FEATURE INTERVIEWS

"The Field is as Exciting as Ever"

Shmuel Friedland Interviewed by Lek-Heng Lim[1]

This interview was conducted in two parts, at the Kurah Mediterranean restaurant on November 16, 2016 and at the Armand's Victory Tap restaurant on December 20, 2016.

— Scholarly work —

L.-H.L. - You have a reputation in the community for going after many daunting problems that would give most other mathematicians pause. One of these is the Jacobian Conjecture. How did you get into this problem?

S.F. - When I went to Stanford as a postdoc, I realized that linear algebra and matrix theory were not what most people were interested in (laughter). Indeed, soon after I arrived at Stanford, Paul Cohen asked me what was the most important theorem in my field and I mentioned the Perron–Frobenius Theorem. A few days later he told me that he proved it using a hint in Dunford–Schwartz [7]. So I looked around for other things to work on besides matrix theory. There was this young assistant professor in the department whose work in geometry seemed to be generating a lot of excitement and I asked him what were the important open problems in geometry. He pointed me to the Jacobian Conjecture. This assistant professor is Shing-Tung Yau and the conjecture is of course still unresolved today. Working on it was both a blessing and a curse. I invested a lot of time but I published only two papers on the topic, despite spending more effort on it than on any of my other major research interests. One good thing that came of it was my paper with Jack Milnor on plane polynomial automorphisms [27], which became my most frequently cited paper.

L.-H.L. - Yitang Zhang, who famously made a breakthrough in the twin prime conjecture, also spent a significant amount of energy and time on the Jacobian Conjecture when he was a Ph.D. student and got little out of it. In fact, even the case of two variables is open. Do you have any advice for young mathematicians who might like to cut their teeth on this problem?

S.F. - Stay away from the Jacobian Conjecture (laughter).

L.-H.L. - So what should they work on?

S.F. - They should work on one famous unsolved problem once in their career, mainly to learn something from this experience. But remember to move on to other, less ambitious problems.

L.-H.L. - Is the Jacobian Conjecture the first big open problem that you have worked on?

S.F. - No. That would be the Bieberbach Conjecture, which was the subject of my master's thesis [17]. I later wrote two joint papers [32, 33] on this conjecture with Max Schiffer in 1977. The conjecture was eventually proved by Louis de Branges [6] in 1979.

L.-H.L. - You proved the Salmon Conjecture [19] a few years ago, a problem in algebraic geometry [1] that baffled many algebraic geometers. Can you say why you succeeded when all these professionals did not?

S.F. - I would attribute this to my familiarity with linear algebra. I combined techniques in linear algebra with some very basic knowledge of algebraic geometry. In fact, for this particular problem, knowledge of advanced algebraic geometry is optional but knowledge of advanced linear algebra is not. Linear algebra contains some of the most powerful mathematical techniques ever discovered. In my view it is much more applicable than algebraic geometry.

> "Linear algebra contains some of the most powerful mathematical techniques ever discovered."

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L.-H.L. - Another notoriously difficult problem that you resolved is the simultaneous similarity of matrices [22, 21]. The Salmon Conjecture is about a special case – four 4×4 matrices – but requires that you go deeper than simultaneous similarity. So did you find your prior work in simultaneous similarity helpful?

S.F. - They required quite different techniques. I read about the simultaneous similarity problem in a paper by Gelfand and Ponomarev [36] and realized that one needs to introduce a rational invariant function, which gives the complete solution to the problem of simultaneous similarity for a pair of 2×2 matrices. I figured out that one can then generalize this to k $n \times n$ matrices using stratification theory. In fact, after my paper [22, 21] appeared, I received a letter from David Mumford saying that he never agreed with Gabriel's division of classification problems into 'tame' and 'wild'. Gelfand and Ponomarev show that the classification of a pair of $n \times n$ commuting nilpotent matrices is 'wild' but my work shows that it is nonetheless very tractable.

L.-H.L. - How did you find out about the Salmon Conjecture?

S.F. - Bernd Sturmfels posed the problem to me while we were traveling together on a bus in Berlin. I was then a visiting professor in the Berlin Mathematical School hosted by Volker Mehrmann and Bernd was also on sabbatical in Berlin.

Shmuel Friedland with his Salmon Prize

L.-H.L. - Did you enjoy your Salmon Prize?

S.F. - Very much. It was two small delicious pieces of smoked wild Alaskan salmon, caught by Elizabeth Allman and smoked by her husband John Rhodes. It tastes much better than anything you can buy from the shops. I wish there were more (laughter). I should mention that my result in [19] was later improved in a joint paper with Elizabeth Gross [24]; maybe they would send us another piece (laughter).

L.-H.L. - You also solved Peter Lax's eigenvalue crossing problem. Can you tell us about that?

S.F. - Lax gave a lecture in Wisconsin describing his problem and I was in the audience. The von Neumann–Wigner non-crossing rule states that in a generic two-dimensional subspace of $\mathsf{S}^2(\mathbb{R}^n)$, the vector space of $n \times n$ symmetric real matrices, every nonzero matrix has only simple eigenvalues. In other words, the variety of real symmetric matrices with multiple eigenvalues has codimension three. What Lax showed in his lecture is that if $n \equiv 2 \pmod 4$, then every three-dimensional subspace contains a nonzero matrix with a multiple eigenvalue, showing the converse of the von Neumann–Wigner non-crossing rule. What I showed, together with Joel Robbin and John Sylvester [31], is that the problem of finding the dimension of the minimal subspace in $\mathsf{S}^2(\mathbb{R}^n)$ such that there exists a nonzero matrix with a double eigenvalue boils down to the problem of counting the number of linearly independent vector fields on the sphere, which was of course a well-known result of the topologist Frank Adams. I have to add that my interest in the von Neumann–Wigner rule predated Lax's talk – I did related work a few years earlier with Barry Simon [34] and so I already knew the topic well.

L.-H.L. - This reminds me of your work [29] with Giorgio Ottaviani using Chern classes to determine the number of singular values of a generic tensor. How important are topological methods in linear algebra?

S.F. - I think topological techniques are very important for nonlinear problems like eigenvalue or singular value problems. Incidentally, not long ago, Doron Zeilberger wrote to tell me that he submitted a special case of our result to Neil Sloane. The number of singular values of a generic $n \times n \times n$ tensor is now in the *Online Encyclopedia of Integer Sequences*.

L.-H.L. - While we are still on the topic of eigenvalues, you are of one of the pioneers of inverse eigenvalue problems. What are your proudest contributions to this area?

S.F. - I introduced algebraic topological tools like degree theory and algebraic geometric tools like effective versions of Bezout's theorem to inverse eigenvalue problems [10]. Henry Landau's proof [38] of the inverse Toeplitz eigenvalue problem was based on my ideas of using degree theory [15] but he went far beyond what I did. I also started the systematic study of the inverse eigenvalue problem for nonnegative matrices [18]. A high point of this problem was the work of Mike Boyle and David Handelman [5], which resolves the problem modulo zeros in the spectrum. Their proof relies on symbolic dynamical systems, but Tom Laffey came up with an amazingly short proof purely in terms of matrix theory [37], although his result is slightly weaker than Boyle–Handelman's. All three of them won the Hans Schneider Prize for their contributions. On the practical side, I did some work with Jorge Nocedal and Mike Overton on algorithms for inverse eigenvalue problems [28].

L.-H.L. - Nonnegative matrices are indeed fascinating. The most famous problem in this area was probably the van der Waerden Conjecture, and you obtained some of the earliest significant results [11, 12]. What drew you to this conjecture?

S.F. - Henryk Minc introduced the van der Waerden Permanent Conjecture to me in 1969 when he was visiting the Technion, where I was obtaining my doctorate. I jumped on the conjecture right away [16]. In 1977, Minc showed me the announcement of T. Bang [4], who claimed to have proved the conjecture up to a small polynomial error, but confessed that he could not follow Bang's arguments. I thought what I did in [11] was to essentially connect the dots in Bang's arguments. But I later received a letter from Bang congratulating me on my "tour de force" (his words); so I suspect he did not completely follow his own arguments either (laughter). A year later, Egorichev [8] and Falikman [9] independently proved the van der Waerden Conjecture. Using the main tool of their proofs – Alexandrov's inequality – I proved [12] the Tverberg Conjecture, a generalization of the van der Waerden Conjecture.

L.-H.L. - Many of the biggest problems in or related to linear algebra and matrix theory have been resolved (Horn's Conjecture, Lax's Conjecture, BMV Conjecture, Kadison–Singer Conjecture) or are nearly resolved (Crouzeix's Conjecture) since the beginning of the new millennium. What else remains? And what are your recommendations for ambitious junior linear algebraists and matrix theorists?

S.F. - I would say the field is as exciting as ever, if not more so. I would mention three key words: *random*, *quantum*, *tensors*. Random matrices have already been extensively studied for many years. My own interest in the area came fairly recently. In Fall 2000, when I visited my *alma mater* Technion as a Lady Davis Professor, I gave a course on advanced topics in matrix theory; Ofer Zeitouni and his postdoc Brian Ryder were in the audience. Ofer is a world-famous probabilist and, among other things, an expert in random matrices. Thanks to him, we started collaborating on the concentration of permanent estimators [30]. I think the connection between probability and linear algebra – not just random matrix theory but also topics like randomized algorithms in numerical linear algebra – is an area with great potential and we will be seeing many more developments.

However my opinion is that by far the most exciting new area is multilinear algebra and tensors – with so many more questions than answers. Since you are the world-leading expert, this topic is best left to your interview, not mine (laughter). Instead I would say something about quantum information theory, which one may view as "finite-dimensional quantum mechanics," and therefore it naturally involves a lot of matrix theory. This has been my main interest for the past ten years or so. What I want to point out is that quantum information theory goes much further than just linear algebra; it also nicely connects with multilinear algebra and with probability theory. An example of this is my work with Gilad Gour and Vlad Gheorghiu [23], where we generalized Heisenberg's Uncertainty Principle to tensor products of probability measures using vector majorization.

— People and places —

L.-H.L. - Your first positions in the USA were at Stanford University and the Institute for Advanced Study in Princeton. Did you like your experience at these places?

S.F. - My time at Stanford was very fulfilling. I learned a lot. I learned algebraic topology by attending Hans Samelson's graduate class. I worked with Sam Karlin [26] and Max Schiffer [32, 33]. Sam introduced me to Gene Golub, and although I did not have much interaction with him then, we became good friends later in life. S. S. Chern invited me to speak in his seminar at Berkeley. His prominent student S.-T. Yau, who was an assistant professor at Stanford, told me about the Jacobian Conjecture, as I mentioned earlier. Even the students were great to work with; an example that comes to mind is Charlie Michelli.

Frankly, I got much less out of IAS than Stanford. I attended many lectures: by Griffiths, Langlands, Selberg, Weil, and many others. I saw the full breadth and depth of pure mathematics, but that was the extent of it. Nothing concrete came out of these lectures as far as my research work was concerned. I did not have much social interaction with the permanent members except this one time when Atle Selberg invited my wife and me to dinner at his home, which I think was only because he and I both came from Israel (laughter). The best thing about IAS was that it offered a lot of quiet time for my own research work without the distraction of teaching duties.

L.-H.L. - I suppose you still visit Stanford quite frequently given that your son is a professor in the medical school? How do you think the mathematics department has changed after all these years?

S.F. - The only faculty member I have been in contact with these last few years is Amir Dembo. The department has changed a lot. Looking at its faculty today, I can see that there are now many more applied mathematicians than there were 44 years ago.

L.-H.L. - After Stanford and IAS, you were appointed to the mathematics faculty at the Hebrew University of Jerusalem. Tell us more about those days.

S.F. - Joram Lindenstrauss hired me in 1975 and I worked my way up to a full professor in 1982. The mathematics department in Hebrew University is called the Institute of Mathematics. Back when I was there it was an amazing place. Everyone was excellent – and I really mean everyone. The students were exceptionally bright; the best I have seen anywhere. I wrote some papers [3, 2] with two Ph.D. students – Noga Alon and Gil Kalai. Peter Constantin was a teaching assistant for my ODE class. All of them are now world-famous mathematicians. Among the faculty, Robert Aumann later got a Nobel Prize in Economics; the dynamical systems group, helmed by Hillel Furstenberg and Benji Weiss, was especially strong. I went to many of their seminars and this paid off later when I started working in dynamical systems myself. Another big group was the one in geometry of Banach spaces, led by Joram Lindenstrauss himself, but also had other remarkable people like Lior Tzafriri. Joram was a terrific analyst and an uncompromising person. He introduced me to the Grothendieck inequality, but I was never interested in it until recently, when you explained its significance and its connection to 3-tensors to me.

L.-H.L. - Many academics originally from Israel yearn to return to the promised land, including people at places like Princeton (Elon Lindenstrauss) and Harvard (Joseph Bernstein, David Kazhdan), but you did the opposite. Why is that?

S.F. - It is the universal reason for most such relocations in academia – the two-body problem (laughter). My ex-wife went to Cornell University. So I visited Cornell for almost two years before permanently leaving Hebrew University and joining UIC. But my time at Cornell was well-spent; that was where I got into dynamical systems and ergodic theory.

L.-H.L. - Can you tell us more about that?

S.F. - I attended a series of seminars on Hénon maps by John Hubbard, which included occasional guest lectures by his former advisor Adrien Douady. I made the connection between Hénon maps and the Jacobian Conjecture and wrote a small report. On one occasion, John introduced me to Jack Milnor. I gave Jack a copy of my report and that led to my collaboration with him on the dynamics of polynomial automorphisms [27]. Jack is a fantastic mathematician in numerous ways – the one that impresses me most is his ability to make even the most complicated mathematics look simple.

Partly encouraged by this collaboration, I continued to work for a few years in dynamical systems and ergodic theory. Among other things, I generalized my results with Jack on entropy to holomorphic self-maps [14]. This in turn led to my work on multidimensional entropy [20] and for this work I should not neglect to acknowledge the help I got from Tim Gowers, who happened to be visiting IHES when I was there in 1994. That year at IHES might have been a low point in his career, as he gave me the impression of being somewhat disheartened. His magnificent talents and intellect were nonetheless obvious to me, and probably everyone else too, even at that time. The final piece of work that came out of this series of papers is [13], where I introduced a new kind of entropy that is now called "Friedland's entropy."

L.-H.L. - You said "among other things." Can you elaborate?

S.F. - Certainly. Dynamical systems occupied a substantial part of my mathematical life and I have worked on many aspects of the subject. For instance, one topic that is very far removed from my work on entropy is something I did with Sa'ar Hersonsky [25]: We combined techniques in normed algebras with techniques in dynamical systems to extend Martin's inequality for discrete nonelementary groups of Möbius transformations in higher dimensions [39] to discrete multiplicative subgroups in normed unital algebras. Aside from this, I have also worked on invariant measures, Lyapunov exponents, Hausdorff dimensions, strong hyperbolicity, monomer-dimer model, Furstenberg's 2-3 Conjecture. The last one is with Benji Weiss [35] and is my only joint work with the dynamical systems group in Hebrew University, written more than twenty years after I learned the subject from them.

L.-H.L. - Besides the people you already mentioned, were there others who played a pivotal role in your career?

S.F. - I think I owe a lot to the late Hans Schneider and, to a lesser extent, also to the late Olga Tausky-Todd. Hans recognized my talents early on in my career. When I was still an associate professor at the Hebrew University, he arranged a two-year visiting professorship for me at the University of Wisconsin–Madison. At that time, it was the world center for linear algebra and matrix theory. Those two years, 1978–80, were my best and most productive mathematical years. Olga was also very kind to me and invited me to visit Caltech several times. She inspired some of my works on the Motzkin–Tausky L-property for matrix pencils, i.e., $A + xB$ such that all eigenvalues are exactly of the form $\alpha + x\beta$ where α and β are eigenvalues of A and B. This led to my work with Nimrod Moiseyev on resonant states [40].

L.-H.L. - My last question is probably an inevitable one in such interviews: When did you realize that you wanted to become a mathematician?

S.F. - This happened when I went from my kibbutz to the Reali High School as a sophomore. I started to submit my solutions to problems in *Gilionot Matematica*, a high school mathematics periodical edited by Abraham Ginzburg. These submissions were graded and the scores published alongside the students' names in subsequent issues. It was completely voluntary but it fostered a competitive spirit among students from high schools all over Israel who partook in the activity, most of whom I had not met and knew only by name. There were only a small number of students who received higher total scores than me – one of them was the logician Saharon Shelah, who later became my colleague at the Hebrew University – and at that time I was quite encouraged by this.

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