Spyridon Alexakis Interview - Transcript

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Andrea	Sypros
What sparked your interest in mathematics?	
	I liked mathematics in school. Uh, and I choose to become a mathematician because the primary subject that I liked very much was Euclidean Geometry of the plane. Uh, and the, what gave me confidence to give a try at a career in mathematics was the International Math Olympiads. Where I had some success, I got on the national team, and I thought that it might be worth giving it a shot. I liked it very much and I felt I might succeed in a research career.
And how did that inform your early research decisions?	
	Not very much because the subject is more than 2,000 years old. Um, it maybe influenced my aesthetical decisions, in uh what I, the subject I wanted to pick, but I know I made my research decisions much later. In grad school I'd say.
And how did you make those research decisions?	
	So I, I went to grad school in Princeton and uh, what was true then was that they, you had to decide quite early on what you want to focus on. A few introductory classes. Many many advanced classes that weren't accessible to someone with knowledge level. So, I felt I was stronger in analysis just because of my training as an undergraduate and I decided to make that choice because I felt I was stronger in that field. I should also add I took one of the few introductory courses was in harmonic analysis that I enjoyed very much and so I decided to, to work in analysis. Yeah.
And then how did Charles Fefferman become your research advisor?	

Andrea	Sypros
	Uh, so, he was a well, I decided to work in Harmonic Analysis and he was a great expert in the subject. Uh, he was also a very approachable and friendly person and that helped a lot. Um, I should add that of course I didn't end up working in Harmonic Analysis and that became quite obvious from after my first meeting with him. But I could see, he was very broad in his interests, um, and very encouraging to try out something new. So, so, so I yeah, I initially was tilting towards analysis, he was a very very strong analyst and a very nice person and then when I did meet with him he gave me a list of problems that one could consider and I liked many many of them. So I decided to work with him.
And you have research threads in general relativity and conformal geometry which aren't really the bread and butter subjects of the Chicago School. How does that speak to the breadth of Charles Fefferman's training and mathematics in general?	
	Right, so uh, of course, my interest in relativity came a little bit later after my work with him. Conformal geometry is what I started studying with Fefferman. It really speaks to the incredible breadth of his interests and the breadth of his work. Um, and it also speaks to I think to his courage that he's so unafraid to try a new field where he doesn't, where he's not an expert and I'd like to, I'd like to inhereit some of those characteristics. Um, yeah. Relativity I moved to later, I could, somehow also by chance, because some work being done in relativity at the time when I completed my work in conformal geometry and it was almost by chance that Unescu and Klineman who later became my collaborators were working on a question and I asked myyself a question related to their work which actually turned out could be applied to what they were working. And so, we teamed up. Um, I'd say that, I made this transition in part inspired by Fefferman's courage to move in new directions that he just felt were interesting and important.
And how do you choose your research problems now?	

Andrea	Sypros
	Uh, I choose them with delay, because it always takes me much longer to uh, to complete the research projects that I give myself. Um, I, well I try to, uh, I try to pick problems where I have a reasonable chance of success and are somewhat adjacent to things I know. But I really want to do things that I feel are imporant and interesting and let me use the word deep also. So, um, yeah, so so so yeah, I'd say it that way.
And how do you define an important problem?	
	Yes, so that's difficult to tell. Um, I'm partly interested in things that are um, that that uh, um, that are relevant even slightly outside mathematics. Most notably to physics for example. Um, and I feel something, something that that um, question that deserves to be asked and an obvious question, a question that pertains to other subjects um, that even within mathematics or even outside mathematics that should be addressed, should be answered. I try to answer such questions.
Does past research ever inform research decisions that you make?	
	Well certainly, I need to feel that I have some chance of success. But um, I um, I, I try not to be purely motivated by what I think I could do but by what I think should be done. So I, I try not to be too stuck to the past. And then maybe I should also add that, maybe along with many mathematicians at my age and my stage in their career um, I had spent a long long time working on one project that ended up being very long. Um, and I was eager to move in another direction, to learn something new, to master new material. So I had done that, the fact that I was at that stage in my career informed the choices that I made and the choice of problems that I picked.
And, what about industry? Does industry ever inform the research decisions that you make?	

Andrea	Sypros
	Up to now, not directly, okay. I do um, but like I said, I want my work to have an impact, and I want, possibly outside mathematics. I want what I do to have interest even outside of mathematics and I don't mind if it's going to be very many years down the road. Um, yeah so for example, my work in analysis and partial differential equations, these things are of interest certainly in the rest of science and I believe in industry too. Um, so, so so um, so indirectly, I indirectly it does influence what I choose to work on. At this stage it doesn't influence it directly.
Would you say that mathematical ideas when they're really solid are ubiquitous to many fields even outside mathematics?	
	It is too difficult. It's a very difficult question to answer. Um, that's a very difficult question to answer. I mean, I think so but we're trained as mathematicians to produce evidence and to prove what we say. It would be very difficult to prove that statement for you. Uh, um, uh, But yes, I think, I mean, what one often sees is that ideas that are developed in mathematics early on are picked up and applied by engineers decades later. So it's very difficult to say what, what is being done today can influence uh, can influence other subjects. Engineering or industry, um, but uh, but maybe often it's not what one expects. It's things that look very theoretical and abstract now, tend to be very applicable decades down the road.
Would you say there's a way to predict that?	
	If there is one, I don't know it. But, um, one way to predict I would think is if you, if a given subject become successful in mathematics. If one works on subjects all by himself and he can't attract other mathematicians into it, it's very difficult to see how it attracts people outside, outside the world of mathematics. Um, uh but uh, but broadly speaking I think it's certainly too difficult for me to predict now. And I think it would be very difficult for, for most people it would be very difficult to predict. What can be applied further down the road.

Andrea	Sypros
And when you view mathematical research areas, do you see them as being distinct from each other, or do you see a blur of lines between areas?	
	Okay, I may be biased here but I think the, the uh, mathematical research is immense and I, certainly for someone at my stage it is very difficult to really grasp what is happening a couple of fields across, a couple of fields adjacent to mine. Um, I think mathematics has, although it's so broad and if one goes to a seminar outside one's broad area of specialization, you're likely not able to undertand anything after five minutes. But uh, um, but one thing that mathematics does have is a common language, common understanding of what we're doing. We produce proofs. We produce evidence. That's not how you feel sometimes in other fields, there's not that common language that common thread is not present. So, yeah.
And what advice would you give for young students starting out right now in choosing an area to study?	
	People who have already chosen to become mathematicians?
Yeah, young mathematicians.	
	Primarily, do something that you enjoy and like because with that you're more likely to be successful there. Um, and you should, you should convince yourself of the merit of what it is you're doing. That's very difficult to do in pure mathematics. I imagine a more applicable field that's fairly obvious. Well, yeah I mean, choices can be informed by by, by other factors which are not present in pure mathematics. Um, but, you should somehow feel that, that the subject that you pick is interesting to you and has some importance within the subject, within mathematics or even outside. You should feel that. It is going to give you strength. And um, yeah, and um, yeah that's it. (laughs)