A Conversation with Alan Weinstein

Henrique Bursztyn and Rui Loja Fernandes

Alan Weinstein has been one of the most prominent and influential differential geometers of the last five decades. He has made fundamental contributions to such diverse areas as Riemannian geometry, symplectic geometry and Hamiltonian dynamics, geometric mechanics, microlocal analysis and quantization, Poisson geometry and Lie groupoids, as well as their various interconnections and applications. Alan will turn 80 years old in June 2023. This conversation took place over Zoom in May and June 2022, while Alan was at his home in Palo Alto, recovering from COVID.

Growing Up in New York

R. You grew up in New York. How was it? Do you remember well the transformations that were occurring there at the time, some of which were portrayed in West Side Story?

A. I grew up in the city only until the age of eight and then my family moved to Long Island to a quite well-off neighborhood, which was almost all white. There were a few African-Americans, maybe one or two Hispanics and one Asian family. So the cultural changes portrayed in West Side Story didn't really impact me very much. But it was a time when people were starting to get politically active. In the late 50s, there were starting to be many protest marches for integration, and I got involved in some of that stuff.

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DOI: https://doi.org/10.1090/noti2595

H. And how was high school?

A. I attended Roslyn High school, where my interest in mathematics was particularly encouraged by a teacher named Anthony diLuna. He had graduated from a master's program at the University of Chicago, which was one of the first places encouraging a more creative approach to teaching math. There is now a scholarship named after him in my high school. I think I had him for advanced algebra, and maybe trigonometry. Apart from that, as a senior, I did an Advanced Placement Calculus class which, in some ways, was kind of a waste, as the teacher wasn't very good, so I was learning calculus on my own. I published my first paper in a journal for high school students [Wei60]. It was on the symmetry of the graph of a cubic equation around its inflection point. I believe that I did it by translating the inflection point to the origin and then showing that the resulting function was odd.

H. In high school, did you take part in any math competitions?

A. I was a "mathlete." There were high school math teams who met on a regular basis, and we'd go to a different high school for competitions. One would normally spend an hour doing problems, and I did pretty well. At the time there were not yet Math Olympiads [they started in 1959 in Eastern Europe]. Later, at MIT, as a junior and senior, I took part in the Putnam Math Competition and got an honorable mention once.

H. How did you get motivation to study math? At home from your parents?

A. Not particularly. They were happy enough to have me study math. They might have wished that I had gone into medicine or law, which was typical for parents at that time, but I never had much interest in that.

Figure 1. Alan Weinstein.

Attending College at MIT

H. How and why did you end up at MIT?

A. I went to MIT partly because I had applied for a Grumman Scholarship, which would have required me to major in engineering. At the time, I thought I might be interested in engineering. I was a runner up for this scholarship, but I went anyway to MIT thinking I'd still major in engineering. Since I didn't win the scholarship, I didn't really have to. I think there were two things that changed my mind. One was two really good teachers I had for calculus in the freshman year. The other was that I didn't much like the laboratories in chemistry and physics. At that time in MIT, every freshman took the same courses: math, physics, chemistry, and something called humanities, which was mostly literature. One of the teachers I had for that, A. R. Gurney, became a quite famous playwright. I also studied Russian for no particular reason, except it was something different.

R. I guess that allowed you years later to translate Arnold's famous book on Classical Mechanics.

A. Yes, that allowed me to translate Arnold's book, along with Karen Vogtmann, who was a graduate student in Berkeley at the time (and actually did the majority of the work). I haven't read anything in Russian for a long time, although I can read in Cyrillic when something appears in the news these days related to the Ukrainian war.

H. How was your experience as a math undergrad at MIT? Who were your main mathematical influences there?

A. I had two wonderful semesters of honors calculus taught by James Munkres and Gian-Carlo Rota. This was almost like a real analysis class, where we started by defining the real numbers and proving everything. We used Courant's calculus book, and we started with sequences and limits of sequences, because that was easier to handle than limits of functions where you have to worry about both deltas and epsilons, while here you have only an N and an epsilon. And then it went on. I think there were even some infinite series before we started with functions of a real variable.

Rota was not so well-known at the time, and he was more of a real analyst, before he became a famous combinatorist. But that was after my time at MIT. I had one more course from him, namely probability, an upper-division class.

Another professor whom I remember having a significant influence on me was Irving Segal, who was writing a book with Ray Kunze on *Integrals and Operators*, for a first graduate course on real analysis. I served as an informal "copy editor" for that manuscript.

H. Do you remember who taught you differential geometry at MIT?

A. The undergraduate class in differential geometry was taught by someone who wasn't at all in differential geometry. But by my senior year I was taking, like many students do, some graduate courses. And I took differential geometry with Sigurdur Helgason, who used his differential geometry book. That was a very good class, and he was a very good teacher, too.

R. By Helgason's book, you mean *Differential Geometry, Lie Groups and Symmetric Spaces*?

A. Yes. That's the one. It is a large book, and I think we only did about the first five chapters. But it became a reference for me later on. Certainly, an important book, which I kept in Berkeley.

R. At MIT did you get to know Victor Guillemin, or did you meet Shlomo Sternberg at Harvard, who later became major figures related to your work?

A. No. I don't think Victor Guillemin had arrived at MIT yet. Or if he did, I had no idea of him. I did interact with Henry McKean with whom I took a class in complex variables. Much later, when I visited NYU for a semester, I actually ended up writing a joint paper with him on solutions of the sine-Gordon equation [BMW94].

R. Is that paper related to your paper with Andreas Floer which, to our surprise, is your most cited paper on MathSciNet?

A. No, the paper with Floer was on the the nonlinear Schrödinger equation [FW86]. The other collaborator on that paper with McKean was Bjorn Birnir. Recently, I saw a reference to our paper so I looked him up, of course, to see what he was doing. And he's been involved in quite practical fluid dynamics, including the transmission of COVID in the air. So I should reach out to him and tell him I just caught it!

Graduate School at Berkeley

H. When did you decide to pursue a PhD in mathematics? Was it a planned decision? Why did you pick Berkeley for graduate school?

A. Not really planned. I guess I didn't really think about it. I just did it. You know, by the time I was a junior I realized I would probably go on to graduate school and I started talking to people about it. In addition to Berkeley, I think I applied to either Princeton or Harvard, I don't even remember. I might have gotten in because I did very well at MIT, but I chose Berkeley partly just to get far away from where I had lived until then and see another part of the country.

H. Tell us about your experience as a graduate student in Berkeley in the 60s. Math environment? Flower power movement? Anything unmentionable (laughs)?

A. Not much flower power, but the Free Speech Movement made a great impression. Also amusing was the subsequent Filthy Speech Movement.

The math culture was great. In those days before the internet, people actually talked with their colleagues. There was a daily geometry lunch at the Student Union attended by many of the faculty, to which graduate students were invited as well. There was a wonderful seminar (run by Smale, I think) going through the proof of the Atiyah–Singer index theorem, which had just come out. Things were much less competitive than they became later, partly I suppose because there was no shortage of jobs in the 60's.

H. When you went to Berkeley, did you already know that you were going to study with Chern?

A. No, I did not. Actually, I didn't even really know Chern. But I pretty much knew that I wanted to do differential geometry, since I liked the subject so much after the class from Helgason at MIT. Once I got to Berkeley, I took a beginning graduate class from Frank Warner, who again is the author of an excellent text. (It seems that I had classes from the authors of lots of good textbooks. Helgason and Segal at MIT, and Frank Warner at Berkeley.) He helped cement my interest in differential geometry. Then, in my second year, I took a topics course with Chern on integral geometry, which was one of his interests. Anyway, that cemented my interest in differential geometry, and I decided to work with Chern.

Figure 2. Alan Weinstein in Berkeley, *circa* 1972.

H. How was it to have Shiing-Shen Chern as an advisor? How did you find your thesis problem?

A. Chern was a great advisor. He mostly listened and encouraged. Chern and his wife Shih-Ning were great hosts, and their home was a center for social life in the geometry community. Regarding my thesis, at some point I met with Frank Warner and Hung-Hsi Wu; we used to go to the geometry lunch I mentioned before. Chern occasionally would also go to this lunch and it may have been there that I heard about a problem that Warner and Wu were working on, which was on a Rauch conjecture about conjugate points and cut points on Riemannian manifolds. So I got interested in that and I started thinking about it. Then at some point, they said, "Well, Alan, you're thinking about this, we'll leave it to you." So that became my thesis, and Warner and Wu became members of my thesis committee.

Wu and I became colleagues when I returned to Berkeley as an Assistant Professor. Wu has been at Berkeley the whole time and now he is my office mate!

H. How long were you in graduate school?

A. I was there for only three years. I was lucky that I found this problem. And I was able to write a 27-page thesis and get out.

H. That was a short thesis!

A. I guess that was all the length I needed. And maybe I was lazy, too. In fact, in February of my third year, I had basically finished writing the thesis. I don't remember when I turned it in, but I went on to spend some time in Paris. I had a car then, so I drove back to the East Coast, via Los Angeles, where I attended one of the first of the so-called *Geometry Fiestas*. I think they are now called the *Geometry Festivals* and they are centered at U Penn. It's an annual, mostly differential geometry, meeting. This one was an early one at UCLA. So I went there first, and I actually gave a talk on my thesis, the first meeting that I'd ever been to.

H. What was your connection with Paris?

A. I got to go to Paris because Chern had some connections with IHES, since he had been there many times. The idea of spending time in Paris, came up in the previous summer (that is, the summer of 1966, two years after starting graduate school). I had pretty much worked out the solution of my thesis problem by then, and I knew I just had to write it up. My girlfriend Margo was about to go off to Paris, where she was doing a Middlebury College master's program in French. So I wanted to go to Paris as soon as possible, which turned out to be February (of 1967). We were married that May at the Mairie d'Orsay because I had an apartment in Orsay while visiting IHES. It's now been 55 wonderful years together!

R. Was it your first time in France?

A. It was my first time in France. In fact, except once or twice on family driving trips to Montreal and Quebec, it was my first time outside the United States.

H. And how was your French?

A. Well, once I decided that I was going to go to Paris, I started studying French at Berkeley. I audited a French class in the fall semester (of 1966), including the oral language labs.

R. So coming back to your visit to Paris, who were your main mathematical contacts there?

A. My main contact was Marcel Berger, who had been at Berkeley, and I knew him through Chern. He was, in some sense, my main mathematical contact in Paris, although I did get to see a fair amount of René Thom, who was there too. One of the nice things about IHES, and I think it still happens, was that every day there's lunch in a building at the bottom of the hill. And almost everybody would come to lunch. So sometimes I would be at the table with Grothendieck. Zariski was there also that spring and he talked to Grothendieck a lot. This was before the time of Deligne.

R. At that time, you began to change your research a little bit. How did that happen?

A. Jeff Cheeger's work was starting to be well known. His work on manifolds of nonnegative curvature and his finiteness theorem for Riemannian manifolds with a bounded curvature, were considered very important. So I remember reading his thesis carefully. At that time, I proved an estimate for the number of homotopy types of positively pinched manifolds [Wei67]. (It was vastly improved by Cheeger.)

I was also working on topics related to Palais's work about actions of compact groups on manifolds. I never actually wrote a paper on that because Palais's paper on proper actions came out, but I attempted to give a talk about that in French; I think I was feeling overconfident. One of the people in the audience was Bernard Morin, a French mathematician who made the first models of turning the sphere inside out. Even though he was blind, he developed an algorithm for doing that. So I was giving my talk and I drew something on the board and he asked me to describe the picture. That was very challenging for my French, so a French person in the audience had to explain to him what was going on.

H. And this visit, still as a graduate student, was the beginning of a life-long connection with Paris...

A. Yes, Margo and I have been visiting Paris regularly since then. Early on, I came back for a short visit, maybe in 1969, and then for a month or two in 1970. Then in the summer of 72, after our daughter Asha was born, we lived in an apartment in Paris. We went back for a year in 1975–76, and that was back at IHES. It was a really good time. Dennis Sullivan was there and very active. There were several kids who were all about the same age. One was our daughter Asha. One was Michael Sullivan, Dennis's son who is now a mathematician, and one was Christian Gromoll, also now a mathematician, the son of Detlef Gromoll. I think we have a picture of the three of them together. It was kind of fun. By then I was really more interested in curvature-related things and also getting more into symplectic geometry.

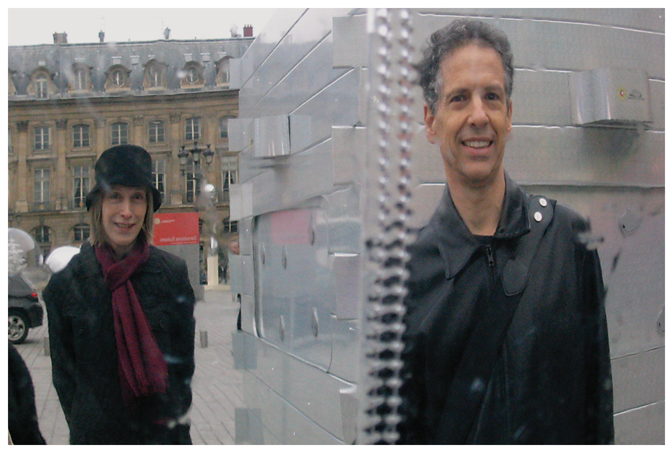

Figure 3. Margo and Alan Weinstein in Paris in December 2004.

H. As a student, who were the mathematicians that you looked up to, that were particularly inspiring to you?

A. Berger, Wilhelm Klingenberg, and Chern of course. They were kind of my mathematical heroes at the time. I was a student, and Riemannian geometry was the thing. I was really interested in curvature, although my thesis wasn't about that. I did write a paper about curvature when I was a graduate student [Wei68]. Klingenberg was also involved in the study of closed geodesics, and so I got very interested in closed geodesics and periodic orbits. Once I got into symplectic geometry, closed geodesics morphed into periodic orbit interest. And that's what led to the stuff I did on periodic orbits, equilibria, and so on.

I also really admired Smale who, as I mentioned before, ran a seminar on the Atiyah–Singer index theorem. Atiyah himself taught a course at Berkeley in the summer of 1968, during an AMS summer conference on global analysis. That was a really great meeting, and Atiyah gave a course on the index theorem. Obviously, I also admired him very much and I had a little bit of contact with him over the years, nothing too close. Later I did a couple of things on the index of Fourier integral operators, though I never got as far as I wanted.

Postdoctoral Years

R. As a postdoc, you went back to MIT and then Bonn, and that was about the time you started to get interested in symplectic geometry. Do you remember when you first heard about symplectic manifolds?

A. It was when I was a Moore instructor at MIT, because I was doing this work on conjugate locus and cut locus. Frank Warner had written a paper about the singularities of the conjugate locus, and I got interested in the subject. I realized that the exponential map was a projection of a Lagrangian submanifold of the cotangent bundle. This hadn't played a part in Warner's work and so I got very interested in that. Arnol'd's paper on the Maslov index had introduced me to Lagrangian submanifolds, though I did not meet Arnol'd until many years later.

R. But according to MathSciNet you had an earlier paper on symplectic structures on Banach manifolds.

A. Yes, from around the same time. This paper used Moser's method. I was getting interested in symplectic geometry and I knew about Moser's paper "On the volume elements on a manifold," where he proved that two volume elements on an oriented compact manifold with the same volume are diffeomorphic. So I figured out how to extend that to symplectic manifolds. This paper on symplectic structures on Banach manifolds, as well as other work I did on normal forms, was all based on Moser's method. For example, I applied Moser's method around Lagrangian submanifolds [Wei71]. By then I was also starting to think about the WKB method, and how it related Lagrangian submanifolds in the cotangent bundle to quantum states. I was also interested in the interface between classical and quantum mechanics, for instance because of relations between the Laplace spectrum and the geometry of Riemannian manifolds, and symplectic geometry turned out to be the right tool for studying that.

H. How long were you at MIT as a postdoc?

A. I did just a year as a Moore instructor at MIT and then I took a NATO postdoc, also for a year, in Bonn. There my sponsor was Wilhelm Klingenberg, who was very involved in pinching theorems. For example, a complete, simply-connected Riemannian manifold with curvature strictly between 1/4 and 1 must be a sphere. That was one of the first topics I was interested in even as a graduate student, because Berger and Klingenberg, who both did pinching theorems, were in Berkeley as visitors brought by Chern. In Bonn, in addition to Klingenberg, there were two of his postdocs, Detlef Gromoll and Wolfgang Meyer, who were working together on some Riemannian geometry problems. After the year in Bonn, I came back to Berkeley as an Assistant Professor.

Back to Berkeley as Faculty

R. During your first years at Berkeley, now as a faculty member, you wrote one of your most cited works on what is now known as "symplectic reduction" or "Marsden-Weinstein-Meyer reduction" with Jerry Marsden. How did you meet Marsden, and how did you start collaborating with him?

A. Jerry and I were attending a class of Smale's on classical mechanics, where of course symplectic geometry is a big part of the story. Smale had proven a version of symplectic reduction for cotangent bundles and lifted actions from group actions on the base. Jerry and I figured out how to do this for general symplectic manifolds and Hamiltonian group actions, and so we wrote that paper on reduction [MW74]. Only later did we learn that Ken Meyer had discovered reduction on his own.

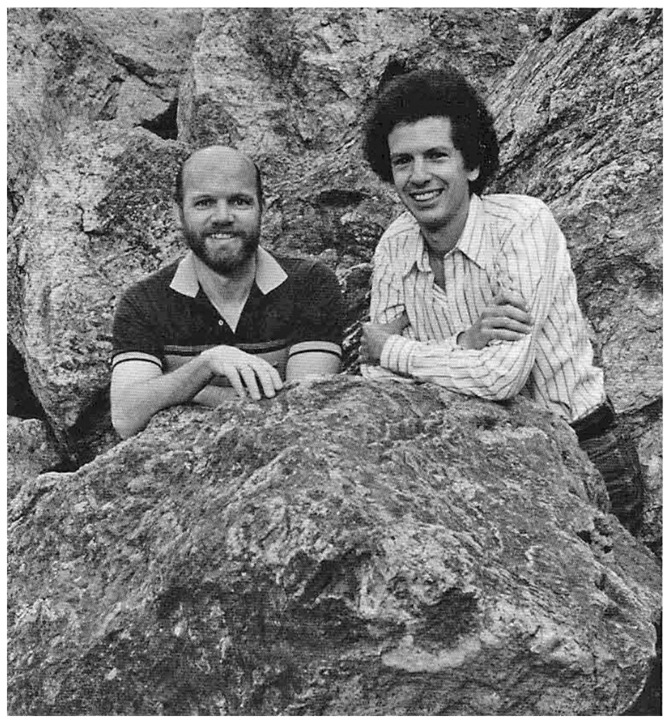

Figure 4. Jerry Marsden and Alan Weinstein.

Soon after, Jerry and I wrote another paper on Hamiltonian dynamics [MW81]. It originated from another seminar we both attended, run by a plasma physicist named Alan Kaufman and his student at the time, Robert

Littlejohn. There they talked about some work of Phil Morrison, who had discovered a noncanonical Poisson bracket and Hamiltonian structure for the Maxwell–Vlasov equations. These are equations for a collisionless plasma, i.e., charged particles interacting with each other via the electromagnetic fields that they produce. Morrison's bracket turned out not to satisfy the Jacobi identity, so we looked at it and realized that we could get a Poisson structure by symplectic reduction from a cotangent bundle crossed with another factor to account for the electromagnetic field. So we had a great simplification and a structure that actually satisfied the Jacobi identity. After six months of work, we figured out how to do this in two minutes! That got us into lots of stuff with infinite dimensional systems and the Hamiltonian structure of many other systems, for fluids and so on.

R. Something you are interested in to these days, right?

A. Something I'm still interested in these days. Moreover, somehow it became clear that Poisson structures were very important, and so I started thinking about Poisson structures in their own right. More or less at the same time appeared the paper of André Lichnerowicz on the subject, which was preceded by an earlier work of Alexandre Kirillov, which also included Jacobi structures, by the way. But I found that one could go a lot further than they had, in various directions [Wei83], and Poisson geometry eventually became a field in its own right. Lichnerowicz was also a visitor at Berkeley, and he gave lectures which had a big influence on me.

H. After returning to Berkeley, one can say you turned into a symplectic geometer...

A. That's right. Although I still wrote some papers in Riemannian geometry, including on curvature pinching, since Chern was of course there and was still very active. He was the head of differential geometry. Frank Warner had moved to Penn by then, but Hung-Hsi Wu was still there.

R. Was it easy to know what was going on in the Russian school?

A. Well, Eliashberg came later, of course. But because I knew some Russian, I could read the Russian journals to follow what was going on. And, of course, there were translated versions of the major journals. I also started having some correspondence (in English) with Maslov and Arnol'd from early on.

H. What about contact geometry? How did you come up with the "Weinstein conjecture"?

A. That came about because, as I mentioned before, I was interested in periodic orbits of Hamiltonian systems, inspired by Klingenberg's and his students' work on closed geodesics. For a Hamiltonian system on a symplectic manifold of dimension $2n$, using a version of Moser's variational method, I was able to prove that in a neighborhood of a nondegenerate minimum of the Hamiltonian, there are n families of periodic orbits [Wei73]. At that time, I also had a student named O. Raul Ruiz, who did a thesis on the existence of brake-orbits in Finsler mechanical systems, which also used variational methods. Then I started looking at convexity, and I was able to use variational methods to prove the existence of periodic orbits for convex Hamiltonian systems [Wei78]. About that time, I was asked to referee a paper by Paul Rabinowitz, where he proved the existence of periodic orbits on star-shaped energy surfaces [Rab79], and somehow just looking at that, I conjectured a wide extension of what he had done.

H. Your conjecture is in an appendix [Wei79] of that paper!

A. Yes, it is there because I was the referee. I thought maybe what lets you apply variational methods to get periodic orbits is the contact nature of the energy hypersurface. In the published version of the conjecture that now carries my name, I included the hypothesis that the manifold be simply connected. The reason was that I (mistakenly) thought I had a counterexample related to the cotangent bundle of a torus. But it turns out from what people did later that the hypothesis was not needed.

Figure 5. Alan Weinstein with his wife Margo, his daughter Asha, and their cats Lucy and Toby in Berkeley around 1985.

R. Coming back to the Poisson brackets, at some point groupoids appeared in the picture too...

A. They appeared because of geometric quantization and deformation quantization. In deformation quantizing a Poisson manifold, the objects you're deforming are the functions on the Poisson manifold. A WKB approach involves looking at functions on a Poisson manifold as Lagrangian submanifolds in its cotangent bundle. If you had a product on the functions on a Poisson manifold, this gave you a kind of binary operation on Lagrangian submanifolds which might imply that the bilinear "quantized" operation should be associative. By then, I had heard about groupoids at a meeting, the Séminaire Sud-Rhodanien de Géométrie in southern France, where Kirill Mackenzie talked about groupoids and algebroids in their own right, and so I made this connection [Wei87]. It turned out that Mikhail Karasev and Viktor Maslov had done something very similar; also, Stanisław Zakrzewski, independently, had thought of similar things. But I pursued it further. (Zakrzewski passed away very, very young. There's a paper of his which I finished after he died.)

R. Although Lie algebroids and Lie groupoids had been around for quite some time, the discovery of their connection with Poisson geometry kind of transformed the subject…

A. That is right, they had been around for quite some time but not in symplectic geometry. Kirill Mackenzie had written several papers on the subject, and Rui Almeida and Pierre Molino, whom I first met in the same Séminaire Sud-Rhodanien, had found the first example of a nonintegrable Lie algebroid.

R. Can we talk a little bit about your creative process? How do you come up with new ideas, and how do you identify interesting problems?

A. I wish I knew!

H. That's a key point of the interview, I'm sure everyone wants to learn that!

A. OK, I'll try. One thing I remember is that for a long time I was interested in lots of different things. Now I'm much less good at multitasking. But I was very good at multitasking back in the day. So I used to think about various things, and sometimes one of these areas gave me an idea that I could apply to some distant problem. That was partially responsible for the variety of problems that I worked on. If I look back, I started in Riemannian geometry, and I kept working on that for some time after I started getting interested in symplectic geometry, and then in microlocal analysis…

Another method which I have frequently used is to approach a problem by considering simple, even trivial, examples, such as the zero Poisson structure.

H. Indeed your research has covered an impressively wide array of topics, including Riemannian geometry, symplectic geometry and Hamiltonian dynamics, semiclassical analysis and PDEs, quantization and noncommutative geometry, Poisson geometry and Lie groupoids, etc. Is there anything that unifies, or a common motivation that explains the breath of your work?

A. One thing came from another. I suppose that the classical–quantum transition was responsible for a lot of it. Since very early on, in fact since I took an upper-division physics class at MIT, I was very interested in the relation between classical and quantum mechanics. You can see reflections of that in a lot of the things I've done that involve quantization. For example, the thesis problem of my former student Steven Zelditch, which was centered on Schrödinger's equation, was an attempt to extend to the noncompact case some previous work on closed geodesics in Riemannian geometry and its relation with the spectrum.

H. It is remarkable that, many times, you had one of the key ideas in a subject, but you don't really pursue it that much and you let other people work on it. For example, for the Weinstein conjecture in contact geometry, which we talked about before, you posed the conjecture but you did not actually work on it afterwards, although it became a huge thing…

A. There is a certain amount of laziness on my part. On the other hand, it often happened that I got interested in something and, fortunately, I had some student who got interested in pursuing it. So I could leave it to him or her.

R. Besides conjectures, there are also these philosophical principles that you suggest and then often everyone adheres to, like "everything is a Lagrangian submanifold," for example, which you called "the symplectic creed"!

A. I am very proud of that. That principle, it was kind of half a joke. I put it at the beginning of a survey article I wrote [Wei82], and it seemed appropriate for a survey. It turned out to be, obviously, an exaggeration (laughs). But if you think about the Fukaya category, for example, the objects are Lagrangian submanifolds. There are many other examples.

R. But the Fukaya category appeared much later than that survey…

A. In fact, I think the idea that everything is a Lagrangian submanifold came mostly from from WKB and geometric quantization. Hörmander's paper on Fourier integral operators had a big influence on me. That paper I probably studied more carefully than any other paper. By then, I was talking with Victor Guillemin at MIT and Shlomo Sternberg at Harvard. There was a nice back and forth exchange of ideas, and that's partly what got me more seriously interested in microlocal analysis. Other people who influenced me were Hans Duistermaat, who wrote notes on Fourier integral operators when he was at NYU, and François Treves, who was a professor at Rutgers and also wrote a two-volume text on pseudodifferential operators and Fourier integral operators.

H. If one considers symplectic manifolds in broader contexts, like graded or shifted symplectic spaces, then your symplectic creed becomes really far-reaching. For example, Dirac structures are Lagrangian submanifolds in an appropriate sense. How did Dirac structures come about?

Figure 6. Alan Weinstein received a honorary doctorate from the University of Utrecht on March 26, 2003. The promoters were Hans Duistermaat (left in this picture) and Ieke Moerdijk (in the background).

A. I was initially motivated by some work of Robert Littlejohn, a physicist at Berkeley that I mentioned before. I was on his thesis committee and his work involved Dirac's theory of constraints. Because submanifolds of Poisson manifolds are, in general, neither Poisson nor presymplectic, there should be something more general. So I gave that problem to one of my PhD students at the time, Ted Courant. Eventually, he came up with the basic theory of Dirac structures and wrote his thesis about them. We also wrote a little joint announcement about it [CW88]. But obviously, Dirac structures caught on much more than we ever thought they would!

R. So you couldn't really anticipate they would become so important...

A. No, not at all. I mean, it seemed like a very good idea. So I pursued it a little bit, writing a paper with Zhang-Ju Liu and Ping Xu where we introduced Courant algebroids [LWX97], and that, together with Dirac structures themselves, is what made the theory explode. Dmitry Roytenberg and Pavol Ševera gave a supermanifold interpretation of Dirac structures as Lagrangian submanifolds, and soon after they appeared in generalized complex geometry. It was first Nigel Hitchin, and then his student Marco Gualtieri [Gua11] who got that subject to take off as a big thing, which I really appreciated. I had met Marco as a student at a conference, and he explained to me what he was doing. Later he visited Berkeley, and we talked a lot, but I never actually did anything much in generalized complex geometry. There is also a connection of Dirac structures with new notions of symmetries, like group-valued momentum maps. These things really made Dirac structures take off!

R. Besides your research, you have also been a very dedicated teacher at various levels. You're the author of several calculus books and have advised nearly 40 PhD students. What can you tell us about your life as an educator?

A. The idea for the calculus books came as I was playing tennis with Jerry Marsden. (Once we started, I never had time for tennis again!)

Being an advisor was one of many things I enjoyed about being a professor. I really liked working with students, with each of whom I had a different kind of relationship. I mostly let them go on their own, as much as they wanted to. Occasionally I had problems in mind, or general areas. I usually had more than one student at a time, which was nice, because they could also talk to each other without me. I occasionally collaborated with students while they were students, but more often I wound up doing collaborations with them after they graduated.

R. One final question: What occupies your mind these days?

A. Besides wondering about where all my time is going, and enjoying doing things with my family, I'm thinking mostly about problems which arise from trying to understand geometric properties of the constraints for the initial value problem in general relativity. This led on the one hand to the discovery of a groupoid symmetry for which the Lie algebroid bracket matched that of the Poisson brackets of the constraints, and on the other hand a theory of compatibility between Lie algebroids over a manifold and presymplectic or Poisson structures on the manifold. Unfortunately, all of this work, which has turned out to be interesting in its own right, has not led to a resolution of the initial question about the Einstein equations, so I'm still trying.

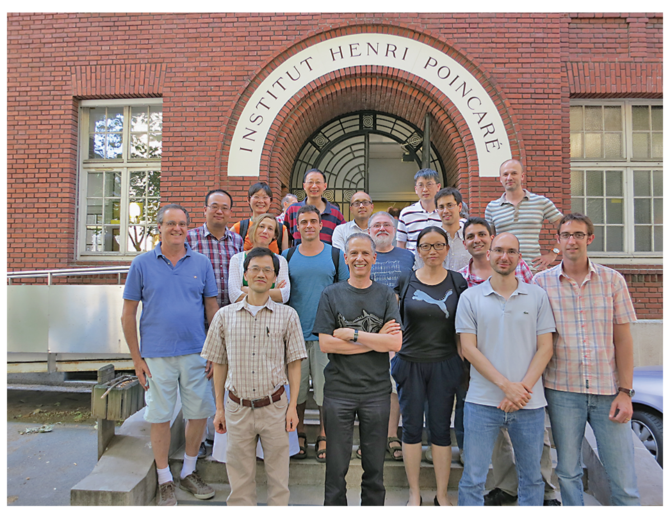

Figure 7. Alan Weinstein with some of his former PhD students, during his 70th Birthday Conference at Institut Henri Poincaré, in July 2013.

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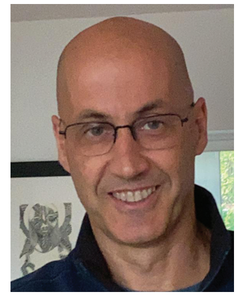
Henrique Bursztyn



Rui Loja Fernandes

Credits

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