

RECOLLECTIONS FROM THE EARLY DAYS OF INDEX THEORY AND PSEUDO-DIFFERENTIAL OPERATORS

R.T. SEELEY ET AL.

Informal post-dinner conversation with various recollections, among them autobiographical sketches from Bob Seeley (then Brandeis), Peter Gilkey (then Courant Institute, NYU), Misha Shubin (then Moscow), Werner Müller (then Berlin), and others.

Editorial Remark: During the Symposium on ‘Spectral Invariants - Heat Equation Approach’, which was held near Roskilde (Denmark) in September 1998,¹ we spent a leisurely evening, during which senior participants shed some light on the early days of index theory and pseudo-differential operators. The following is a transcript of parts of the taped discussion with some notes, remarks, and corrections added later. *Roskilde, November 1999, BBB, KPW*

Seeley: (Note. This addresses the questions that were raised in the discussion at the conference, but was written somewhat later. There were parts of the story that I didn’t know for sure, and what I did know was not clearly remembered at the time. This is how I understand it now, thanks to help from Palais and Singer.)

The questions concerned the origin of the Atiyah-Singer Theorem, and the analysis required in the proof. About this, Singer says:

“The Atiyah-Singer collaboration began in January 1962 in Oxford, with the following exchange.

ATIYAH: Why is the \hat{A} genus an integer for spin manifolds?

SINGER: You know the answer better than I - why do you ask?

ATIYAH: There must be a deeper reason.

¹See B. Booss-Bavnbek, K.P. Wojciechowski (eds.), **Geometric Aspects of Partial Differential Equations**, Proceedings of a Minisymposium, held September 1998 in Roskilde, Denmark with support from the Danish Science Research Council, AMS-Series *Contemporary Mathematics*, vol 242, Providence, R.I., 1999.

In March, I suggested a deeper reason: The \hat{A} genus is an integer because it is the index of the Dirac operator.

We quickly worked out the index formula for the index of geometric operators. We showed Smale what we were doing when he visited Oxford in May(?). He called our attention to Gelfand's paper which led us to the work on singular integral operators by Agranovic/Dynin and Seeley. We realized that if one could prove our theorem for the Dirac operator (coupled to vector bundles), we had an index theorem for all elliptic operators."

Generalizing from differential to singular integral operators made the topology simpler (because of the freedom in making homotopies) but required more analysis. Fortunately for me, some of this was in my then-recent thesis under the direction of Calderón. He had suggested that I develop the Calderón-Zygmund singular integral theory on manifolds. The two main points were the effect of coordinate changes on the symbol, and the construction of the square root of the Laplace operator, to serve as an isomorphism between Sobolev spaces, and to reduce differential operators to bounded operators on L_p . The thesis was for scalar operators, but the extension to vector bundles was routine. One new bit that they needed concerned products of manifolds. The symbol of the tensor product of two singular integral operators of order zero is discontinuous, but this can be avoided by raising the orders.

The hardest part of the analysis in the original index proof is the Cobordism Theorem - the index of a certain operator is zero when the underlying manifold is a boundary. This was presented in the seminar run by Atiyah, Bott, and Singer at Harvard in the fall of '62, using the ideas of Agranovič and Dynin. The proof in the Palais seminar is quite similar, but uses Calderón's approach to boundary problems.

The first index proof, and the later one which Atiyah and Singer published in the *Annals of Math*, were eventually replaced by *local* versions. The interest in this was brought to my attention by Bott, in 1965 or 66. He asked whether the analytic continuation of the zeta function for the Laplacian, given by Minakshisundaram and Pleijel, might not be possible for more general elliptic pseudo-differential operators, to yield a local index formula. (Around the same time, independently, Singer was discussing essentially the same idea, the McKean-Singer formula, which uses the asymptotics of the heat equation instead of the analysis of the zeta function.) The affirmative answer to Bott's question was found by combining an idea of Calderón (to construct the square root

of the Laplacian as a contour integral of the resolvent) with another due to Agmon (to treat the resolvent parameter as another homogeneous symbol variable). This showed that the index is indeed given by a local formula. To identify the integrand in the interesting geometric cases went beyond mere analysis; this was done (in varying degrees of generality) by Kotake, Patodi, and Gilkey, between 1968 and 1976; a sketch of these developments is in Gilkey's book *Invariance theory, ...*

Much later, when Singer was working on the APS problem, I had just finished a paper on the resolvent of differential boundary value problems. So he asked me about pseudo-differential boundary conditions. I didn't think it would come out quite the same, but foolishly didn't really pursue him about why he wanted it. Much later, when Gilkey asked about the resolvent expansion for that situation, I realized that it could be done in a straightforward way (at least for the cylinder case), yielding among other things the APS formula. This is in a recent joint paper with Gerd Grubb.

Question: How did you come to global analysis and index theory?

Seeley: Well, my involvement with this famous theorem is a result of a number of happy accidents. The first was being a graduate student at MIT, looking for a thesis advisor, just when Calderón was looking for a student. Calderón proposed a problem in measure theory, but I couldn't make sense of it. So Calderón said, 'OK, try this. I've got these ideas about singular integrals, and I think they should be done on manifolds.' This was indeed a very nice problem, with the two main points mentioned before. So Calderón had really given me two great ideas.

Then I went away to a college and taught undergraduate engineers for 3 years. A fortuitous encounter with Leopoldo Nachbin led to my going to Brandeis. When I arrived in summer 1962, Dick Palais was there and I met him at the first party. He asked what I was interested in and I said, 'well, there are all these singular integrals, and on the circle there is this interesting topological formula for it with the winding number. There must be something similar in higher dimensions.' He said, 'Well yes, you know Atiyah and Singer are working on that right now, right here at MIT.'

So, somehow I became a consulting engineer for these architects. I think Singer suggested my reading Hirzebruch's book, but I had a very hard time with it. I would have needed a sympathetic seminar to work through it. So I didn't see the overall structure as they did. But they would have questions about what you can do with the analysis. That was exactly the sort of thing I thought about.

Question: How was the index theorem received in Moscow?

Shubin: There was a seminar in Moscow in 63-64 where many good people participated, in particular, M. Agranovič, A. Dynin, S. Novikov, M.I. Vishik and others.

Wojciechowski: So the proof of the index theorem was already published?

Shubin: At that time no detailed proof was published, only the short note was published. I was too young then and did not start to study PDE, so I did not even know about this seminar. I was told about this seminar 3 years later when I developed my own interest to the index theorem and organized my own seminar where we studied the proof. I was a first year graduate student then, and the participants of this seminar were several good graduate students.

Booss-Bavnbek: Atiyah announced the index formula at the Arbeitstagung in Bonn in July 1962. About this Hirzebruch says:

"Atiyah hielt den berühmten Vortrag 'Harmonic Spinors and Elliptic Operators', wo $Spin(X) = \hat{A}(X)$ angekündigt wurde. Dies war aber noch nicht bewiesen. Serge Lang war wie wir alle begeistert und schrieb sofort eine Ausarbeitung des Vortrags, die in Bonn vervielfältigt wurde."

Parallel to the seminar at Princeton, we had a seminar in Bonn in 1964. But the analysis was too difficult for us in Bonn. I was a student then and had to report on Seeley's work: how to define the singular integrals on manifolds. Hirzebruch was actually very eager to catch the analysis part. As I remember, he gave classes in functional analysis, distributions, mathematical physics etc, just to have a thorough understanding of the underlying analysis. It was a strange situation: Everybody in Bonn understood the topological side. This was a beautiful confirmation of Hirzebruch's ideas. But in the beginning, nobody could understand the analysis side (except for M. Breuer).

Shubin: It was vice versa in my seminar. We understood the analytical side more or less, but the topological side was more difficult to us. But I was told that in the 1963/64 seminar Novikov explained all the topology which was necessary.

Booss-Bavnbek: Who made the first connection of the index with the zeta-function?

Wojciechowski: The formula that $\zeta(A^*A)|_s - \zeta(AA^*)|_s = index(A)$ at $s = 0$ is due to Bott. But who really made the connection of the determinant with the zeta prime?

Seeley: Maybe Singer. I'm pretty sure of that.

Wojciechowski: This we know, because you can safely refer to the Analytic Torsion paper which was written with Ray, but was there any connection before that?

Müller: In this context it was definitely invented by Ray and Singer in their well known paper. They started out with the usual definition of the Reidemeister torsion, which is a combination of determinants, and then they converted it into an expression involving the determinants of combinatorial Laplacians. Then they took this formula and replaced the determinants of the combinatorial Laplacians by the zeta regularized determinants of the smooth Laplacians. I think that's how they invented their formula for the analytic torsion. In this way they also introduced the method of zeta function regularization of determinants of elliptic operators.

Question: How did you come to analytic torsion?

Müller: My thesis adviser, a topologist in Berlin, didn't give me a good problem. In fact, no problem at all. By chance, I ran into the paper by Ray and Singer and I found it - for reasons I can't remember anymore - very interesting. This problem looked very challenging to me, because first of all, we had studied the Atiyah-Singer index theory to a great extent, and this was a favourite subject if you were a topologist or an analyst. So I saw the Ray-Singer paper and started to think about it.

The other paper I had seen recently was the one by Dodziuk and Patodi on the approximation of eigenvalues for the Laplacian on forms using a version of the finite element method. In fact, Dodziuk had started the subject probably for the same reason: to see if this method could be applied to solve the Ray-Singer conjecture. Knowing the formula of Ray and Singer, which expresses the Reidemeister torsion as a combination of determinants of combinatorial Laplacians, this approach was quite natural. The starting point for applying the finite element method in this context was Whitney's idea that the combinatorial chain complex of any smooth triangulation of a compact smooth manifold could be naturally embedded into the de Rham complex of L_2 -forms. Then Dodziuk and Patodi, using the inner product on the co-chain complex induced by the Whitney map, defined an appropriate combinatorial Laplacian and they were able to show that the eigenvalues of this combinatorial Laplacian approximate the eigenvalues of the smooth Laplacian.

Of course this doesn't settle the problem at all, because from this result follows only that the zeta function of the smooth Laplacian could be approximated in the half plane of convergence by the zeta function of the combinatorial Laplacian. But, in order to define the torsion or

the determinant, one has to continue analytically the zeta function to a neighbourhood of zero. Then, of course, the main issue was to see how one could get across the poles. Well, I was able to manage this problem, which finally led to a proof of the Ray-Singer conjecture.

I sent the paper to Singer, at least the first version of it, and I learnt from Singer that Cheeger was working on the same problem. That was in 1975.

Shubin: How did you (Gilkey) come to the index theory?

Gilkey: I was a graduate student at Harvard but wanted to learn PDE's. Fortunately, I had a NSF graduate student fellowship so I went on 'travelling guidance' from Harvard to NYU; I remained a Harvard Graduate student (and got a Harvard Ph.D. and my Harvard 'reader' was Bott) but essentially I transferred to NYU, where Louis Nirenberg was giving this course on pseudo-differential operators. When I went to NYU, Bott (who was my Harvard pre Ph.D. advisor) told me to learn PDEs from Nirenberg. So I took Nirenberg's course. He was basically working his way through Bob's paper, trying to understand all the details. I wanted to work with him and he gave me this problem on something else that I couldn't make any progress on. I put it away and ignored it. During the course, he said that there was this wonderful invariant that Bob Seeley had constructed analytically and, 'here is Bott's proof of the index theorem, and somebody should actually show that this gives a heat equation proof of the index theorem.' That struck me as a fun problem, so I went home that night and gave the heat equation proof to the Gauss-Bonnet-Theorem.

I asked Nirenberg how long my thesis should be and he said 250 pages, which I took very literally. He was only joking, but when I wrote up the mathematics, it only ran 120 pages so I was in terrible trouble because I had written all I knew on those 120 pages. So I wrote a computer program, computing asymptotics in the heat equation. The output added another 130 pages. And I just printed all that up and added it to the thesis.

It was funny, nobody ever looked at those calculations until much later on, when Peter Sarnak got a copy of the thesis and looked at those formulas for the heat equation asymptotics on two-dimensional manifolds and observed something very interesting about them: that you've got coercive control of the covariant derivatives of the curvature tensor, and then he said that maybe the pattern continued. So he sat down and wrote out a proof in general and that is a central feature of the Osgood-Phillips-Sarnak paper which deals with the 2 dimensional case to show that isospectral families of metrics on Riemann surfaces are compact modulo gauge equivalence.

Then when I got a copy of his paper, my response was ‘oh ****’ as I should have seen the pattern for the leading terms back in 1972. So I sat down and generalized the “leading term” analysis to higher dimensions; this was used later by Chang, Perry, and Yang in their analysis of conformal isospectral problems in dimension 3. (My first proof was pretty combinatorial; latter with Branson and Ørsted we got a better proof).

But to get back to 1969. Nirenberg gave me this problem on the wave equation and kept asking, ‘How is it going, Lad?’ and I pretended that everything was fine and that I was working on it. But in fact I was fascinated by the index theorem and working on that. After doing the Gauss-Bonnet theorem, I took the subway to visit Singer at Rockefeller, and he said that I should really be working on the signature complex and not on the de Rham complex. That was the weekend before Thanksgiving, so I went up to my aunt’s house in Rhode Island and all the way up on the train I was filling page after page with garbage, trying to understand the signature theorem. I worked all weekend and I managed to get the heat equation proof of it. When I saw Singer next, he told me to go to the Institute for Advanced Studies in Princeton where Atiyah, Bott and Patodi were spending the year and to talk to them. I gave a terrible talk on my work there to those 3, but I did manage to get across the idea of the main theorem in invariance theory which was needed to make things work.

Booss-Bavnbek: How did you (Shubin) come to index theory and spectral invariants?

Shubin: M.I. Vishik gave me the 2 volumes of the Cartan-Schwartz seminar about the Index Theorem. It was my favourite source, because it had all the topology explained very well, including K-theory, characteristic classes and cobordism. Necessary analysis was explained too. We discussed all this in my seminar. I actually wanted to translate the Cartan-Schwartz seminar into Russian but it occurred that it was too late because the Princeton seminar was already accepted for translation and it was considered redundant to translate two books about the same topic.

Rozenblum: Misha’s book was the first book written in Russia about pseudo-differential operators.

Shubin: I made lectures in 1972-73 which were written down and submitted for publication in Moscow in 1973. But the “Nauka” Publishers did not accept it with the motivation that the topic (pseudo-differential operators and spectral theory) is too special and only a narrow circle of people would be interested. I am still curious who wrote this review. I resubmitted the same manuscript a couple of years later after

some (non-mathematical) preparations. Arnold gave me a good advice how to do this and where to submit. I went to another division of “Nauka” and this time it was published, so it appeared in Russia in 1978. Springer-Verlag decided to publish the English translation almost at once but unfortunately this took 9 years because of troubles with the translator.

Booss-Bavnbek: How did you come from analysis to geometry?

Shubin: I was actually interested in geometry from my undergraduate years. In particular, being a second year undergraduate student I took a course by Rashevskii on Riemannian geometry and general relativity, and also participated in a good seminar on Lie groups by Vinberg and Onischik. I also attended some Arnold lectures about classical mechanics and symplectic geometry in Moscow. Much later I profited a lot from my contacts with Gromov and Gelfand in Bures-sur-Yvette. But in 1964-65 I moved to Analysis and PDE. In 1966 I learnt pseudo-differential operators from a short (about 17-pages) paper by Hörmander. (This was his first paper about this which appeared right after the Kohn and Nirenberg paper.) Then I started learning index theory by the Cartan-Schwartz seminar volumes, and in particular had to learn relevant topology. I enjoy topics which involve interacting between analysis, geometry.

One of the papers which influenced me a lot was a paper by Coburn, Moyer and Singer (1973) about the index of almost periodic operators. It was fascinating because it included two incredibly beautiful things at once: almost periodic functions, which I learnt at this moment (by an excellent book by Levitan), and then von Neumann algebras (I read Dixmier and Naimark books about this). The most fascinating thing with the von Neumann algebras was for me that, say, subspaces can have dimension $\sqrt{2}$ or π . After I started reading these things I immediately got the idea that you can use von Neumann dimensions in the spectral theory by measuring spectral projections of elliptic self-adjoint operators. In this way you get a spectrum distribution function, which later proved to be equal to the integrated density of states already known from physics of solids. I learnt about the integrated density of states from Berezin; the coincidence of the von Neumann spectrum distribution function with the integrated density of states was his conjecture which I proved.

I spent some time working on almost periodic operators and then switched to random operators. Then I explored other possibilities to use non-standard traces in the same way. For instance, the next one

came when I saw the lecture notes by Atiyah about transversally elliptic operators and figured out how we can introduce the spectrum distribution function for them, expanding over the characters of the compact group which acts there.

Then S. Novikov already in the 80s considered the following situation: there were Morse inequalities, but they also hold if you consider finite covering and then divide the result by N , the degree of this covering. So then he said, ‘how can you interpret it, if you let N go to infinity?’ He had an idea that it could be interpreted by the use of von Neumann algebras, which would naturally yield a more general result. He discussed this with several people, but then I heard about it and more or less was able to come up with the proof of these Morse inequalities next day. This led to a joint paper with Novikov. This paper also contained an idea of the spectra-near-zero invariants.

Wojciechowski: How did you meet Berezin?

Shubin: F.A. Berezin was a great personality and I miss him very much. We met in a summer school in Baku, on the Caspian seashore. The school was about spectral theory and group representations and I was a graduate student of first year. We lived 3 people in the same room: me, another graduate student (G. Litvinov) and Berezin. I realized much later how young he was at that time (about 36), but for me then he was like a patriarch who descended from Olympus. G. Litvinov talked about some quantum mechanics but we didn’t know anything about it. Then Berezin asked us if we wanted to learn quantum mechanics and we said, ‘yes of course’. He said, ‘let’s try to write a book about it’. This was kind of strange because at that time I couldn’t imagine my name on the cover of a book - the idea was strange to me. I asked what we should do and he said we should just write down what he said.

He had lectured about this and had some notes. He gave us the notes and I started reading them. Then we realized that there were almost no proofs there. Even foundations were not explained, there was just a rough sketch. So I started writing something, and Litvinov started writing too, but he dropped out quite fast and I was left alone eventually. So I had to invent all the proofs and when I had a difficulty, I asked Berezin and he was always able to clarify it to me. After that I had to work more on it to understand what he was talking about and maybe invent more in the line of proof. But, unfortunately, I did it very slowly. It just took so much time, and I felt there was no hurry.

Actually, Berezin taught me a lot of things. In particular, he taught me not to be afraid of talking with physicists or attend physics seminars. He ran his own seminar where physicists came to consult him on different things. Also Albert Schwarz participated in this seminar.

Once Berezin invited me to his seminar and then I heard about a talk which would be there. I told Berezin that I doubted that I would understand anything in this talk (which was about quantum field theory). Then he said, 'what is there to understand? Do you know already that the quantum observable is just a selfadjoint operator in a Hilbert space?' I said yes. Then he said, 'you really don't need anything more, you'll understand everything.' So I came to the talk and saw that he was more or less right. If you know these things approximately, then you can understand something and eventually more and more.

Here is what happened with the book. We published some mimeographed edition. The process continued. Eventually, M. Hazewinkel who represented Kluwer publishers, came and said they were interested. But I was lazy, which I'm very much sorry about. So this small book existed, but aside from this something like 60 % or maybe 70 % of the work still had to be done, when Berezin died in an accident.

People, especially A. Schwarz, encouraged me to finish the book as soon as possible, for otherwise it would not be possible to publish it. So, all I had was one or two years. OK, I started learning, working fast, but of course I couldn't do many things because I had nobody to ask at this particular moment. And so I finished it in a year, more or less, after that.

Actually, Berezin taught me many non-mathematical things too. For example, once he took me to a cross country skiing near Moscow. He was in a much better physical shape than me in spite of the age difference, which was like 15 years. He went ahead and since I could only move much slower, he used to return to see if I was still alive and then go forward again. At some point we had to cross a small stream, maybe 3-5 meters wide or so. It was minus 10 degrees (C), but the stream was not frozen and we were quite far from any town or place where we could get warm. There were two long but thin fallen trees lying parallel across the stream, about 3 meters above the stream, and we were supposed to step along them using our skiis. I said I don't understand how we can do this. He said that it was very easy and showed me. He went over and back again. He was used to do risky things. I crossed, too. I had no choice.