Adrian Nachman Interview - Transcript

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Andrea	Adrian
What sparked your early interest in mathematics?	
	Well there's the spark and then there's the lasting flame. The, the spark is that um, some of, two of my best friends liked to meet after school and read uh, books about math, when we was, when we were in high school and they uh, asked me to join them and um, one of their grandfathers I think really loved working on elementary problems so he would egg us on and I was just so happy um, I was good at it, and so that might be the spark. I happened to be lucky that, you know, the mathematics a researcher does, has nothing to do with uh, the stuff you learn in high school, I was uh, consider myself very lucky that somehow the mathematics I do today, I love more than ever.
I understand you also considered becoming a conductor. How did you end up choosing math over music?	
	Um, that's true. Um, so for conducting for me was fascinating because you get to think about a piece of music, you get to figure out what's it's structure, what's behind it, uh, and then you somehow with one move of the finger you get a hundred people to express your thoughts about that piece of music. Um, why did I choose math? Again that could have been an accident. You know at McGill I talked to the, uh, the uh, best conducting professor. He invited me to his home, we had a chat. I thought I was going to do both. But then Arts & Science didn't think that this deserved credit, and so I stuck with math, partly thinking that "What if I become a conductor and there's no orchestra to conduct?". Whereas with my days in high school, uh, I knew that I could sit and do my mathematics on a piece of paper without an orchestra.
Did you ever consider becoming a composer?	
	Uh, no. Whatever tiny bit I composed was way too simplistic. People were, um, other people were a lot better at it than I am. Or I could imagine being.
And how did Charles Fefferman become your research advisor?	

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	Let's see, so for that, for that we have to go back a little bit to my undergraduate years at McGill, where, uh, Karl Hurtz, who was a great mathematician unfortunately passed away. I think he, he inspired my taste and so when it was time to apply for grad school, uh, and I was asking him how's this versus that school and what's they're ethos, he said "Just go to Princeton." And, um, so I liked Analysis and I was at Princeton and if you would hear a Charlie Fefferman lecture, um, people would walk out of there completely awed and inspired. So that when he accepted me as a student, I was late.
And how did his advising style influence your PhD research experience?	
	Oh completely, I mean um, uh..., I was uh, just had three years of undergradaute math from McGill, um, he had an amazing overview of the Field and so, um, on the first or second day we met, he made a list of problems he felt were important and those problems influenced the ones I chose and the ones I didn't choose at that time, influenced my path for a while. Um,
What was it like to be a student of Charles Fefferman?	
	Um, I consider it an immense priviledge um, mathematically, um, Charlie could see further than anyone and coupled with that he was a model human being. Um, uh, it was also scary, he had very high standards and you have to work very very hard.
How did you first get interested in the mathematics of medical imaging?	

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	Ah, that's another story, so um, I started being interested in um, so in Princeton I developed a great love for harmonic analysis and I, uh after Princeton, um, I, I thought that, uh, by talking to people it felt to me like that um, scattering and inverse scattering um, problems were a version of non-linear harmonic analysis. So they would sort of lead you on a practical problem, lead you by the nose into developing some of the beautiful um, discoveries in harmonic analysis and push them towards new non-linear problems. Um, and then I gave a talk, and then later at Rochester, I gave a talk about this and um, uh, my friend Robert Wag, whom I didn't know at the time was in the audience and he was an engineer, a superb engineer working on medical ultrasound and he had various ideas on how to improve medical ultrasound, how to use it in reliable ways to detect breast cancer, um, but the measurements he wer making gave data that was too complicated to unscramble and he thought after my lecture that I was just the guy to talk to. And so I joined a team, um, and what I liked about medical imaging as opposed to inverse scattering from Physics, is that um, you have more, um, choice in the kinds of data that you can collect. So I can talk to my engineering friend and you know and I would say you know if we could put make measurements of this kind and that kind that would, uh, dovetail well with what can be recovered, um, whereas in physics or let's say in oil exploration, you have much less of a choice, people go they go do make measurements out in the lab or in the field they bring you the data and now do what you can with it. So that's one aspect of medical imaging. Um, again that sparked it. Since then, I've learned a lot about the field, um, and I think it's fantastic because on the one hand, um, it can uh, somehow demands and makes use of some very recent advanced mathematics uh, things like abstract things you might not think of like differential geometry uh, which are being used in several areas of medical imaging.

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	Um, and on the other hand, you're helping people and there's a whole spectrum of you know you get to talk to engineers who build things, you get to talk to doctors, to experimenters, so um, I like this aspect of one day talking to uh, colleagues in pure mathematics, who uh, think of the differential geometry problems I'm interested in because of medical imaging and the next day uh, talking to someone who was at the Sunnybrook Hospital making their MRI experiment and hearing what happened to their circuits and what went wrong and how we can fix that and so forth. So, um, this variety and spectrum of things to think about is quite attractive.
And what problems are you working on right now?	
	Uh, right now. Well, I'm interested in um, you might say well we have medical imaging we have CT machines, we have MRI machines, we have ultrasound machines, why do we need new machines? Well each of them, uh, gives you different information about, uh, about your body and we function electrochemically. And so I'm interested in imaging the electric properties of tissue, which cannot be obtained by the method I just mentioned. Um, and again on the one hand, that has lead to uh, problems some people describe as pure math. I don't like so much to, delineate, as I said I spent my, uh, my whole life trying to erase these boundaries. Right now for instance I'm, uh, I'm organizing two huge programs. One for MITACS, a focus period on the mathematics of medical imaging and one here at the Fields Institute on inverse problems and imaging. Um, and so far my biggest joy has been that it's been a surprise to everyone. It's been a surprise to mathematicians, how many interesting, difficult problems there are. The deep mathematics that some of their colleagues that they didn't know about are doing in the medical imaging. It's been a surprise to doctors um, how much mathematics can help in some of the things their thinking about. Um, I've had people like engineering colleagues in biomed come to me and say you know, I go to big conferences and you hear a lot about incremental progress. I came to this conference and 80% of the talks were amazing new ideas. Um, interesting and exciting that I've never thought about. Uh, that can completely change how an approach to some of these problems. Um, now I forget what the question was.

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It was, what problems are you working on now?	
	So, uh, right, so I'm interested in uh, both the mathematical and the engineering aspects of, uh how to find the electric properties of tissue.
And your research relates a lot to industry, relates a lot to practical medicine, would you say?	
	Okay, well, uh, yes, uh... so, okay, there's, um, there's a lot of steps between the mathematics and the industrial. You have to um, first you figure out how something works, and uh whether it's possible to do, then you find the method to do it. Uh, then you might talk to someone who's uh, good at writing computer code on how to do it on a computer then you talk to experimentalists to try it out, then you talk to doctors uh, to tell you what you can see there and what, how to modify the method to make it useful to them and so the lead time to actually building something new in a company is rather long in this particular field. Um, so uh, it's very practical. We're interested in new ways, uh, to image the body, um, but they are completely and so the typical industrial company is working more on incremental progress on existing methods, rather than, so think of this as the new generation of, uh, imaging methods.
And, do you find that a lot of people understand, um, how many steps there are to get to that point where we can go to a hospital and we can be imaged?	
	Um, it depends who you ask. I don't think, uh, I don't think the public understands, I don't think my mathematical colleagues understand, uh, I think that the people who build those machines understand that very well and I think even people in engineering who are somehow in between the mathematics and the industrial also understand that there are many steps. Um, so everyone thinks that their end of it is the more important. But I feel that, um, all these steps are important and need to be put together to get to the product.

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When you're choosing your problems, what you're going to work on, do you think about those steps when you choose the problems?	
	I think about those steps, um, I am open to hearing problems from any source whatsoever. Uh, the uh. One of my most recent discoveries came on a problem that was connected with discussing with uh, my colleague in engineering Mike Choi who had a great idea on how to use MRI, um, in a completely new way to image currents. Um, but I wouldn't say that it's sort of industrially directed. I think that if the problem. I, um. You know, some people are interested in making things, I'm more interested in understanding things. My prejudice is that, understanding fundamental and difficult questions, then helps in many industries. So for instance, um, a major recent discovery in math is um, this um, theory of compressed sensing. Which in it's current form is about seven years old. We'll have some of this in the program here at Fields. Um, it came out of mathematicians trying to understand um, something observed in MRI that, if one is clever about there's a way to obtain images that are completely accurate with a lot less measurements than a lot of the classical sampling theory that either taught in school, would predict. So now subsampling is a huge problem. If you want to observe the Earth from space, you have fewer sensors than you'd like. Um, if you want to transmit over a phoneline, you have fewer frequencies than you'd like if you want a lot of bandwidth. Um, to fit a lot of things in a given bandwidth. Um, so here's an example where. Uh. Figuring out something then spawned a theory which is now literally in five years, thousands of papers. Completely in different areas, geophysics, uh, astronomy, uh, imaging, uh, building chips, uh, all of which use compressed sensing. Uh, colleagues in engineering are doing it. For instance, here's a very simple question. Um, you have um, hotspots for wireless. So I turn on my laptop and I can see uh what hotspots I'm detecting and uh, I know where the hotspots are in Bahen. Alright, where am I, based on that information. I have a colleague who used compressed sensing to uh, to find a very good

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	... algorithm to do that. Um, one would have never thought of that, had one tried to directly solve that problem. As to for instance solving the one that came out of MRI. So, I think that if I were to do only industrially directed questions, I would do lesser work.
How does the Chicago school of analysis, inform the work that you do now?	
	Um, well. Um, I'm very proud to be a descendent of the Chicago School of Analysis. Um, when Charlie Fefferman and his advisor Eli Stein coming to Princeton I guess some of the best part of that school became the Princeton School of Analysis. Um, it so happens, um, and again I learned this after the fact. That, uh, some of my work on inverse scattering, could be um, the methods developed there could be used to uh, attack a problem that had been um, suggested by Calderon. Now Calderon, who was one of the towering figures of the Chicago School, had suggested that in terms of geophysics. Uh, he had no idea that it would be taken up by the medical imaging community and it is now one of the seminal problems around which the mathematics of medical imaging has been developed. Um, separately from that I happened to use some other methods that Calderon developed in other papers. Um, and so I feel I'm a lucky and humble descendent of the Chicago School.
How would you describe the experience of discovery?	
	It's of course a great joy. Um, but at least in my exp.. So... A great idea gives you great joy, I mean it gets me all excited I have to, I can't sit down, I have to walk around, for half an hour and then I sit down and I realize it's wrong and then maybe I have another idea. And once in a while, these work out and that's a great joy. But finding that they have, might take a year's worth of work, and so it's incremental everyday. You push a little further, you figure out what is and what isn't true. Um, and by the time it's all put together, you've been with this for so long that I don't know if you can describe the experience of discovery, looking back, you're of course tremendously excited that you were able to find some structure that somehow nobody else could see, um, and that happens to be interesting and useful and beautiful.