer of 1964 Bott and I learnt of a conjecture of Shimura concerning a generalization of the Lefschetz fixed-point formula for holomorphic maps. After much effort we convinced ourselves that there should be a general formula of this type for maps preserving any elliptic operator (or more generally any elliptic complex). As a first test we applied it to elliptic curves with complex multiplication and, as we had the leading experts with us at Woods Hole (Cassells, Tate, etc.), we asked them to verify it for us. Unfortunately our formula failed the test. Fortunately we did not believe the experts: the formula seemed too beautiful to be wrong, and so it proved. We were especially convinced when one day we suddenly realized that the famous Hermann Weyl character formula was a particular case of our general formula. In due course we found the necessary proof and the result was briefly presented in [61] with full details appearing later in [62], [63].

Perhaps the most interesting application of the general Lefschetz formula concerned the action of a finite group on the middle-dimensional cohomology of a manifold. This yielded surprisingly strong theorems and with Milnor's help we were able in [63] to prove that h-cobordant lens spaces are isometric. The formulae in this case were very reminiscent of those for Reidemeister torsion of lens spaces, so Bott and I spent some time trying to relate Reidemeister torsion to elliptic operator theory. Moreover our first proof in [61] of the Lefschetz formula, relied on the ζ-functions originally introduced by Minakshisundaram and Pleijel. Although we eventually found a simpler proof, we found the ζ-functions very appealing and tried to exploit them further. Although we were unsuccessful at the time ζ-functions came into their own later on and, in particular, Ray and Singer found how to use them to attack Reidemeister torsion.

The first proof of the index theorem for elliptic operators, as presented in [56], was based on cobordism and it suffered from certain limitations. For some years Singer and I searched for a better proof modelled more on Grothendieck's proof of the generalized Hirzebruch–Riemann–Roch theorem. Eventually we found such a proof, based on embedding a manifold in Euclidean space and then transferring the problem to one on the Euclidean space by a suitable 'direct image' construction. This proof, given in [64], worked purely in a K-theory context and avoided rational cohomology. It therefore lent itself to various significant generalizations developed in the remaining papers [65–68] of the series. One generalization, the 'equivariant' index theorem, dealt with compact group actions preserving an elliptic operator. This extended the Lefschetz formula of my papers with Bott to the case of non-isolated fixed points (of isometries), and rested on the prior development of equivariant theory. This had been carried out by Graeme Segal in his thesis and the relevant applications to index theory were developed in [65]. In particular the 'Localization theorem' relating geometric localization (fixed points) to algebraic localization (via ideal theory of the character ring $R(G)$) was a sophisticated version of the Lefschetz fixed-point principle. In various forms this was to reappear in subsequent work, notably that of Quillen on the cohomology of finite groups.

The index theorem for families of elliptic operators [67] was an easy consequence of the new proof and could be seen as a first attempt to generalize the Grothendieck Riemann–Roch theorem. In its real form [68] it included various mod 2 index theorems, such as the computation of the dimension (mod 2) of the kernel of a real skew-adjoint elliptic operator. Despite the many interesting new proofs of the index theorem that have been produced in recent years (heat equation, supersymmetry, etc.), no other proof encompasses these refined mod 2 index theorems, and the proof in [68] remains the only one available.

Although [67] was analogous to Grothendieck–Riemann–Roch for a fibre map we were still a long way from a fully fledged analogue dealing with arbitrary smooth maps. A basic reason for this was that in algebraic geometry Grothendieck had the advantage of two K-groups, one K^0 based on vector bundles and the other K_0 based on coherent sheaves. Formally these behaved like cohomology and homology respectively and in particular K_0 was naturally covariant. I felt that, in the differentiable context, we were missing the analogue of K_0. In [69], posing as a functional analyst, I put forward some preliminary ideas on how to define K_0 by using an abstract notion of elliptic operator. I was very pleased when years later these ideas were taken up and developed into very satisfactory theories by Brown–Douglas–Fillmore and Kasparov. Moreover these theories are rather natural in the general context of C^*-algebras (not necessarily commutative) and have led to considerable activity in this field.

In [70] Singer and I carried out a systematic investigation of mod 2 indices in the framework of Clifford algebras. Very much to our surprise we found an entirely new proof of the (real) periodicity theorem emerge, based on Kuiper's proof of the contractibility of the unitary group of Hilbert space. The connection of this with other proofs remains to this day somewhat mysterious and so far it does not appear to have been exploited, but I understand from Quillen that it may find a natural niche in relation to Connes' cyclic homology theory.

One of the interesting applications of the index theorem for families of elliptic operators concerns the signature of fibrations, and this is the topic of [71]. Chern, Hirzebruch, and Serre had shown that the signature of a fibration is multiplicative (i.e. the product of signatures of base and fibre) provided the fundamental group of the base acts trivially on the cohomology of the fibre. The index theorem for families clarified the role of the fundamental group and non-multiplicative examples occur naturally in algebraic geometry. As I was to realize much later from Witten these questions are related to gravitational anomalies and [71] provides the simplest examples of such anomalies.

My general interest in differential equations (both elliptic and hyperbolic) together with my background in algebraic geometry led naturally to the little note [73], where I explained how Hironaka's theorem on the resolution of singularities gave an easy proof of the Hörmander–Lojasiewicz theorem on the division of distributions.

One of the most surprising applications of the equivariant index theorem was the result proved in my joint paper [74] with Hirzebruch showing that spin manifolds with non-zero $\hat{A}$-genus cannot have effective circle actions. This arose out of related results proved by my student C. Kosniowski for the holomorphic case. It is also reminiscent of the Lichnerowicz theorem that $\hat{A} \neq 0$ implies that there is no metric of positive scalar curvature. These questions have since been developed much further by Gromov and Lawson, and very recently there have been new related results by Landweber and Stong which have stimulated Witten to develop a remarkable link between quantum field theory, manifolds, and modular forms.

During a visit to Harvard in 1970 Mumford asked me whether some classical results on Riemann surfaces, concerning square roots L of the canonical line-bundle K, could be fitted into the index theory framework. In fact, they fitted extremely well into the mod 2 index formulae for spin manifolds that Singer and I had explored. Normally our sights were on higher-dimensions (> 4) and I found it surprising that non-trivial phenomena of this type already occurred in dimension 2. My account of this topic appeared in [75] and this in turn led Mumford to produce a parallel algebraic proof. More recently such 2-dimensional questions figure prominently in super-string theory.

In [75] the dimension mod 2 of the space of holomorphic sections of L is a deformation invariant (as we deform the Riemann surface) and is the mod 2 index of the elliptic theory. An analogous mod 2 index is the essential ingredient in my joint paper [76] with Elmer Rees. This appears as a mod 2 'semi-characteristic' for the sheaf cohomology of a rank 2 vector bundle (with $c_1 = 0$) on complex projective 3-space. With its interplay of topology and algebraic geometry via sheaf cohomology it seemed a particularly appropriate paper to dedicate to Serre (on his 50th birthday), as an indication of how much I learnt from him in earlier years, in terms of both content and style.

My colloquium lectures [77] to the AMS in 1973 (just after my permanent return to Oxford!) were never published, but they provide perhaps a useful historical survey of the Riemann–Roch story and its evolution into the index theorem. Of course, this should now be up-dated, since much has happened in the intervening decade, particularly in relation to theoretical physics, but that will have to await another day and possibly another pen.

The Lefschetz formula of my papers with Bott gave the answer as a sum of local contributions from the fixed points. For a circle action the Lefschetz number is a character of the circle, i.e. a finite Fourier series, but the local contributions have poles. In particular the Hermann Weyl character formula is of this type, and we know in that case that the local contributions can be interpreted as *distributional* characters of infinite-dimensional induced representations. These facts led Singer and me to look for a general context in which such local contributions could be interpreted in an appropriate sense as local 'distributional indices'. We had in mind possible applications to the Harish–Chandra theory of representations of non-compact semi-simple groups and the Blattner conjecture (giving the restriction of the character to a maximal compact subgroup). The ideas led us to introduce the notion of an operator which was elliptic transversally to the orbits of a G-action. Such an operator then had an index which was a distribution on G, and the problem as usual was to give a topological construction for this index. This is the problem studied and partially solved in [78], the lecture notes of a seminar I gave in 1971 at the Institute for

Advanced Study. General methods reduce the problem down to the case of a torus acting linearly on a vector space. Dealing with this case turns out to be unexpectedly intricate and heavy use has to be made of commutative algebra including the theory of Cohen–Macaulay modules. There are interesting applications to situations where there are only finite isotropy groups, but our hope of attacking the Harish–Chandra theory this way never materialized. However, subsequent work by Schmid and others has had some success with Lefschetz formula methods. Moreover, I returned later (with Schmid) with a quite different approach to the Harish–Chandra theory (see below). On the whole therefore [78] has not made much of an impact, probably because there are really no natural examples of tranversally elliptic operators (other than ordinary elliptic ones). Moreover the commutative algebra in [78] was probably unfamiliar and unpalatable to potential users (e.g. topologists or analysts).

The joint paper [79] arose out of the attempts by Bott and myself to understand the remarkable results of the young Indian mathematician V. K. Patodi on the heat equation approach to the index theorem, a topic which was to blossom later in the contact with theoretical physics. In our work on the Lefschetz formula for elliptic complexes, Bott and I had described the Zeta-function approach to the index theorem but had commented on its computational difficulty. Singer and McKean took the process a step further, in the heat equation version, by concentrating on the Riemannian geometry. They speculated on the possibility of remarkable cancellations leading directly to the Gauss–Bonnet form for the Euler characteristic. This was proved some years later by Patodi. Even more significant was Patodi's extension of the result to deal with the Riemann–Roch theorem on Kähler manifolds. Patodi's approach was rather direct and a considerable *tour de force*. But the algebraic manipulations were difficult to understand and it was therefore very interesting when Gilkey produced an alternative indirect approach depending on a simple characterization of the Pontrjagin forms of a Riemannian manifold. On the other hand, while Gilkey's result was beautifully simple, and easily led to the index theorem, it appeared to be enormously complicated to prove. In fact Gilkey had discovered it while performing algebraic computations on the computer.

This was the situation when Bott and Patodi joined me at the Institute for Advanced Study in 1971–72. After considerable effort we eventually realized that Gilkey's results was a very easy consequence of the Bianchi identities, together with the main theorem on tensorial invariants of the orthogonal group. Gilkey's proof had appeared complicated only because he had not taken this route.

Besides proving Gilkey's result, and giving its application to the index theorem, [79] also included a leisurely account of the heat equation asymptotics based on Seeley's approach. Moreover we gave, in the Appendix, a new proof of the theorem on orthogonal invariants. We were impelled to do this by our difficulty in understanding the notorious 'Capelli identity'.

My second collaboration with Patodi, this time with Singer as the third partner, concerned the signature theorem for manifolds with boundary and led to the series of papers [80–83]. The problem of generalizing the Hirzebruch signature theorem to manifolds with boundary had long been an intriguing question. There had been many clues, notably the work of Hirzebruch on signature defects of cusps of Hilbert modular surfaces. While Patodi was with me in Princeton I had suggested this problem to him, hoping that he could apply his virtuosity with the heat equation to the problem. In fact he succeeded on these lines, but his method was again highly computational and tied to the extensive use of differential forms. As such it did not apply to the Dirac operator and its generalizations which I felt should be included. Singer and I therefore tried to analyse the problem in its more general form. Eventually we saw that the natural formulation was that of an index problem with a 'global' boundary condition. This was conceptually a major breakthrough but there were several crucial obstacles still to be overcome when I left the Institute and returned permanently to Oxford at the end of 1972. Shortly after my return I solved the outstanding technical problems. One of these involved using standard formulae from a classical textbook on heat conduction, which I found rather amusing. More important was the psychological effect of feeling that my return to Oxford had started off well, and that the difficult decision to leave the Institute for Advanced Study would not turn out to have been disastrous.

In many ways the papers on spectral asymmetry were perhaps the most satisfying ones I was involved with. The way they stretched over differential geometry, topology, and analysis with a nod in the direction of number theory appealed greatly to me. At the time these papers had only a modest impact but, a few years later when contact was made with theoretical physics, they became extremely popular. In particular Witten's work on global anomalies brought our η-invariant into prominence, in a way which we could never have foreseen.

Sadly these papers were the end of my collaboration with Patodi. He returned to the Tata Institute and we continued to correspond for a while but later his health deteriorated, and a kidney transplant became the only hope. I was involved in the medical discussions concerning this and at one stage it was planned to bring him to Oxford for the purpose. However, this was eventually deemed unnecessary and plans were made in Bombay, but sadly Patodi died of some preoperation complications. It was a tragic loss both personal and mathematical. Patodi was a mathematician of real originality and power. He was charmingly modest, with a friendly and captivatingly simple disposition.

Although elliptic differential equations constituted the centre of my mathematical interest for many years, there was an interesting collaboration with Bott and Gårding on hyperbolic differential equations which led to the papers [84–86]. Our collaboration began in an unusual way. Bott was staying at the time in Oxford and Gårding came from Lund for a few weeks. He said he had a problem for us. There was this important but obscure paper of Petrovsky which involved some homology of algebraic varieties. Bott and I were essentially contracted to understand *and explain* Petrovsky's paper. We were at the time very ignorant about hyperbolic equations, but we had in Lars Gårding a world expert and an excellent tutor. In return we instructed him in topology, and so the collaboration began with mutual education. In due course we managed to understand Petrovsky, then to modernize and generalize, leading eventually to our rather lengthy joint paper. Altogether it was an enjoyable and instructive episode.

Another part of my education in analysis had of course been going on for some time with Singer (and at other times Hörmander) as tutor. In particular I learnt from Singer, who had a strong background in functional analysis, about von Neumann algebras of type II with their peculiar real-valued dimensions. We realized that *K*-theory and index theory could be generalized in this direction, but it was not clear at first if such a generalization would really be of any interest. However in one particularly simple case, that of a manifold with an infinite fundamental group, it became clear that the ideas of von Neumann algebras were quite natural and led to concrete non-trivial results. This was the content of my talk [89] at the meeting in honour of Henri Cartan. Since I was not an expert on von Neumann algebras I attempted in this presentation to give a simple, elementary and essentially self-contained treatment of the results. Later on in the hands of Alain Connes, the world expert on the subject, these simple ideas were enormously extended and developed into a whole theory of linear analysis for foliations.

For many years I had taken a general interest in the representation theory of non-compact semi-simple groups. In fact this was such a major industry at Princeton, and it had so many ramifications, that it was impossible to ignore it. On the other hand the work of Harish–Chandra was forbiddingly technical and I constantly hoped that more geometrical methods might lead to a conceptual simplification. In particular I was attracted to the idea of realizing the discrete series representations by solutions of the Dirac equation. This seemed a natural generalization of the case of compact groups where the Borel–Weil theorem could also, as I knew, be reinterpreted in terms of the Dirac operator. I had a number of discussions on those topics with Wilfred Schmid, after which I realized that my paper [89] could be used as an existence theorem for square-integrable harmonic spinors on suitable homogeneous spaces. I wrote to Schmid, explaining this idea, and he soon saw how one could really develop much of the theory in detail from this starting point. However, to avoid relying on Harish–Chandra's work it was necessary to find an alternative direct proof of the fundamental theorem on the local integrability of the irreducible characters. In 1975 Schmid and I both spent a term at the Institute for Advanced Study, and during that time we worked out a reasonably satisfactory proof of the local integrability based on a careful study of invariant differential operators and conjugacy classes. We planned to write this up as a joint paper, but first we decided to write up quickly the work on the discrete series. Unfortunately this took much longer than planned, ended up as a much more substantial paper [90] than originally planned. The other project, on the local integrability, got postponed indefinitely but the essential ideas were explained in a series of lectures [91] I gave at Oxford in the Spring of 1976.

Another set of lectures [88], given at a summer school in Italy in 1975, give a leisurely account of index theory. Although not containing any new material these lecture notes remain perhaps a useful quick introduction to the subject.

The η-invariant which was introduced in my joint paper with Patodi and Singer had been, in part, motivated by Hirzebruch's work on cusp singularities and in particular his result expressing the signature defect in terms of values of L-functions of real quadratic fields. Singer and I saw how to rederive Hirzebruch's formula from our general results and we planned to extend this to deal with the conjectured formula for totally real fields of any degree. However, there were some analytical difficulties in the general case which needed careful treatment, so that matter was postponed. Many years later with the assistance of Harold Donnelly we completed the programme resulting in the papers [92], [93]. Independently, and almost simultaneously, similar results were obtained by W. Müller in East Germany.