Numberphile Podcast Transcript
Episode: A Proof in the Drawer - with David Eisenbud
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David Eisenbud's entertaining stories about mathematics are a fascinating glimpse into how math works - how it really works.

MSRI - where David is director

David Eisenbud's MSRI page with some links to publications and other material

Google Scholar citations

Videos with David on Numberphile

The 17-gon video

David's math genealogy

With thanks to

Meyer Sound

Brady Haran [BH]: Are you worried that I'm gonna give you too easy a time, that's the question? I don't you're worried about that, the way I always embarrass you in my public talks. [chuckles]

David Eisenbud [DE]: I am not worried.

BH: [laughs]

DE: I don't mind the embarrassment either.

[gentle piano music]

BH: Today's guest is a true Numberphile VIP, and let me explain why. David Eisenbud is a well respected mathematician. He's got an impressive array of papers and books under his belt. These days he's at the helm of a place called MSRI, that's the Mathematical Sciences Research Institute. It's in Berkeley, California.

[music continues]

BH: That name my sound familiar, because MSRI has been a major supporter of Numberphile for many years. I spend several weeks each year at the Institute meeting and interviewing top mathematicians who visit there. It's become bit of a home away from home for me. And over that time David's become a person I'd consider most responsible for like my mathematical education. I don't mean teaching me calculus or how matrices work, although he has tried that from time to time too. What I mean is that David's taught me how the world of professional mathematics works. About universities, about how like research gets done, how papers get published.

[music continues]

BH: Like any field of endeavor, mathematics is full of gossip and politics, personalities. It's a very human world and there are few people who know it better and seem to be better connected within it than David Eisenbud. Today's interview with him is very typical of the sorts of conversations we have and I hope it gives you some insights into not just David's life but how mathematics works. How it really works.

[music fades out slowly]

BH: If I had met you… as… a boy…

DE: Hmm.

BH: … would I have thought, ah yeah this guy, he's gonna be a mathematician for sure.

DE: You would certainly have thought I was a nerd. I was one of the kids in my science class who liked to present articles from the Scientific American in fact. Which was a weekly thing, but I was one of the few who really wanted to do it. But I began announcing to my friends and family that I was gonna be a mathematician when I was twelve. And I don't remember how that came about.

BH: Did you know what a mathematician was then? 'Cause a lot of people I've spoken too would say when they were young I didn't know mathematics or mathematician was like a job. But you'd figured this out?

DE: Well my father was theoretical physicist, a mathematical physicist. Student of Eugene Wigner. So they were all around me and I think actually mathematics, why mathematics? I think it was sort of the right distance from physics at the time though later on I thought I would be a joint math-physics major in college and I got in over my head in physics and finally fled. I didn't like the labs. And then I took a mathematical physics course, a graduate course, which I was sure I was gonna fail, but by good luck I did well. Pure good luck. There were all kinds of special functions and equations I had no notion about, but I happened to read about vortices the night before the exam and vortices have the funny property that the more energy is in them the more slowly they propagate. And I knew that. And I got an A as a result [chuckles].

BH: So this twelve year old, David…

DE: Yeah.

BH: What were you imagining? Like what was the fantasy? Was it sitting in a room with a notepad and doing famous proofs or was it being in front of class

and teaching? What was the dream?

DE: Oddly enough I don't think there was a concrete dream associated with it. I wanted to be like my father and he sat in the room a lot and with a pad and there were no computers at that time. But he had taught me quite early and that was a very exciting thing for me when I was in, I dunno, 7th grade I think, he taught me how to solve simultaneous linear equations. And that was quite exciting and then later on he taught me how to prove elementary geometric things like Euclid propositions using vectors. And I won some science fair prize for that. That early bonding with him was enormously central I think in my choice.

BH: But you were good at it too? You were like top of the class getting the As and…?

DE: By that time I was good at it. When I was in 4th grade I was in the very last group in arithmetic in the class. And for a good reason actually. And I remember I somehow got into the next to the last group and the teacher complemented me on my great achievement [chuckles] but I was adding my own way. I was adding from left to right rather than from right to left. Of course I was inaccurate and approximate and that was not considered okay.

BH: Did your father foster this? Was he like, yeah be a mathematician or was he like, no why don't you consider other options? 'Cause he knew that perils of academic life and was trying to steer the son away, or…?

DE: I don't remember any comment from him one way or the other. I think he was happy that I took this path. But I don't remember any concrete conversation. My mother said she always hoped that he would push me towards science and that therefore I would be a writer, which was her dream. She was a psychoanalyst, also very important in my early life.

BH: Why did you not become a writer?

DE: I don't know, really. I wrote some short stories early on. I write a lot now in my current job and you know I've written a lot of books and… a lot of papers by now. So I consider myself a writer in a way and I think a lot about style in writing. But I never aspired to write fiction, although I read fiction a lot. I like fiction a lot.

[violin music plays]

DE: So my parents moved a lot in the early days. My father had been blacklisted. He had worked for Brookhaven National Labs in the late forties, around the time I was born. I was born '47. And he was working there but my mother's sister had been a genuine communist. And he was called in one day, into his boss's office and asked to name her friends. And he refused and was immediately fired. And then he couldn't get a academic job for ten years.

BH: Huh.

DE: This was in '47.

BH: Yeah.

DE: They wandered a bit and I grew up in that early ten years in Swarthmore, Pennsylvania mostly. He found a job at a private research institute, it was located on the Swarthmore campus but not part of Swarthmore.

BH: Right.

DE: A little bit like MSRI on Berkeley actually.

BH: Right.

DE: And it was from the roof of that institute that he… he took me up to the roof to see the first Sputnik for example, in ’57.

BH: Wow.

DE: That was very exciting.

BH: Oh yeah.

DE: All his colleagues of course came too.

BH: Did those difficulties he had during those years, you know, in this sort of wilderness in the difficulties that was caused…

DE: Yeah?

BH: …did that affect you in anyway? Or were you like a bit young and a bit blind?

DE: I was blind. I didn’t really know about it til after.

BH: Okay.

DE: Anyway the in ’57 he began to get, you know, academic offers. Actually one at Brandeis which he turned down in order to go to Stony Brook. And he was one of the founders of the Stony Brook department which is now very illustrious.

BH: Okay.

DE: He did a lot for that. He recruited Dirac for example to come to that department for a year as a visitor.

BH: Wow, alright.

DE: My parents knew Dirac, and his wife who was Eugene Wigner's sister… pretty well.

BH: Okay. So let's go to you. What happens to you? Are you excelling at high school?

DE: So I skipped a couple of grades in high school. I was sick of high school in my junior year and in April of my junior year I decided I would like to go to college the next year. Instead of going on to be a senior in high school. And so it was too late to apply at much of anywhere. But somehow my parents knew people at Harvard so they were willing to look at me and they decided no.

BH: [laughs]

DE: [chuckles] Which was I think extremely lucky for me actually. Also at Chicago, somehow I got an interview and they did take me. They had a habit of taking early young kids, so I entered there when I was sixteen. And was already mostly paying attention to mathematics and music, not so much anything else.

BH: Sixteen at college? Would you be like the baby of any group you were in? Was it hard being a sixteen year old in college then or…?

DE: I was young. Most kids enter at eighteen. I wasn't the youngest person in the room.

BH: What was university like, were you good like were you again like top of the class and excelling? Were you just a sort of a middle of a road or…?

DE: I was pretty good. Chicago had a very rigid system, lots of requirements

and so I placed out of them and so took math classes. And pretty soon graduate classes.

BH: Placed out of them? I don't know what that means.

DE: Oh so, you could take an exam instead of taking the course and took the exam and I was good enough and so I didn't have to take the course.

BH: Oh, alright.

DE: Got out of the requirement.

BH: Is that something we can still do today, that sounds like… that would make like…

DE: I'm sure Chicago still does that today.

BH: Okay.

DE: Lots of places do it. All universities have placement exams, usually to place people into the lower level where they really belong instead of the higher lever where they want to go but it works both ways.

BH: And was this time at college was this the formative time? Was this were you started seeing specializations and strengths and did a clear path emerge then is that when it looked…?

DE: So certainly it was very clear to me that I was doing mathematics and soon it was clear that I was not doing physics. So that was clarifying. There were no other questions in my life actually.

BH: Okay.

DE: I thought… just before I went to college I thought I might be a professional musician and that was a lucky thing I didn't choose that. [chuckles]

BH: Well, you don't know you could have been… world famous!

DE: Oh I dunno. First of all I don't think I had the talent. I was a flutist. On second, the life is miserable compared to… at least compared to a happy academic. Academic lives ccan be difficult too.

BH: Was there a discussion to have with your father when you had to confront him and say, Dad I'm not gonna be a physicist?

DE: Again I don't remember any such discussion. I certain told him… I must have told him my relief of passing this course. But yeah that I remember my terror to this day very clearly.

BH: What? When thinking you were gonna fail and have to tell him you'd failed it?

DE: Yeah, basically, right? The two memories of teachers I have from that era both of whom influence me a lot and they couldn't have been more different. One was Antoni Zygmund who was a very famous analyst. Built like a plumber somehow, short and very compact and solid looking… and a Pole. I remember he used to stand in the ramrod stiff at the edge of the common room when it was his seminar time and clap twice in the air and turn his back and walk out and all the analysts would troop along after him.

BH: Right?

DE: And he was just a lucid lecturer. Incredibly beautiful prepared lectures. This was Lebesgue Interval that I took from him. It was a graduate course that I

took. But just a beautiful beautiful talk. And the other course was taught by Otto Kegel who was the most disorganized person. He was teaching an elementary kind of algebra class, in second course in linear algebra. But he had decided to do in a Bourbaki way, very abstract way, for which I wasn't to ready, nor was the class I think. Also this is a course about vectors. Vector begins with a V and so typically in that class every letter is a V or a W and being German and having just come to the United States, he couldn't really remember which was Fau and which was Vhey and which was V and which was W so they were interchanged freely…

BH: [laughs]

DE: In his speech.

BH: Okay.

DE: And his handwriting was also more or less inscrutable so there was completely chaos and confusion but somehow his enthusiasm for the subject and love of it was just infectious. So at the end of my… I also finished college in three years and at the end of my time my parents said well you could go to Europe if you want for the summer and I chose to go to Frankfurt where Kegel was an assistant and work with him on mathematics that summer. And that was where I wrote my first paper which was wildly exciting.

BH: What was your first paper called?

DE: Oh it's real juvenilia. It was on order… order morphisms of infinite ordered sets.

BH: Ah yeah. Oh I think I've read that one. [chuckles]

DE: [laughs] Someone did read it.

BH: [laughs]

DE: But not many. [chuckles]

BH: So you went to work with this man just because…

DE: Because I was so excited by his presence somehow.

BH: Right.

DE: And it was a good choice too. He gave me this problem, he helped me into it. And let me run. I was a terrible nuisance. I mean I couldn't stop talking about this subject and so I was, you know, really not very socialized at all.

BH: Was this a formal thing? Like were you working thing or have some place…

DE: No, no.

BH: …like it sounds to me like you were on holiday and you just turn up or…?

DE: More or less. I wrote to him of course to say I would like to come. But it was an interesting summer. Reinhold Baer who was a very famous man. So Kegel was Assistent [pronounced as the German title] by Baer at that time. That's a whole class of people we don't have here. But the assistants, just an assistant, had positions and they were more or less the servants of the professor and at that time still the system was in the old style and so some of Baer's assistants took his wife shopping for example.

BH: Right. [laughs]

DE: Which would no longer be tolerated but was then.

BH: Right.

DE: Quite normal.

BH: And also Baer took his whole mathematical family, which include me as the very junior member to this conference center called Oberwulfach. At that time the senior professor in Germany, you know, you could be a proper professor which was pretty good already or you could be a professor with week at Oberwulf, so he had sort of right to bring his mathematical family.

DE: Okay.

BH: And it was wonderful, and, you know, this place in the Black Forest. I've gone back… many many times since to conferences and I just love the place.

[gentle piano music]

DE: Well I wanna talk a little bit about my graduate school experience, it has an unusual twist I think. I started working under somebody I don't need to name and I've discovered that I really didn't like him personally. Just I ran away. So I took the advice of one of my fellow students which was forget about the subject just look around whom you like. And work with that person, you'll like the subject soon enough. So that's what I did.

BH: Is that good advice?

DE: For some people it's very good advice I think. [pause] Yes. I tell students that not exactly that now, I tell them to look around and find the group of students who have seen most integrated and happy together and work in that

area. If they like the area at least.

BH: Okay.

DE: Saunders Mac Lane was then my target. And he was a wonderful guy. I was one of his last students of many many. He was a very forthright sort of honest direct person. He was a wonderful lecture. I have to tell you my first meeting with him I was a, you know, a precocious undergraduate so I came to class early. So at the beginning of the semester there I am, sitting in the front row waiting for people to show up. And I'm working on something, reading something, I'm, I mean, completely oblivious to the environment and so finally the professor walks in and it's Mac Lane, I didn't know him at the time at all, I didn't know that he wasn't teaching that course, and I was in the wrong room. So… Mac Lane started talking about category theory, which was his passion. I… didn't know a thing about category theory and I was not prepared for the class. You know, my eyes glazed over in the first minute or two but I was sitting there as if… paying attention.

BH: Why did you not leave?

DE: Why did I not leave? I don't know. Embarrassment. I was sitting in the front row. Famous professor, all these people around. Maybe I was interested in being a spectator too. Anyway at a certain point Mac Lane, who was very proud of being aware of his audience and what they were understanding, I looked up and there he was pointing at me and said you, you didn't believe that proof did you? So of course I was paralyzed at that point, I didn't say anything.

BH: Yeah. [chuckles]

DE: And so he went back to the blackboard and proved it again to what he thought was my satisfaction. [chuckles]

BH: Right. [laughs]

DE: And then the lecture could proceed. [laughs]

BH: Okay. [laughs]

DE: But years later at his seventy-fifth birthday party, I reenacted this scene with him. With him in the audience.

BH: Right.

DE: And he denied everything. [laughs]

BH: Okay. [laughs] I'm sure it's burned into your memory a lot more than his.

DE: [laughs] It sure was. I liked him a lot. I listened to all his classes, I really admired him. And he was a big shot around the department too. So I decided I would work with him.

BH: He had to decide that too, didn't he?

DE: He had to agree to it, certainly.

BH: You picked him but why did he pick you?

DE: I guess he… I had a reputation of being an interesting student somehow. I don't know the answer to that question. But we liked each other, we became quite good friends. So I thought I would work in category theory.

BH: Which was his…

DE: Which was his thing.

BH: Yeah.

DE: And it didn't really work. So I kept thinking of different problems. I never found a problem in category theory that I really liked. And then he went away for a quarter and so I was drifting around and I knew lots of people in the department and there was a postdoc, Chris Robson, who was there working with a different professor, Israel Herstein, on a different subject, non-commutative rings. But I knew something about that subject, not very much, and I was interested so I began talking to him and we worked together and proved a few nice theorems. Then Mac Lane was returning and I remember sitting in a cafeteria opposite of a group of students, a bunch of us went out to dinner, chatting about this and the person opposite me said something which indicated that he thought my thesis was about non-commutative rings. And a light went off in my head, that I had written a thesis without worrying.

BH: Right.

DE: And without a thesis neurosis. And I was so pleased by that thought I went back to Mac Lane and said you know are you willing to have this be my thesis? He talked to Robson to make sure that Robson thought I had actually done something and he agreed and very generously read the document and corrected lots of things. His system was that he would begin at the beginning and read until he had marked twenty-five places where something needed to be improved and here he would give it back to me.

BH: Right. [chuckles]

DE: Then he would start again when I rehanded him a fixed copy.

BH: A linear editting.

DE: And this went through three times.

BH: Okay.

DE: And finally he approved and that was my thesis.

BH: I mean this seems like some kind of academic infidelity that he went away…

DE: [laughs]

BH: …and you went and found a whole other topic but did he allow this out of kind of like a graciousness or did he think oh this is good I'm off the hook here? I've got a…

DE: My problem child has…

BH: Yeah.

DE: …you know, fixed it. Well, he was famous for having students in lots of different areas. He would teach a course on something, which he didn't know much about 'cause he wanted to learn something. And then one of the students in that class would become his student, work on that subject, become famous, and in the meantime Mac Lane had moved on. So the most spectacular example of that is a man named John Thompson who… was very central in the program to classify finite simple groups and Thompson was his student at the moment when Ma Lane thought he was teaching a course in group theory. Thompson was an interesting guy in terms of thesis too. He wanted to settle what's called the Burnside Problem. Uh… well… never mind.

BH: [laughs]

DE: But… [laughs]

BH: [laughs] You love it. This is a trait of David he goes up on these tangents…

DE: Yeah.

BH: …and then realizes hang on, this is actually quite complicated isn't it? [laughs]

DE: Yeah. Thompson spent a year working very hard and Thompson was really a hard worker.

BH: Hmm.

DE: And at the end of a year he packed his notes into cubic foot of cardboard box and threw them out. Decided he was not gonna get that problem. And did something else. Also very good.

BH: Oh wow.

DE: That was a brave thing.

BH: What was your thesis called?

DE: My thesis was called Hereditary Noetherian Rings.

BH: Was it a good one?

DE: [pause] It was okay.

BH: Yeah?

DE: It was pretty good. I was very close with another math major undergraduate, a man named Joe Neisendorfer, whom I still know. So he and I were kind of inseparable Bobbsey twins as undergraduates and we took courses together. One of the requirements of the department was a logic course. And so we took a logic course, and it was given in the philosophy department. And at least half the class were philosophy majors who didn't know any mathematics and we felt the course was below us. We wanted out. So after a quarter, it was a two quarter thing, we went to the math department and said look find us a… please find us a reading course in this subject and we'll go on. Nobody really wanted to give a reading course to undergraduates on that subject in the math department. But John Thompson was buttonholed because he'd once written a paper on recursive functions which was close. And so he agreed mistakenly I think to give us this reading course. This was a period when he had just had a divorce. He was working like crazy on a now very famous paper. And I think he was sleeping under his desk, he wasn't leaving… it was a mad time for him. But here are these two undergraduates coming once a week to talk about Gödel's Incompleteness Theorem which was not what he was interested in.

BH: Right.

DE: So we would come in and he would sort of put the book down. Clearly he had just opened the chapter we were supposed to read.

BH: Right.

DE: And so we would spend the time talking about the first page so to speak of each chapter but it was still a wonderful thing to have worked with him a little bit.

BH: Is it different today? Like do you look at the people, you know, the young ones today doing PhDs and think, yeah your life and your experiences are

similar to mine, or is the landscape changed at that level?

DE: Interesting question. Different universities are very different. So Chicago had a pretty rigid system. Everyone took the same three first year courses and then went on to other things. Berkeley, the faculty is too broad to agree on any three courses. So students are just sort of do something. It's very different that way. But once you settle on a thesis I think it's very similar. And there are these little clumps of seminars and people working together. What's really different though is the job market.

BH: Yeah?

DE: When I was ready to graduate, I applied to exactly two places because I was… one of them I knew the person I wanted to work with and the other one Kaplansky told me to apply there and I did. Yale and Brandeis. I got offers from both of them and I went to Brandeis as I had intended. That's just unheard of now.

BH: What's it like now?

DE: Now people apply to hundred places. And it's scattershot and you try to get noticed so that it isn't quite so random. I find the whole process rather unpleasant now.

BH: Is the problem that there are now too many applicants or too few positions?

DE: People from Berkeley get positions, reasonable positions by and large, so it's not that I mean… I'm talking about Berkeley experience now.

BH: Yeah.

DE: Different places are different. There are more applicants but not that many more. Are there fewer jobs? Maybe than in my year the balance has changed. I was at the end of the sixties. And things were very different in the sixties. One of the things that's different is that it's trivial to apply to a hundred places. Because of a wonderful institution or horrible one called Math Jobs. You fill out one application, your people upload their letters and it goes to any place you check off and you check off every place.

BH: Yeah.

DE: Which is good for students in a way and bad in a way because there's so much noise in the system then it's easy to get overlooked. It becomes a different game and if your thesis advisor is well-known it still matters. It used to matter in a different way but unfortunately it still matters.

BH: So you end up at Brandeis and you're there for quite a while.

DE: I was there for twenty-seven years. I was a postdocwith a very temporary position.

BH: Yeah?

DE: I guess I was called an instructor. Post doc word wasn't so common. And then I was promoted to an assistant professor tenure track. At Brandeis about a quarter of the tenure track people got tenure. It's different in every place. My first job was in 1970, and got tenure in '75 I think. '74 or '75.

BH: And without melting my brain what was your area of expertise?

DE: So when I got there I was still working on non-commutative rings.

BH: Which you'd done your PhD on.

DE: Which I had done my PhD on. It's become a pretty hot field now but at that time almost nobody in Boston working on non-commutative rings. And I'm a gregarious person as you've probably have gathered and so… after a semester I was unhappy. I went to Buchsbaum who was the person because of whom I'd come, I had heard him lecture and liked what he did. And said, look, I know a little bit about commutative rings, you know about commutative rings, but I have a lot of energy and I'd love to work on a problem with you. And so we began working on the conjecture. It proved to be a false conjecture and that was rather a disappointment. So I've always had a pessimistic view of conjectures.

BH: What was it called?

DE: It was called the Lifting Conjecture. And had I known enough algebraic geometry I would have known that it should be false. At that time there was no proper counter example but there was such a strong hint of a counter example that it really wasn't in the cards.

BH: So mathematicians always when there being altruistic tell me it doesn't matter if it's true or false, what matters is that we found the truth. That's all that matters. But deep down you want it to be true.

DE: Oh, it's yes. Yes. It's more glorious if you prove a theorem than if you find a counter example. Though the counter examples are awfully useful. They keep you from trying to prove the theorem.

BH: And what did you move on to next? What was the field that became…

DE: So then I really worked in commutative algebra with Buchsbaum, he was my mentor. And we worked together very closely over the next… nearly ten years. It was a very very profitable thing for me. He was my mentor in many senses. Mac Lane also had been engaged in the mathematical community. He

was a member of the National Academy. He was an editor of the Proceedings of the National Academy. He was a social mathematician. He had been president of the American Mathematical Society, as I eventually was too. So that was a kind of model but Buchsbaum was also very engaged in department politics and university politics. He brought me along with him in some way. He would tell me about what was going on and so I became aware of those things and aware of it as legitimate thing to think about.

[violin music]

DE: I proved some theorem. There was a conjecture… or there was a weak theorem proven and I thought, gee it would be if it were true more generally and I proved it. And I put it in my drawer because I thought, you know, I'm gonna run out of ideas sometime and then I'll take this out of my drawer and publish it and I'll at least have a last paper. So this was in the first couple of years. I was really worried about the well drying up.

BH: So you were like staggering out your… genius?

DE: Yeah. Right. I got over it and I finally did publish the paper.

BH: There was no fear of being scooped?

DE: It could have happened but it wasn't. It didn't. As I've grown older and this is true of quite a few people, it takes me longer and longer to publish something. So I do the work, it sits around, I do a little more work on it a year later. Two years later sometimes five years later. And before it becomes mature and goes out. So that could be scooped sometimes it is. Doesn't bother me so much as it would have then. But I have retained a sense of competitiveness when I'm working I want the good result that I got to be mine.

BH: I was looking on Wikipedia like, you know, looking at things about you

and obviously there are a certain conjectures and things with your name on them. I mean I don't understand what they are and I'm sure in a podcast we can't explain what they are but what's that like? Having something named like the Eisenbud thing?

DE: Especially when it's false later. The Eisenbud Conjecture.

BH: This happened to you, didn't it?

DE: Oh yes.

BH: This was recently wasn't it?

DE: Oh, it's happened more than once but it's happened recently.

BH: [laughs]

DE: I've disproved some of my own conjectures.

BH: [laughs]

DE: But other people have disproved some too. Kaplansky always used to say that you should sort of think of the best possible thing that would be true and make a conjecture, it'll get other people working. It'll show the way, it's a kind of light… a search light ahead of what might be there. And I always take that seriously.

BH: Has anything become a theorem that kept your name on it? Have you had any wins?

DE: Yes. That too.

BH: What's your proudest one? What's the one that, you know, they'll etch on your tombstone or something?

DE: The paper's that most quoted I think is my paper on matrix factorizations from about 1980. That was a fun discovery.

BH: Well I'll put a link in the notes for the show for people who want to…

DE: Sure.

BH: …try their luck. [laughs]

DE: So that's one. The three things I guess I'm best known for are the early work with Buchsbaum, and that theorem and the things that came out of it. And then my work with Joe Harris on modulis space of curves and something called limit linear series which cleaned up a large number of conjectures in the theory of curves.

BH: I know you're still working and doing research, perhaps less than you'd like 'cause of your other responsibilities, we'll come on to that shortly I'm sure. But did you have a white whale? Did you have one that got away?

DE: Ahh.

BH: Your Fermat's Last Theorem or your Riemann Hypothesis or…?

DE: Not so much. There are people who really spend many many years chasing a particular thing. In my very earliest time with Buchsbaum we chased this conjecture quite hard and it got away because it turned out not to have been there to begin with [chuckles], a will o' the wisp. But I tend to work on things that I can do rather than things that are the shining glory of a problem. You know, there was this recent film about Hardy and Ramanujan. And in the film

Hardy is declaiming that the important thing is to achieve immortality by proofing the great theorem. That was never my idea of what was fun in mathematics or interesting in mathematics.

BH: You know so many mathematicians. Are they two types? Are there ones who just do what they can do and are there others that just… are like a dog with a bone with one problem for their life?

DE: Yes indeed there are. Many people keep their thesis problem for all their lives or things related to it. And sometimes its such a good problem that that's a really good thing to do and sometimes its a terrible problem and they're just stuck on it.

BH: It's quite tragic. [chuckles]

DE: It's quite tragic yes. There's luck involved and taste and who your thesis advisor was. I once was talking to a young student at some conference and I asked, her it was, you know, what are you working on? And she told me her problem and we discussed a little bit and became clear that it was really her thesis advisor's problem which he hadn't solved. And it wasn't a very good problem and I thought oh I wish I could do something for her. But of course you can't at that point.

[gentle piano music]

DE: Brandeis, I'm sorry to say lost it's ideal of being a real strong research university. A number of bad things happened that were not Brandeis' fault. They over invested in some building project, the Chancellor who had been so connected to the Jewish community and such a wonderful fundraiser retired finally, does happen to people. And was replaced with someone who was not a good fundraiser. Actually there was a funny story about this guy which I think is worth telling. Which was told to me by one of the fundraisers. He said, you

know, if you're president of Brandeis, there's one word you've got to be able to pronounce correctly and that's Brandeis. But when so and so says it comes out Princeton.

BH: [laughs] Right.

DE: So anyway, that was a problem. It was a problem that Harvard dropped its anti-Semitic quotas and so that rich Jews felt comfortable about giving money to Harvard instead of Brandeis. So there was a whole complex of things that happened. The Seven Day War in Israel, also diverted resources to that.

BH: Yeah.

DE: And so Brandeis began to lose resources, tried to fix it by recruiting more undergraduates. The grade point averages of the incoming students, the SAT scores went down. It became less of a prestige place to go. This spiraled a bit.

BH: Yeah.

DE: And the glory days of funding of science in the sixties also passed and so the whole institute became sort of unsupportable in it's former form. And… math department was cut to a fraction of its size. I was chair a couple of time during this period and I was mad as hell at the administration.

BH: Yeah.

DE: And so I was easy bait.

BH: Right.

DE: And when I heard about this job in Berkeley, I can tell a story about that too.

BH: [laughs]

DE: I was eager.

BH: You were ready to jump?

DE: I was ready to jump. I had been visiting Bernd Sturmfels in Berkeley when he was a new faculty member here. And we were chatting very idly about retirement. This was infinitely far in the future for both of us.

BH: Yeah [chuckles]

DE: But a friend of mine had said that she would really like to retire in Berkeley when she retired. So I repeated this story. And we joked about a bit. And so at the time when the job in Berkeley became available Bernd sent me an email with the subject line, which I still remember, which was Retire in Berkeley? question mark.

BH: Right.

DE: [laughs] And that was the lure.

BH: You just replied, yes? [laughs]

DE: Yeah [laughs] It was quite a bumpy road before I was hired but never mind.

BH: So you came to Berkeley but you didn't come to Berkeley just to be like a math professor down in the math building and teach and research you came with another role?

DE: Right. Right. I came to be director of MSRI.

BH: This is the Mathematical Sciences Research Institute.

DE: Yes.

BH: For people who don't know.

DE: Yes. I would have come to be a professor had they invited me.

BH: Right.

DE: But I was more attractive I think as a director of MSRI.

BH: Right.

DE: Than as a professor. Though they were willing to hire me fortunately. I would not have come without having a tenured job a Berkeley either.

BH: Why would they have targeted you to run MSRI? What had you done?

DE: Well, not much actually. To be perfectly honest I did not have the required experience. I was a bit of a dark horse in every sense. I had been a year… a visitor for a year at MSRI. But… how shall I say, the other candidates were less suitable. [chuckles]

BH: Right?

DE: One of them was perhaps more suitable but there were other political problems that occurred and kept it from happening.

BH: But I mean you'd been chairing a math department.

DE: I twice chaired a math department, I'd run big meetings. I'd been very much involved in organizing things in the community.

BH: Yeah?

DE: In small ways I'd been involved with the American Math Society, so…

BH: You're quite a networked guy then?

DE: I was quite networked but not particularly with people at MSRI or Berkeley.

BH: So when you come to MSRI, this is already an established Institute.

DE: Yes.

BH: And it's doing it's thing, what was your big thing? What did you want to be like your legacy or what was the change you wanted to bring? Or were just wanted to keep it ticking over?

DE: So… Elwyn Berlekamp was the chair of the board at the time and he took hiring a director very personally and very seriously. Even came to visit at home in Newton, Mass.

BH: Right?

DE: To see whether I lived right.

BH: [laughs] Oh right, unannounced or…? [laughs]

DE: No! Announced.

BH: Right.

DE: So I was nervous about this of course and I invited Buchsbaum to come over and join us for the talk.

BH: Okay. Wingman.

DE: Which he did.

BH: Right.

DE: And after Elwyn left, Buchsbaum said boy I'm glad he's not hiring me!

BH: [laughs]

DE: [laughs]

BH: He gave you a thorough going over, did he?

DE: Yeah that's right.

BH: Right.

DE: Anyway apparently he liked what he saw.

BH: Yep.

DE: So eventually I got this offer.

BH: Yeah.

DE: Berlekamp also taught me about fundraising. I had really not done any fundraising though I was interested.

BH: Yeah?

DE: And I went and I spent endless time with… Berlekamp in the car going to visit his friends and him telling me stories of MSRI and of…

BH: Yeah.

DE: Of such things.

BH: Yes.

DE: And he was my mentor very much in those first years. I've been extremely lucky over time with mentors.

BH: Were you lucky with mentors or are there people who just are amenable to having mentors? They are like more sponge-like. Like is it a skill of yours that you're willing to accept mentorship?

DE: I think it's partly a skill of mine and I played this card pretty consciously. That I could go to someone and say look, I'm young and I have lots of energy and you actually know something about this field, let's work together. Then really plunge into their problem.

BH: Yeah.

DE: And that stopped working at a certain point 'cause I grew up.

BH: Yeah.

DE: But then it sort of has worked in the other direction. Former students of mine and I have collaborated very… very happily.

BH: I know you still do mathematics and I don't want to belittle that but this does seem like a time where you've transitioned from being like a mathematician to more of a administrator and mathematical politician or for whatever, you know, the lack of a better phrase is. Like… did you enjoy that? Did you feel like, oh it's time for a new challenge or is that just a natural progression for someone in their career that they have?

DE: Well it's actually a little different than… at least I perceive it a little differently than that. I've been really quite productive in these years.

BH: Right.

DE: So I see myself now as doing both… with some frustrations of course. I'm sure I would have more proved theorems if I weren't director. But that I'm very happy with the trade off. I feel I've done a lot of good for mathematics… in this way. Probably more than those theorems would have done. I'm very proud of MSRI and the changes I've been able to make there too.

BH: I mean also does the fact that you're in so many rooms with so many mathematical people…

DE: Yeah.

BH: …give you bit of an advantage too? Can you sometimes cherry-pick someone who you think you can collaborate with on your own work?

DE: [pause] Sometimes, yes. I had a fantasy of knowing all of mathematics from the beginning. I have to tell you.

BH: Yeah [laughs]

DE: So I wandered. I worked in algebra early on… sort of category theory then algebra. I was interested in algebraic number theory. I was interested in operator algebras. I then moved into singularity theory. I moved into algebraic geometry. At one time I thought I'll move from area to area, I'll know a lot of things. Of course that has dried up a bit because I don't have the time to both be productive and learn a lot of brand new areas. I have to choose. But… when I came to MSRI I had the fantasy that every semester there would be a new program and I would really learn something about a new area every semester. But it's been too busy a job for that and I've been… the process of staying productive mathematically has… ruled that out to some extent.

BH: Give me your best explanation of what MSRI is. I've heard you give this explanation a few times and I have my own way of doing it. Give me your best one that you give to someone who's not a mathematician at all. So if you're in a cab and you're being driven up the hill to MSRI for example and the taxi driver says, what happens in this building? What's this all about? How do you explain it to them?

DE: Well, I've often said and I think it's true, that MSRI is the world's greatest collaborative math research institute. So it's primary a math research institute where people come from all over the world because they want to talk to each other about their favorite problems and we host programs, we don't create them, but we host extremely good people and people who are just developing. So that's one of the things MSRI does. Another is to cultivate talent in lots of areas, lots of different levels of people. Undergraduates, graduate students, postdocs, and others. And the third really is public understanding of mathematics and math education which are related but different areas. And you're my finest achievement in public understand of mathematics. [chuckles]

BH: Alright, well okay. [laughs] Well that's disappointing then. [laughs]

DE: [laughs]

BH: It's important to emphasize there aren't like professors and staff who are employed by MSRI.

DE: There are no permanent staff, scientific staff, at all.

BH: Just the people who run the show, like, you know yourself.

DE: People who run the show have term appointments or like me, or like Héléne serve at pleasure, but presumably… fairly long term. I've been there pretty long too by now.

BH: Yeah.

DE: It's twenty… twenty-one years since I came.

BH: Yeah.

DE: I did take off six years in the middle.

BH: Yeah.

DE: But, it's been twenty years.

BH: Do you wanna hear how I explain it to people?

DE: Please, yes I do.

BH: Alright. I always say, it's this amazing building up on top of a mountain overlooking the San Francisco Bay. It's this building amazing place and it's like

Top Gun for mathematics. And every semester they'll pick one or two big hot topics in mathematics, like things you may not have heard of but to mathematicians are like one of the real big deals at the moment. And then, they'll get all the best mathematicians from all around the world to all come and hang out in this place for half a year and just throw themselves into the topic and talk amongst themselves and have seminars and lectures and gather around blackboards and just like talk about this for six months and see if they can advance it. And then they'll all just go off back to where they came from but for this six months it was this amazing attack and collaboration on the problem.

DE: It's pretty good. I might steal some of that. [chuckles]

BH: Yeah. Top Gun for mathematics though like. 'Cause in Top Gun where they train the pilots.

DE: Uh huh, yeah.

BH: And that's always the pilots from all around, like the Navy, all get picked, like the best…

DE: Uh huh.

BH: …dozen or so get to go to like San Diego just to immerse themself in the best training and the best other pilots. And then they just go back to their aircraft carriers like Maverick.

DE: People tell me I need a better… name for MSRI.

BH: Top Gun?

DE: M S R I, is hard to remember.

BH: It is.

DE: It's hard to say.

BH: And sounds like misery, you always tell me. [chuckles]

DE: Ah.

BH: Yeah.

DE: Well, emissary. Shall I tell you that story?

BH: Go on then.

DE: So at the end of the eighties people had gotten into the habit of calling MSRI, Misery, which is a terrible thing if you're… also trying to be important in the world and raise money and things like that.

BH: Jokingly? Like to be derogatory?

DE: Jokingly! No, completely, you know…

BH: Yeah.

DE: It was such a wonderful place to be.

BH: Right.

DE: Ha ha, we call it Misery.

BH: So it's like irony that…

DE: Yeah.

BH: Okay right.

DE: And the first laser printer that they had was called the Miserable… Laser Writer, right?

BH: Right.

DE: So that was all too catchy, I'm sorry to say.

BH: Right.

DE: And Bill Thurston, one of the things he decided to do was try to fix that. And so he was a very clever guy and he said well if you pronounce the letters rather quickly and with a French accent, Em-S-Air-EE, it sounds like emissary, so let's call it Emissary, that's at least a word.

BH: Yeah. Yeah.

DE: It's pronounceable. Not bad.

BH: Yeah.

DE: Uh, he didn't make friends with this idea because he would say at the beginning of every workshop, now we're changing the name to Emissary so please repeat after me Emissary. And of course the mathematicians don't like to repeat after me at all.

BH: Okay.

DE: So this was kind of a negative for the name. There are still people walking

around who call it Emissary but not very many. And so I abandoned that attempt when I came and just use MSRI.

BH: A few questions that could potentially could have boring answers or could have interesting answers, I don't know.

DE: Hmm. Yeah?

BH: I'm curious about how the topics are picked. Like I don't understand any of the topics that come in these seminars, I read the titles…

DE: Certainly not.

BH: They bamboozle me. But it seems like it would be sought after to have your area get picked for a program at MSRI.

DE: Mhm.

BH: 'Cause it's like this is great if it's there there's a chance I get to spend six months in Berkeley and have this great experience.

DE: Paradise, right.

BH: Yeah, yeah.

DE: We should rename it Paradise, but…

BH: Top Gun, I've said! Top Gun! [laughs]

DE: Top Gun, yeah.

BH: I get to go to Top Gun for… so like…

DE: That's right.

BH: Is there jostling is this like a political backstabby thing or is it… who's making these decisions and how do they do it?

DE: So the decision is made by the so-called Scientific Advisory Committee which is advisory in name only. It's a committee with the power to make those decisions.

BH: Yeah.

DE: They're ten top mathematicians who serve four year non-renewable terms and they work hard. They come in person to MSRI for two days at a time twice a year and they work in between as well. And they donate their services to the institute because they believe in this thing.

BH: But are they being pitched to, like a Shark Tank or Dragon's Den… or?

DE: Yes. So they receive proposals from the outside and they also instigate proposals. So every meeting, One the largest agenda item is consideration of the proposals on hand. And thinking about how to pair them. We also have this schtick which… is that we run two programs at a time typically and we try to choose them so that they have some interesting relationship to each other. And sometimes that's been very successful sometimes it's not successful. Matchmaking is always perilous. But it can be very good and so the committee spends a lot of time on pairings and things like that.

BH: When it's announced like the winner, is this like being told you're gonna get to host the Olympics? Is this like a really big deal for that math community? It's like oh we got… I can't believe it we've got an MSRI slot, or is it a bit more low-key than that?

DE: People work hard as organizers. So it's a little more muted celebration I think.

BH: Right.

DE: But it is very good for the field and people are eager to do it for their field or to have someone else do it for their field preferably.

BH: And then how do you pick who gets to come and actually be on the program?

DE: So the first thing is that a program is chosen with a set of organizers.

BH: Hmm.

DE: The organizers have proposed it. Every program comes as a proposal whether it was instigated purely from the outside or sort of seeded by the committee. It comes as a proposal and it's judged on that basis.

BH: So they've started almost as like pitchers and or advocators and now… now you actually have to run the thing?

DE: That's right.

BH: Right.

DE: So… Brady makes the proposal to work on, you know, Numberphile mathematics.

BH: Yeah. yeah.

DE: And is accepted and then that's three years before the program.

BH: Right.

DE: So we choose almost exactly three years in advance.

BH: Right.

DE: And of course it's been a consideration before that. So the proposal was submitted probably four years in advance or something like that.

BH: Hmm.

DE: Three years in advance you get the green light and then you begin advertising in your community. Who wants to come, oh boy we really need these three people. I'm gonna tell them to apply. But every person who comes has to make an application. And that's actually something which is controversial. We feel it strongly because it levels the playing field.

BH: So even if I'm a Fields Medalist or something like I've got to…

DE: You have to put in an application. You know, there are ways of making it easier for you.

BH: Yeah.

DE: And the organizers can write the recommendation letter but there has to be such a letter and people get quite mad at this. I'm a Fields Medalist! Or I'm a Whatever! Actually the Fields Medalists don't mind.

BH: Right. [chuckles]

DE: [laughs] But the people just below the Fields Medal… [laughs]

BH: [laughs] Okay.

DE: …might be pretty angry.

BH: Right.

DE: And so we fend off this criticism a lot. And the other institutes don't do it. I think we're the only one who really takes that mantra seriously.

BH: Right, right.

DE: Anyway so people apply and they apply in different categories. There are research professors, members, and postdocs.

BH: Yeah.

DE: And some graduate students come with their mentors too. But there's a long list of applications. The organizers submit their recommendations to the SAC which has the power to decide, the Scientific Advisory Committee. And the Human Resources Committee also combs the applications to make sure nobody… no women, no minorities are being missed. And so all that information gets to the SAC, and they make a final decision. And then they call in one of the organizers and say, you know we're thinking of changing your recommendations in the following way, tell us why we're being stupid.

BH: Hmm.

DE: And then there's a negotiation and then there's a final list.

BH: Is it over subscribed? How many people want to come to how many you

take?

DE: So we have ten to one Post-Doc applications.

BH: Right.

DE: Ten applications for every person who comes.

BH: Hmm.

DE: And they're already preselected by field pretty much. So it really is quite a serious competition.

BH: Hmm. Hmm.

DE: Members… it's not as draconian as that, but there's still lots of very good mathematicians whom I would have loved to have who we just didn't have room for.

BH: So how do you measure if the program successful? Have you ever come to the end of the six months and they say like, oh my goodness we've just… we've got a proof!

DE: We've blew it! We blew it! [laughs]

BH: We blew it or we did it? Like you know…

DE: Yeah.

BH: We've just solved the Riemann Hypothesis this is great, like, how do you… what is success?

DE: So that's an interesting question and it's a subtle question to because success is almost never, we've solved the Riemann Hypothesis. There are lots of breakthroughs made. We have lists of them on the website, you can find them. But… we're really an incubator.

BH: Mhm.

DE: Mathematical theorems take a long time, somebody comes they, they had an idea, they wanna prove this, they have a reason why it's possible. They have seventeen conversations, it's advanced this way and that way. And then probably if it's gonna be proved by them they go home and prove it. Rather than on site… so it's a very complicated process and complicated to measure. I personally measure it by the sort of excitement in the field that's been generated by it. And there are programs where the senior people don't come in sufficient numbers and it's kind of quiet and at the end of the thing you feel it just wasn't as good. It's a very robust form, I don't think it's ever really a disaster but certainly there are times when it's just everybody is ecstatic at the end program and other times well it's just sort of like… ah, time to go home.

BH: Well I now know something new because of you as well, it's quite possible they are solving these things and just putting them in a drawer for later…

DE: Yes, that's right. [laughs]

BH: For later in their career. [laughs]

DE: [laughs] that's right.

[gentle piano music]

BH: You meet so many mathematicians and you know so many of them it

seems… I feel like you must know every mathematician in the world sometimes. And you also know some of the really good ones, like superstars. What makes someone a really good mathematician as opposed to an everyday grinding one. You know those superstars, do they have anything in common?

DE: Well they work really hard and they have tremendous passion. Motivation counts for a lot in mathematics and people drift away from… research is hard, research is frustrating for pretty much everybody… and… the rewards are fairly rare. There are big rewards if you really get something you're excited about, that's super exciting.

BH: Yeah.

DE: But, it can be rare and it can be frustrating and the really good mathematicians… maybe they have enough reward so that they keep on or maybe they keep on… and therefore have enough rewards. Talent certainly plays a role. People who say there's no such thing as talent… I think are talking nonsense. I do think everybody can learn mathematics and enjoy mathematics with good teaching. You know there's no such thing as not being able to understand fractions. But talent at the highest level you see and you just… it's jaw dropping.

BH: But what does that look like? Or am I not capable of understanding that? When you're a chalkboard with someone who's like one of these superstars and they're just showing you something, what are they doing that the other man or woman isn't doing?

DE: It's hard to describe but well here's a personal experience. So I was a visitor at an institute in Paris when I was pretty young. So I was five years beyond my degree, I went to this Institute, I walk in, there's one of the most famous mathematicians in the world standing there, Pierre Deligne, and ah… Pierre Deligne, oh my god, but he's very friendly. He takes me out on a bicycle in

the French forest and we have… I'm still in awe. But if you ask Pierre a question, I've always felt and I found this over the years, you get back an answer which first of all is absolutely penetrating but it's at your level. So… he's seen through the problem, he's understood at a very deep level and so he can explain it… what you need to know. And he's not pretentious about it. He's willing to explain it to you.

BH: But that makes him a great communicator…

DE: But the depth of understanding that has to come first and this rapidity with which it happens is astonishing. Now… it has to be said that there are quick mathematicians and slow mathematicians and some of the best ones are not quick. So that kind of instant response and instant ability to solve any problem, some of the best ones have it and some of them don't.

BH: Hmm.

DE: Von Neumann was very famous for speed, for rapidity. There's a sort of silly mathematical problem. Two trains start twenty miles apart facing each other on a track and they go towards each other at forty miles an hour, and a bee starts from the nose of one and flies to the nose of the other and flies back and flies back until finally the trains meet and something happens to the bee. How far did the bee fly? And there are two good ways of solving the problem. There's an infinite series there, you can sum the infinite series, it takes you a little while. Or you can notice that the trains took, if they started twenty miles apart, they each were traveling forty miles an hour, eighty miles of relative speed, it took them a fortieth of an hour or whatever it is to get, right?

BH: Mhm.

DE: The speed of the bee is known as well, I forgot to say.

BH: Yep.

DE: Okay, so the speed that's how far the bee flew.

BH: Yeah.

DE: And the joke was that you could tell whether a person was a mathematician or a physicist by which of those two options they chose. The mathematicians would sum the infinite series, the physicist would see instantly that it was the length of time that the bee flew. So somebody von Neumann this question, he answered immediately and they said, oh! We thought you would have solved it as a mathematician and summing the infinite series. And von Neumann is reported as saying, is there any other way?

BH: [laughs]

DE: [laughs]

BH: Right.

DE: So, [laughs]

BH: [laughs] He just did it quick.

DE: Yeah, right.

BH: Right.

DE: But he was legendary for that speed. And some mathematicians are like that, and some are just not. Some people are superstars in the Olympiad and some never like Olympiads. Don't like those problems, they're not very good at them, just don't feel like working on a problem that somebody else sets, or feel

that they're inadequate at it or something but just don't get grabbed by it. So, there's a great range of style. Some people really genuinely slow seeming but boy do they get to the goal.

BH: Mhm.

DE: By… deep thought somehow.

BH: Hmm.

DE: I've known some of those and I've known some of the very very fast people

BH: Besides being slow or fast, in that respect, are there different styles of mathematician in the same way you can have like a different footballer? Like some footballers are good at catching and others are good at that…

DE: Hmm. Yeah.

BH: Are there genres of mathematician or is…?

DE: Certainly there are. There are people who… who have… incredibly ability to work with complicated formulas and other people who just would never touch a formula and see a geometric picture. Thurston was incredibly geometric for example.

BH: Right.

DE: And just had that insight which was astounding to everybody around him. And there are people who work all their lives on one problem and go deeper and deeper and there are people who… fidget around and are interested in everything in the room and you know… you've happen in with your problem

and they're just as likely to think about that as about their own.

BH: [laughs] Are you scarred of those people coming and stealing all your proofs? [laughs]

DE: You know it's a craft thing. As with most really good craftsman you're glad when somebody else pitches in. You're glad to have them as a collaborator. You're proud to be their collaborator then. Right?

BH: But there must be jealousy and…

DE: And there's also jealousy there and there are different styles of that. There's a famous mathematician whom I won't name who tells his students they must not talk about their problem with anybody else.

BH: Hmm.

DE: Until they've solved it.

BH: Yeah.

DE: It's a secret and… I think that's very bad for students. My students work with each other all the time. So there are lot of personality things. There's a kind of intensity about mathematicians which is… not universal but very characteristic I think. Some people think that very good mathematicians are usually on the autism spectrum somewhere. I think that's over… drawn. But here's another story about how mathematicians are. You know I've studied voice for many years. I'm a singer. I once had a voice teacher… with whom I'd studied for a while and one day he said you know, I'm really sorry to tell you this but you'll never be able to be a professional singer. I didn't want to be a professional singer, I was already well along in my career in mathematics. So I said, you know, what makes you say that? And he said well you're hopelessly introverted.

And… I had to tell him that for a mathematician I was wildly extroverted. [laughs]

BH: [laughs] Yeah.

DE: You know, those two spectra don't meet.

BH: Yeah.

DE: The singer walks out on stage you can tell them instantly from the rest of the instrumentalists by just their bearing. And mathematicians are somewhere to the left of the instrumentalists in their typical introversion. So… there's that side of it too. But you can like collaboration. You can collaborate a lot. But you have to be willing to sit by yourself too.

[violin music plays]

BH: Do you get excited by like new proofs coming out? Like do you follow like the literature and the news and is that like an exciting thing, like sports results for you or…?

DE: Yeah, it is.

BH: Yeah?

DE: I do it less than some people. Some people really keep track of who's scored and what, you know, more like score than like the result.

BH: Right.

DE: I'm very interested in hearing what the latest result is.

BH: Right.

DE: But not so interested in who did it or what the circumstances were. Some people really pay a lot of attention to who's gonna win the Fields Medal next, right?

BH: Yeah.

DE: They have a list. I'm completely out of that.

BH: But you've been involved with the Fields Medal haven't you?

DE: I was on the committee, actually. Not this most recent one but the one before. I was… very pleased at the choice of Maryam Mirzakhani and I'm proud of having had a part in that. Not that it was a difficult thing.

BH: Yeah.

DE: It was an exciting committee. We met in secret. It's gotten a little less secretive now but the tradition was that the committee is completely secret except for the… and that all communication in or out goes through the chair of the committee.

BH: So you can't be lobbied or…?

DE: So you can't be lobbied that's right. So… we were enjoined, we had meetings in New York once and in an European city once and we were enjoined not to give lectures at the nearby universities when were there because all these well known mathematicians suddenly showing up would be suspicious at that time of year. So it was all a big hush hush thing.

BH: So what you couldn't tell your colleagues, you'd say oh I'm going to New

York or I'm going to Europe and you had to have a cover story?

DE: That's right. That's right.

BH: Cool!

DE: [laughs]

BH: [laughs]

DE: That's the most cloak and dagger thing I've ever done.

BH: Yeah.

DE: But it was pointless, of course, really. And I think now they've taken steps to make it a little less crazy. By statute there's no repeat of one Fields committee to the next one.

BH: Right?

DE: So there are records… but they're sealed and the idea is that they should be sealed for seventy years.

BH: Right.

DE: When I was on the committee, seventy years from the first Fields Medal had just come. So I thought, oh goody, I'll look at the records from the first Fields Medal, so I wrote to the secretariat of the International Mathematical Union, saying seventy years has passed, it's my time, I wanna know. And they said we'll look for the records. And a month later they wrote back sadly they said the records have been lost. [chuckles]

BH: Oh, how convenient.

DE: So… [chuckles] the IMU kept its records in a shoebox until quite recently.

BH: Oh okay [laughs] There are a few big like famous ones out there aren't there? The Riemann Hypothesis springs to mind.

DE: Yes.

BH: Fermat's Last Theorem was taken away from us.

DE: ABC is the big…

BH: Yes?

DE: …heartthrob at the moment.

BH: Yeah.

DE: Shall we talk about that controversy? Do you know about that?

BH: Well I know a bit. I would love to talk about it, yeah. Go on then.

DE: So… Mochizuki, who has done very good things, a very fine technician, claimed to have proved it. And he did some odd things. The proof is embedded in a very long series of papers, which don't seem to have much to do with the goal when you look at them.

BH: Hmm.

DE: And people tried to wade through them, very few people got beyond a small piece or really figured out where the action was. There is one exception to

that, which I'll come to. But so he had his coterie of supporters and a group of people who said we don't see the point, we don't see how he could have proved it. We don't see the new idea. He's not telling us the new idea. We're not so sure there is one.

BH: Although he's published, he's just not saying what the part is you should look at?

DE: Published is too strong a word. It's on the archive.

BH: Right?

DE: And it's out there but he refused to come and give talks about it. I put it out there, go read it. If you wanna know how it's done go read it.

BH: Well, Perelman did the exact same thing, didn't he? With the…

DE: No! Perelman came and gave lots of talks.

BH: He did give talks on it, right.

DE: And very good talks.

BH: Okay.

DE: And very… responsive talks.

BH: Okay.

DE: Very different. Actually.

BH: Right, right.

DE: Okay so there's Mochizuki sitting in Japan, surrounded by a group of his students who believe in him.

BH: Yeah.

DE: And a few other people who have sort of taken up the cudgel. Maybe because it's related to their own work is the worm's eye view of that.

BH: Yeah.

DE: Anyway, they believe. So there's this controversy, it's the most important problem in number theory maybe, baring the Riemann Hypothesis, and nobody knows whether it's been proven. It's terrible! And years go by, then the rumor comes out that it's gonna be published by the journal at the place where Mochizuki is.

BH: So even though it's on the archive and publicly available, the fact it's being published gives it more of a veneer of respectability?

DE: It's presumably refereed by somebody respectable.

BH: Yeah.

DE: At a respectable journal. So when this news gets out… the person who is best placed to have understood it and who has understood it… comes forward and says I've read it and it's wrong. And here's where it's wrong.

BH: Right.

DE: And that's Peter Scholze who just won the Fields Medal. And Peter is an amazing person. He's one of those jaw dropping mathematicians.

BH: But what he'd kept quiet as sort of what just out of respect because it wasn't published?

DE: He was very young when he'd read it.

BH: Yeah.

DE: He is still very young, from my point of view.

BH: Yeah. [laughs]

DE: [laughs] And he said well other people are gonna read, they're gonna find the mistake too, it's not my business to get in there.

BH: Right. Yeah.

DE: But somehow when it was announced that the journal where Mochizuki is was gonna publish it he felt that that was wrong.

BH: Right.

DE: So he came forward and the journal immediately retracted this idea that it was gonna be published. Said no, we haven't actually accepted it.

BH: Okay.

DE: So maybe it was just a rumor.

BH: Right.

DE: And other people began to look and so there's no a considerable body of

opinion on the side of Scholze. Mochizuki has not retracted and says Scholze's just dead wrong.

BH: But Scholze's case that he pointed at's pretty compelling is it like? He was like…

DE: People say well, we gotta study for ourselves but he pointed… to the page and the lemma which he said is just not proven and was the key.

BH: Okay.

DE: And so even having that pinpoint statement and from a person of Scholze's standing is pretty scary for the proof. So I would bet considerably against it at this point though… you know, people do make mistakes on both sides, so who knows?

BH: But these ABCs and Riemann Hypothesis sort of things…

DE: Yeah.

BH: They're like… they even excite you guys like they're not just like the ones for us outsiders.

DE: Oh, yeah.

BH: Like throw us a bone. These are the ones that get all you guys buzzing as well.

DE: Yes. ABC trivially implies Fermat for example. And you know it's just a very central statement. Unlike Fermat it's hard to state for a layman, but, not impossible and I think you've done something about it haven't you?

BH: We have video about it.

DE: Yeah.

BH: A while ago on Numberphile. Yeah.

DE: But it is hard to get your mind around.

BH: What would you most like to see happen in mathematics. Like what's the one? Is it… I guess it's gonna be Riemann Hypothesis? Or what's the one you'd love to see happen?

DE: Riemann Hypothesis is certainly the most famous.

BH: Yeah.

DE: The Clay Prizes are not bad. One that I would personally be very excited by is P not equal to NP. Call it computer science if you'd like but I call it mathematics.

BH: Yeah.

DE: It's sort of the next natural step after Gödel.

BH: Right?

DE: And that's a very big one. There are some that are very central but I'm not personally so involved like the equations of motion of fluid mechanics and… knowing whether they could have good solutions and things. Somebody understanding turbulence would be a very big deal, in both in applied and pure mathematics. There are a number of good things out there and they do get solved sometimes. I've been very pleased, I mean some conjecture that I was involved in

which seemed untouchable for many many years got proven pretty recently and by a really nice easy proof. I was just delighted.

[gentle piano music]

BH: There's this great… cliche… that mathematicians are in their prime when they're young.

DE: Mhm. Yeah.

BH: Before they're forty and they do all their best work and after that…

DE: Uh huh.

BH: But you know I speak with you and I think you're past forty now…

DE: [chuckles] I'll bet, yes.

BH: But you know, you're super sharp and seem to know so much about mathematics and for a layman like me seem to know everything. Do you feel slower or less mathematically able than when you were like, you know… romping around in your prime?

DE: Hmm. I notice a difference in how I approach things, yes.

BH: A detrimental one?

DE: There are pluses and minuses. I'm sure overall it would have to be a minus but not a fatal minus by any means.

BH: What do you notice?

DE: So… you know… people don't run four minute miles after they're a certain age.

BH: Yeah.

DE: There's a kind of ability to sprint and to have absolutely no focus but some computation.

BH: Right.

DE: For an extended period. And that's not the way I work anymore. Could I do it? Well… hmm… maybe.

BH: So is it like endurance and concentration?

DE: There's a kind of endurance issue, I think, yes. And sprinting, right? Just the… speed of… the legs running. My twenty year old son could climb stairs a hell of a lot faster than I could and that was a while ago.

BH: Yeah.

DE: It's just the engine doesn't produce the calories as fast.

BH: Hmm.

DE: But on the other hand I know a lot more and I have a lot of associations that I didn't have when I was young. So I have some advantages too, but really I'm not in it for the great problems. I'm in it to have fun. That's the truth of the matter. Let me tell you an agist story.

BH: Okay.

DE: I got to Brandeis as a postdoc, a rather young postdoc. When I was twenty-three… I guess. So a year after I'm standing in the hall at MIT with Mike Artin. And Mike Artin is only a dozen years older than I am, but he was already a famous mathematician. He was tenured professor at MIT, he worked with Grothendieck. He was really the big stuff. He and I and another postdoc who was a good friend were standing together chatting and Mike is a very informal person. Always was. So… he said something about, you know, people our age. The three of us. And I thought, how could someone be so deluded as to think that I would see him as my age.

BH: [laughs] Right.

DE: So he was half again as old as I was at that time.

BH: Yeah. [laughs]

DE: Now of course I understand this error completely. I look at a graduate student and they look to me like myself. But I'm sure they don't see me that way.

BH: [laughs]

DE: So there's that.

BH: What do you want your main legacy to be? Is it all the stuff you've done like at MSRI and the things you've helped create and things you've put in place there or is like, you know, your theorems and your proofs and your mathematical body of work? Which means more to you?

DE: Hmm. That's an interesting question. [pause] They both mean a lot to me… honestly. I think that the fate of administrators is to be forgotten. I've done a lot for MSRI. I've built a wing on it, I've raised money. I don't think that will be well remembered. And that's okay. It's there. I think that some of my theorem's

will be remember longer though. Whether they're remember a hundred and fifty years from now is… well… probably not. But maybe? They'll be part of the texture of mathematics then, but as an individual theorem I don't know. My book on commutative algebra is the best known thing I've done bar none I think, probably better known than my activity at MSRI right now, just in terms of number of people who've used it, or number of people who find it useful to themselves.

BH: And your 17-gon video.

DE: Oh and the 17-gon video. God help me. [laughs]

BH: [laughs] Sorry about that.

DE: If only I had paid more attention to those constructions. [laughs]

BH: [laughs] That will be your legacy.

DE: Yeah, right.

BH: They'll put that on the tombstone. [laughs]

DE: [laughs]

BH: [giggles]

DE: Yeah… so… you know some exposition, some administration, some theorems… I think it's a mix. Life is a whole. Students. I mean I've had a lot off students.

BH: I saw on your Wikipedia page I think that you'd had thirty-one…

DE: Something like that, yeah.

BH: PhD students?

DE: That's right.

BH: I'm not gonna ask you to do it, but could you name them all when it gets to that number?

DE: [laughs] Um… I could get a long way into it.

BH: Okay.

DE: Yeah…

BH: [laughs]

DE: I'm sure I would miss a few. Some of them had left mathematics, some of them have died, some died very young. Some of them I'm very much in touch with and in contact with and take great delight in. It's an interesting thing when I work with a student or when I meet a student even whom... who I haven't seen for a while, we have an instant connection because we have a language in common. And… a whole set of associations, it's like children in a way but it's… for me it's been a very positive thing. I know people who aren't friendly with their students, who are often jealous of their students. But… not me. I'm very very pleased to have those students around.

[gentle violin music]

BH: What happened to your father and like what did he see of your career and things like that because right at the start you were talking about how much that interaction with him…

DE: Mhm. Was very important to me, yes.

BH: Right, what happened later on?

DE: So… he had a long career teaching at Stony Brook and was part of building up that department. He was I think unhappy about his research career and sort of went out of research. Began to teach courses on history of science and on anti-science and… but he taught a course on quantum mechanics over and over and he loved that subject very much, and wrote a book for relative beginners, called the Conceptual Foundations of Quantum Mechanics which was then republished by the American Math Society sometime after. And my wife and I also endowed a prize in his name for math and physics at there. But he lived to see me established at MSRI.

BH: O, right?

DE: I think was very pleased by that.

BH: Did he follow your mathematics? Like would you send him your papers and your breakthroughs?

DE: No. No.

BH: Not for him?

DE: I could tell him some things about it. But we didn't communicate on a technical level very much.

BH: There was snobbery between the two of you mathematics versus physics, or?

DE: No. no.

BH: And your mother? Like did she get to read your books. She always wanted you to write.

DE: Yes. She… well she read introductions. We shared a lot of poetry actually. And I know a lot of poems from that time. She really… got me into that. So that was a very rich stream for me. She would read introductions, she couldn't read the books themselves. But I think she was very proud of me.

BH: And twelve year old and then maybe even university David who had dreamed of being a mathematician.

DE: Yeah?

BH: Do you think he would be… disappointed or pleased or surprised what it actually ended up being like?

DE: Certainly surprised.

BH: Yeah?

DE: He didn't know a thing about the… future really.

BH: What would he be most surprised by?

DE: Well… who knows? But maybe the most surprising aspect would be something that came up between me and Yuri Manin once. I was… immensely pleased to meet Manin at some meeting finally in Spain. He was just beginning to be able to travel and he was at some meeting where I was. And we fell in and got to be friends. And we traveled down to Toledo to see the artwork there. He was a immensely cultured person. I don't know if you've ever had contact with

him or heard of him, even. But a great mathematician and immensely cultured. Anyway we went together, his wife was along and my son was along. And we were sitting in the square in Toledo enjoying the sun and the food and the wine… and Yuri said to me who would have thought that being a mathematician would lead to seeing the world? So.. in a way that's a very surprising thing, how international it's been for me, and how nice that's been.

[gentle music fades in]

BH: Well that's it for today, can I just say a very thank you to Meyer Sound for supporting this episode. Meyer Sound's a great audio engineering company also Berkeley. They're not here to sell you anything, they just wanted to support the Numberphile podcast and we're really grateful to them for doing it. I'm Brady Haran and I'll be back with another podcast very soon. Thanks so much for listening. [music slowly fades out]