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Daniel Koditschek

An interview conducted by
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with
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Q: Why don't we start with where you were born and where you grew up?

Daniel Koditschek: Where I was born. I was born in Montclair, New Jersey, and I moved to a variety of different towns in New Jersey, and eventually my family moved back to Montclair. And I spent my formative years going to Montclair public high school where I learned everything I really know, I think. At least anything I know that counts I learned in Montclair public high school. And I was a historian maybe, or interested in literature, and I was going to be an English major, so that was what – I was never good in math in high school, never, never good. And then I graduated from high school during the time that the protests during the Vietnam War were raging, and that had a huge impact on all of us. I don't know what I would've done about the draft. I had a very high draft number. In those days everyone was given a draft number, and if your number was low enough and the threshold was crossed then you would be drafted into the army. And many people either went to another country or they found a way to be conscientious objectors or they went to jail, so I don't know what I would've done exactly. It was a very, very important experience for us at that point to recognize what mistakes the country was making in Vietnam. My draft number was something like 364 out of 365, or 363, so it didn't become an issue, so I went to college. <laughs> I went to college at Yale University. I started out as an English major. I gradually became disaffected. If you got to write about what other people got to write it would be okay, but you had to write about what other people write about what other people write about, and so that third order removal was just a little bit too hard. And it was also clear that technology was moving ahead, and that was very interesting. It was a time when people were turning their backs on – the agenda of logical positivism had kind of long ago spun its course, but I was confused as to why Wittgenstein had renounced that whole idea why language games? Why not logical positivism? So I moved away from linguistics and started taking more courses in – well, from English to linguistics, from linguistics to maybe anthropology, maybe social anthropology, maybe physical anthropology and eventually came to realize that being in college didn't make any sense and so I dropped out of college. <laughs> And I worked for a little bit of time and then came back. And at the time I was out I had started getting very interested in Wiener's – Norbert Wiener had written numbers of popular books about technology. He didn't write just mathematical books. I couldn't read the mathematical books. But he wrote these popular books, "God & Golem" and things like that about cybernetics and what cybernetics would mean, and it became increasingly irritating to me that I couldn't read these mathematical – that there was this language – I had never encountered the notion that there would be a language that I couldn't read. I thought, this is silly. Why would you be in school and study things that you could study on your own? Why not use school to study something that you couldn't possibly do by yourself? <laughs> So I went back to school and I had in mind to maybe try to force myself to learn mathematics or do mathematics even though it would be very hard. When I got back I met an engineering professor at Yale whose name is Narendra, K. S. Narendra, who had a huge, huge impact on me. He was in this tradition of cybernetics, and he was what's called a control theorist. By then we had stopped using the word cybernetics and started talking about control theory, and he was very, very interested in the intersection of mathematics, you know, the unreasonable effectiveness of

mathematics in the physical, material world. Obviously a very smart guy but a very thoughtful guy, not your typical engineer, as I thought engineers typically were. So I got very close to him intellectually, and by the time I finished college –I finished two years later –I was working almost exclusively with him in his lab beginning to write a few simple papers under his guidance. I had learned enough mathematics painfully, but I was able to do it to be able to start writing some papers, and then I quit, or then I graduated and I swore I'd never go back to academia because this was at a time when the world was changing rapidly and we didn't know – it looked like the social obligations were much stronger than the intellectual obligations on a citizen of the world. So I went to become a literacy tutor in New York State in an area where there was a migrant stream. There was a fruit and vegetable worker stream that was very disenfranchised, and there was a non-migrant stream of native Americans, people whose families had come over on the Mayflower who were involved in dairy and beef farm labor, and they were equally disenfranchised, and it became clear to me gradually, not so quickly, that they needed much more than literacy, so I began to start – we were very influenced by Cesar Chavez and his work with the farm workers in the southwest, and we thought, well, there should be a farm labor organization in the northeast, and so a group of friends and I together with some of the farm workers began to build a farm labor organization. <laughs> That was very exciting, but it didn't work very well. I'm not a very good organizer. I got very, very tired, and we weren't having great success. And all this time I'd been going back to work with Narendra. I would spend – oh, maybe every two months I would come back and work with him, and then every month I would come back and work with him, and then every other week I was coming back to work with him. You know, at some point he said, "Well, w- w- well, who are you fooling here? Why don't you just come on back and be a graduate student?" So at that point, that was 1979, I guess, '79, yeah, I stopped being a public citizen and I became a narrow, a monk, an academic. I mean I stopped thinking that I was not going to be an academic. And then from that point on I had a very straightforward – up until then I had a very non-straightforward entry into academia, but from that point on I had a very traditional experience in academia. I had the good fortune to have an extraordinarily attentive and thoughtful mentor. So in a sense it's anything I know about the world I learned in Montclair public high school. Anything I know about my field I probably learned from this guy, from my mentor, from Narendra. And everything else is kind of a detail, important details, but he really, really had a huge impact on me.

Q: So you were studying control theory?

Daniel Koditschek: Yes, he was a control theorist.

Q: And that was in the engineering department?

Daniel Koditschek: He was a control theorist in what eventually became the electrical engineering department. And the role of control theory, as I guess you guys already know, is to

try to reason formally about the way in which dynamical systems can be affected by external influences. So there's a world of dynamical systems that is much older. The world of dynamical systems got started essentially with Poincaré in the late 19th century. And it was the beginning of a new branch of mathematics that gave rise to what eventually became common to anthropology or algebra – and also C2 it was then called, then it became algebraic topology as it's now known, but it was then common to anthropology, so it was a way of trying to understand what's the geometry of change or what's the geometry of evolution? There's a geometry there, but it's not a spatial geometry. It's a spatiotemporal geometry, and Poincaré got that off the ground. The first control paper was written by Maxwell, James Clerk Maxwell in 18 – I don't know, '70, something. I forgot when it was now. You guys probably know these papers. Maybe you know it. And he looked at windmills. He wrote down a little model of a windmill. People had been building windmills for many, many, many years but the urgency to understand – so if the wind didn't blow fast enough then the windmill would turn too slowly and the emperor's corn wouldn't be ground at the right granularity, and if the wind blew too quickly then the windmill would turn to quickly, right, and so the emperor's corn would be ground too finely, and so the people would lose their heads. And so people began to build governors, as they were called, to adjust the sails of the windmill, to furl or unfurl the sails as a function of the speed. And there was a very long tradition of this that only gradually became understood. People built floating clock valves. Probably you're familiar with these old – going back to the time of antiquities. But Maxwell is the first one to recognize that if you didn't get these feedback systems right then they would <inaudible> or they would go unstable or they would shake themselves into pieces, and if you really a master craftsperson and had great insight then you would get exactly the right masses and exactly the right lengths, and you'd build them so that they wouldn't shake themselves to pieces. Maxwell wrote the first analysis of why something would shake itself to pieces, and it turned out that it had everything to do with the resonance phenomenon that's very familiar to kids who blow across a bottle. Anything that has mass and springiness to it and some damping is going to have this tradeoff between how shaky it is versus how slow and molasses-like it is. You'd like it to not be molasses-like because you'd like it to respond quickly, but if you allow the thing to respond too quickly then it's going to start getting the shakes. And so Maxwell realized to first order you could understand this just by writing down a simple Newtonian spring mass damper equation and looking at the interplay between the mass parameter and the spring parameter, and in some sense that was the beginning of control theory. And then the control theorists got a huge boost in the early 20th century when they realized that the same equations could be used for many, many, many different phenomenon. The Bell Labs people realized that circuits could be analyzed this way. Acoustic phenomena could be analyzed this way. Anything that's got mass and energy is amenable to this analysis to first order. So Narendra was in this tradition. The field had gotten a big bang from Wiener in the early part of the 20th century. Everyone thought that cybernetics was going to be the first synthetic science. You know, kinematics in some sense was the beginning of – kinetics is the study of, for example, how you take rotary things and you get linear motions out of them or how you take linear motions and you automate them to get rotary motions, and that had been practiced by the windmill kind of people or the sawmill people. The waterwheel kind of people had been building those systems for a long time. It became mathematicized by Reuleaux and

people like that in France in the mid-19th century. And then if you started to add dynamics on top of the geometry you began to get things like dynamics and control, cybernetics that looked like that was going to emerge as the first real synthetic science. In round about the middle of the 20th century there was a revolutionary meeting of the Turing style logical folks with the McCulloch and Pitts style neuroscientists together with the electrical engineering circuits people, and they invented this thing called a von Neumann computer, and they realized that there was already a theoretical framework that Turing had developed. So the McCulloch and Pitts paper was one of the most important – it was a neuroscience paper, but they recognized that you could write down a discrete dynamical system, not a continuous dynamical system. You could write down a discrete dynamical system and you could study a computational engine from using this kind of applied mathematics. So as I guess you guys know this was the beginning of computer science and computer engineering, which rapidly took off from 1946. You go down and you'll see the ENIAC, which was developed here in our department in electrical engineering at University of Pennsylvania, between 1946 – in 20 years around the early sixties already the field of computer science or the field of computer engineering, depending on how you think about it, began to show up on the gross domestic product. I did a little investigation once, and I found with the advent of the IBM 360 in 1962, 1963 you can already begin to see information technology showing up as a percentage of the GDP in the statistical abstracts of the United States. You know, it took off, right? It's the information age, and left in the dust, it left the project of cybernetics far, far, far, far in the dust because things were not compositional in the same way. You couldn't snap things together the way you could begin to – the abstraction barriers apparently were clearer. I don't know if they were clearer. The mathematics was already in place to get the abstraction barriers right, so composition – one could begin to build assemblers on top of the machine line, which then one could begin to start thinking about compilers on top of the assemblers and one could – whereas we still don't have – in the world of analog dynamics, continuous dynamics, we still don't really know what the abstraction barriers should be. I mean we think we begin to know in my group, but that's only because my students are contaminated by me. We don't really know. I mean none of us I think know what the abstraction barriers are much less how to compose things. We don't have a compositional theory. We don't have syntax. We don't even have symbols. Again, I think we begin to know what the symbols are, but the whole world of analog engineering was left in the dust by the growing successes of the discrete symbolic computational theories, which wasn't supposed to happen. You know, computers, you would've thought that analog computation would be the right way to go because after all that's what the animals do, and the animals are so much better than anything we could build. But we just couldn't build good analog systems whether electronic analog, electronics or analog mechanical things. So why is that? Why is that so much harder? In this infancy of computer science robotics also got its start, so Engelberger, whom I'm sure you guys have heard of, began to build his company and his universal manipulator. You could say the ENIAC moment for robotics was arguably 1956. If ENIAC in computer science was 1946 then 10 years later Unimate robots began to appear in factories. But it wasn't a 20-year period. It's only now that robotics is beginning to have – so hard automation had an impact on the factory floor in the seventies, eighties. I haven't pinned this down quite as well. But robotics almost still hasn't really shown up in the statistical abstracts of the United States.

Carver Mead revolutionized the design of digital electronic circuits in the seventies at Caltech. He developed these ideas about automating the design of digital circuits, and that gave rise eventually to the MOSIS Foundry System where you could design something in CAD, hit carriage return and have it come back to you in physical instantiation a few months later, now a few weeks later. He then tried to do the same thing for analog electronics a decade later. You can read his piece. He's got these wonderful books. "Conway & Mead" was this book where he and Conway set up a revolution of digital circuitry and digital circuit design and fabrication, which is as important as the theory of computer science for allowing us to have what we call the information <laughs> age, right, or the information technology revolution. Carver Mead then tried to write a similar book, an analogous book for analog electronics inspired very much by NeuroSystems, and his 1986 book is wonderful to read, but it didn't have anywhere near the impact that his 1974 book had. His 1974 book gave rise to a revolution, which changed the human experience. And so I'm trying to portray for you all the ways in which it's confusing to try to understand what it is about one kind of technology that's going to take off and what is another kind of technology that doesn't allow it or can't take off. And you'd like to know why it is that 20 years after ENIAC there was this information technology revolution whereas 50 years after Engelberger we still don't really know what robotics is. And you could say, "Well, those guys were smart and we're stupid," but that's not the explanation I prefer. <laughs> It's not my preferred explanation. My preferred explanation is there's something quite different and much, much, much harder about analog design and engineering in the continuous world than there is about engineering in the discrete world, and that's what my students and I try to study. Gradually as I became increasingly sentient, you know, when did I become a sentient creature? I don't know, when I was 30, something like that.

Q: So when you were doing your graduate work was it already clear that the applications you're interested in were in robotics, or did you get to robotics later?

Daniel Koditschek: No, I thought of myself as a control theorist there. Narendra was working on problems of adaptive control, which roughly speaking is how could a system learn what its parameters should be? How could a controller tune itself up to fit a physical system that wasn't really already experimented with? If you experiment enough with a physical system you would get the parameters down and so you could design your controller and you can drop it in. But you may not have the time or you may not have the ability or you may not have the money to do experiments on this physical thing before you need to drop a controller in. So could you have an adaptive controller that you plug it in and it would figure out enough about what the nature of this phenomenon is that it could function? So there was a growing theory of this, and it turned out that there was a mathematical problem that arose that Narendra thought could be solved by a certain kind of nonlinear analysis. In those days people were doing almost exclusively linear analysis. So he got me started on that, and we wrote some papers and it was fun, and then it got hard and it got harder. And later on it became clear to us that to solve the thing that he really wanted me to solve we would've had to answer one of Hilbert's problems that remains unsolved. Most of Hilbert's problems turn out that either they were solved in the 20th century or one or two

of them turned out to be undecidable. Hilbert didn't dream that there could be such a thing as an undecidable problem, right, but I think his 10th problem turned out to be undecidable, but his 16th problem, it remains open as far as I can tell. And it turned out that what Narendra wanted me to do was to solve it completely would've been a corner of a corner of Hilbert's 16th problem, which I'm not a good mathematician. <laughs> I'm not a mathematician at all. But we didn't understand at the time. So it was very exciting and a lot of fun and we published papers. And then it was time to graduate, but I didn't want to graduate because it's much better to be a graduate student, much, much, much better, as I think you guys – both of you have Ph.D.s. So it's much better to be a graduate student. It's the best thing to be a graduate student. But time to grow up, he said. And I hadn't thought I would be an academic, but he thought I should be an academic. But it was becoming increasingly clearer to me that the field of control had become sterile. Part of the reason that control wasn't working was that the agenda of cybernetics had split into things called control theory, communications theory, signal processing, and it all became very – as every part of technical knowledge in the 20th century, later 20th century everything became radically particular and very silo-ed and very specialized. And it seems to me that the field of control almost self-destructed in the eighties and nineties because it became so sterile. It became so much a game of applied mathematics, but not necessarily the kind of mathematics that the mathematicians would care about nor necessarily anything that you could apply in the physical world. It was already clear to me, or Narendra was helping me see, that the field is very mature. The field of control is very, very mature, and it wasn't clear what would be earth shaking about solving yet – you know, I'm not going to solve Hilbert's problem, right? That's not what I'm capable of doing. I'm not a mathematician. That's not what I'm supposed to – I'm supposed to be designing things. I'm not supposed to be analyzing things. You analyze things only so you could design them better if you're an engineer, but we were doing all analysis and no design. So it seemed like – and Narendra sort of said, "Well, why- why don't you think about robotics? The computation is now available, and people think that robotics is going to be a big thing. Everyone's been working- people have been wanting to do robotics for 2,000 years, so why don't you work on that?" So that was a great idea. <laughs> So I started working on robotics as an application of control and gradually it became clear that robotics was not just an application of control. It certainly wasn't an application of computer science. It wasn't an application of mechanical engineering. It was at this point that I began to see Raibert's work on the hopping robots and had a huge impact on me to recognize that he built a one degree of freedom hopping robot and then he made it hop around on a boom and then he took it off the boom. He had a three degree of freedom hopping robot and then he could put pairs of legs on it and then he had these systematic deeply intuitive – he didn't have any mathematics, a very, very little amount of mathematics that went into his designs, but they were very compositional. And he was building more complicated machines from simpler machines. It didn't work perfectly. They didn't work perfectly, but they worked better than anything else that had been built. No legged machine had ever, ever balanced, barely had any legged machines ever done anything. So that had a huge impact. That work that Raibert did, as I've said, I think is the most important work in robotics in the 20th century because it showed that the problem of robotics is how to get the power – power is always limited. Power is always constrained, or power density is really constrained. Watts per kilogram is really the scarce resource. I mean energy is constrained also,

but you can always get a new battery. But actuators, you can't get watts per kilogram. You'll melt before you get enough watts per kilogram. Okay, so what do you do with the limit – given the power that you have available to you what do you do with that power? How do you distribute it and when? So the problem of programming power, the problem of programming the exchange of energy with the environment is really a fundamental problem that nobody had ever thought to work on before. Raibert realized – he wouldn't say it this way. He makes fun of me for being pompous when I talk this way. He's not a pompous guy. He's not an academic anymore at all, so he can't afford to be pompous. But we're academics. I'm pompous. But he really, really pointed out that that problem was a new problem, and so some of my students and I started chasing that problem. And in the course of chasing that problem it became clear to me that you didn't necessarily want to do control theory. What you wanted to do with control theory is you want to close loops. And when you close loops you're going to end up with an analog system, and the question with an analog system is what are the symbols? How are you going to compose analog systems? Then you begin to realize that no one has ever really succeeded in building a compositional theory of analog systems. People know how to build RL – people in Bell Labs in the '20s, the artists at Bell Labs had built RLC circuits, and then there was enough mathematics done, linear analysis done so that we could teach it to our sophomores by the '70s. We would teach analog design using RLC analysis. But those are all linear dynamical systems. They're not interesting the way that Raibert's machines are interesting. Okay, so how do you study this thing? And that's what my students – any students who work with me and survive end up at least being contaminated for a while to agree with me that what we're really doing is we're trying to understand how to program work, how to take energy exchange systems and compose them, how to mold the energy exchange systems and adapt them to the physical world, and the palette is a mixture of algorithms and electromechanical designs. So I'm going to stop and let you guys get some words in edgewise, because this is now the beginning of my advent as a thinker and a contributor to my field. I would say having graduated from my guru, my advisor, Narendra, and becoming aware of my older brother Raibert's work and how important that was that was my entry point into the life of the field, I would say.

Q: So what was the first robot that you actually worked on and built?

Daniel Koditschek: That my students and I did. Be careful. I'm not a good builder or designer. My students are all the people who do the building and designing. I sort of sit back and I kibitz or I watch what they're doing and try to ask them 100 stupid questions. We then said, okay, if Raibert really is onto something we ought to be able to take his ideas and put them in a completely different physical form, but we ought to be able to use his ideas to get dynamical dexterity in some very different systems, so we said why don't we do dynamically dexterous manipulation? Well, what is that? Well, you know, it's juggling. <laughs> So we built a series of robot jugglers. So the first machine that we built was built by Martin Buhler in my laboratory in '87, I guess. It was a puck sliding on an incline plane, which could become almost vertical, and that puck got hit by a bar. And what we wanted to do is we wanted to show that Raibert's ideas about his vertical hopper could be taken, mathematicized, turned into a juggling algorithm,

and that's essentially what we did. And then we showed that you could do this with a spatial, so Al Rizzi's thesis was to do the same kind of thing with not a planar mechanism but with a spatial mechanism. And then we showed empirically, not with enough mathematics yet, that you could actually compose these things so that just like Raibert could have a two-legged vertical hopper that began to be a biped we could have a two-juggled robot. And we had some analysis that showed that the designs were correct, and we had experiments that would show that the analysis was not unreasonable. And that was probably the first work in the field that tried to simultaneously be theoretical and empirical that we didn't feel, I didn't feel, I still don't feel that the field is mature enough to support this division that you'll see in physics or chemistry between the empirical side and the theoretical side. I think that's a very dangerous thing for an immature field to do. So I think probably our lab was the first to – I don't think there were earlier labs that brought new mathematical analyses to bear on new electromechanical designs and would have to run experiments whilst running proofs, trying to get proofs. So sociologically that's probably one of the main contributions that's come out of my group. Intellectually we've not succeeded in our purpose. So sociologically I think the field is certainly – most people believe that you have to build things and prove things, that you can't do one or the other, and I'm pretty sure that we had made some substantial contribution to that. I'm sure it would've happened, anyway, although maybe not. Maybe it would've gone the way of control theory where it becomes either very mathematical or very pragmatic. But the agenda, the intellectual agenda I still don't think we've made the progress that I'd hoped we would make, which is understanding what it means to program work or what the symbols are. We've made some progress there, and we have better and better machines, but it's hard to argue that those machines are better because of our analytical understanding. I think they are better because of our analytical understanding, but it's hard to argue that given all the advances in computational technology and more recently these advances in microelectromechanical systems, which give rise to amazingly good sensors that we never had before. So it's hard to know whether – well, I think the analytical point of view has made progress, but I don't think we've achieved our intellectual aim, and I guess we won't. You know, I'll probably be retired before robotics becomes a discipline with very sound and intellectual foundations.

Q: What year were the juggling robots?

Daniel Koditschek: So Raibert's first hopper was '82, '83. I became aware of his work in '84, '85. Martin came and joined me in '86. He built his first mechanism in '87. It began to work in '87, '88. Louis Whitcomb came along and built the spatial version and then Al Rizzi came along and used the spatial version to juggle in '89. At the same time we were beginning to be more systematic I had another student at that time, a different student, whose name was Rimon, Elon Rimon, who is now at the Technion. You know, it's a small enough field that all of my st – you know, they all were or they became academics or they've defected, so Buhler now defected from McGill. He formed a company and then Raibert bought him out. And then he quit Raibert's company and he's now working for iRobot. And Rizzi is now the chief roboticist at Boston Dynamics. It's such a small field still that all these lineages are still very – they look very

personal. But of course they're really not that personal. You know, the fields are much more powerful than any individual or any school of thought. But it had felt very personal <laughs> for a very long time. So Elon Rimon started working with me one of Oussama Khatib's ideas. Oussama had done this wonderful work as a doctoral student. He was the one who figured out that Lord Kelvin had a good idea. He wouldn't say it this way, of course. He would say it a different way. But Lord Kelvin had this idea that if you put dissipation into a physical system it's going to decay down to the rest states of the potential energy, and Oussama's idea was, "Hey, let's use this- let's use this to on the one hand avoid high energy places and on the other hand seek the low energy places. If we could encode our goal while making the thing we don't want to have happen be high energy and make the thing that we do want to have happy be- happen be low energy then we'd get an automatic task solver, and it would be an energetically viable task solver. It wouldn't just be a piece of computation." He never said it this way, right? This is my gloss on his ideas, but they were his ideas. And when we looked at what he had done we thought that's exactly right. Lord Kelvin was really an analog computer programmer even though he didn't think about himself that way. So Elon and I began to work on obstacle avoidance and came to realize that a lot of the fundamental limitations had been – that there are fundamental limitations of what you can get out of an analog solver <laughs> that have to do with things that Poincare had begun to discover 100 years earlier. There's this hairy ball lemma. You guys may not have heard of it, but you can comb your hair but you're not going to avoid a cowlick. You just can't avoid the cowlick. If you want it to be smoothly combed there's going to be either a bald spot or there's going to be a cowlick, and there's a fundamental topological obstruction to having smooth fields of arrows or vector fields, as we call them, as the mathematicians call them, on spheres if you think about your head as a sphere. And those constraints begin to give you a foundation of what you can't do in analog design. If you believe that your analog design has to move energy then you're stuck with Newtonian, Lagrangian, Hamiltonian, Kelvin style system, second-owner systems, as they're called. They're dynamical systems, and if they're going to be that dynamical and if you believe at the lowest level you can only have access to smooth things, that you can't do any switching – switching could be done by a computer, but it couldn't be done by a spring mass damper system, per se. Spring mass damper systems have to be – even if they're electrical springs they have to be smooth. They have to act in a smooth way. And then you're suddenly in this world of the topologists. The topologists had been studying the limitations of continuous things, what defects there are if you're trying to build continuous systems and when you're going to end up having breaks, discontinuities, necessary discontinuities, and so those necessary discontinuities, when you squint your eyes they begin to look like symbols. You begin to get discrete phenomenon, and of course the topologists did that. They realized, oh, well, it's not enough to do continuous things. You have to understand the combinatorial nature of the way in which continuous things fit together. It's an electable that you will have discretized components if you're going to try to build things out of continuous pieces. And so that was called combinatorial topology in the early part of the 20th century. It became algebraic topology in the later part of the 20th century. And now there's an emerging field called computational or applied algebraic topology, which we're beginning to benefit from immensely. But it was the first time that I saw in my worrying about where the symbols were going to come from, that there was a very, very, very deep foundational principled way in which

you couldn't avoid the symbols. If you committed yourself to smooth dynamical systems, and why would you do that? You would do that because you realize that you have to program energy. And Newton says you can't program energy without F equals ma. There's no way you're going to get exchange of energy without dynamics. There's no way you're going to have dynamics without smooth or continuous things. And the topologists tell you that there are constraints, there are obstacles, there are obstructions to the way in which things can be put together in the continuous world and still be continuous. You can't. So that was a domain of inquiry that Elon got me started on when he joined my lab. So on the one hand we were watching Raibert build these energetic systems and program and manage energy in his systems. On the other hand Elon was showing me that – we didn't know about algebraic topology at that point, but it became clear that there were these constraints, there were these obstructions that you couldn't avoid. When you tried to do what Oussama had said – Oussama did only a local version. Oussama said, "Oh, let me try to get to the- let me try to get to the bottom. Let me try to get my glasses without hitting the table," and we said, "Okay, let's- let's do that globally. Let's try to build- let's try to set up a field so that the robot would walk into the room and not only would grab the glasses but would from anywhere in the room would be able to avoid all the chairs in the room and all the tables in the room and not bump into any people and- and move down to get glasses," using Oussama's ideas. And eventually you see that there are topological obstructions to doing that, and so the topological obstructions imply that you're going to end up with discrete subcomponents of the space. You have to begin to break the space up into chunks, and we don't know what those chunks should be. You know there will be chunks, and so you don't know what the symbols are, but you know there will be symbols if you pass from signals to symbols at the threshold of analog from continuous to discrete representations of things. And so this was now by '88, '89. We were building juggling machines and Elon was proving things as my graduate student about what kind of obstacle avoidance functions you could have and what you couldn't have. You know, and that's what I've been doing ever since. I guess you have an explosion of understanding in your thirties, twenties and thirties, and then maybe you turn into a broken record. I mean that's what I've been trying to do for the last 25 years, trying to take those ideas and really, really show that they're necessary, discover how they're not sufficient. I'm sure they're not sufficient. Try to learn enough mathematics, which is always very hard for me, so that we can use what's known by the mathematicians, try to make the best use of technology as it rapidly emerges so that we do the best experiments that we can do, and we're always behind. I've had wonderful, wonderful students, and almost all of the students have done things that – we've produced pretty good highly cited papers, and a lot of them have become professors in their own right, and we still don't have a claim. Not only is my group not Turing but none of my students I would say yet have shown that their students are going to be Turing or the Turing equivalent. So that was I guess the second chapter for me, and then the last chapter was when I started thinking about animals and biology, but that was a later chapter. So I'm talking and I think I'll shut up and give you guys some room to –

Q: So how did you get interested then in Moore and the animals or going from manipulation to locomotion?

Daniel Koditschek: Well, locomotion, it's clear that robots are supposed to walk, right, so Raibert, we thought, well, someday we'll be smart enough to beat Raibert at his game, right? So that still hasn't happened, but maybe someday that will happen. But animals, I mean everybody knew that the animals are better, right? Anything that we can build the animals can do better. But it's very hard to study animals. <laughs> The traditions of engineering had been that you've got two poles of design. One pole is experience and the other pole is authority. That you learn as an English major. In Chaucer, you read Chaucer and there's this war between experience and authority, right, and in engineering this tension is very strong between the people who are intuitive designers who can just be able to get out of their way and they'll build something that'll be really, really good, as distinct from – not necessarily opposed to, often opposed to but not necessarily opposed to, distinct from the analysis people who are going to study the thing that was built, try to model what was built, try to see what the flaws were, try to reason mathematically about what could've been done better or how to generalize this thing. Even if you couldn't build it better can you generalize it, or could you take it away from the world of artistry and turn it into the world of synthesis? And I guess what became clear to me somewhere in the nineties was as biology became more quantitative and more accomplished scientifically there was the prospect that you could have a third pole of design, <laughs> which would be if not copying which is a very foolish thing to do, at least inspiration, that you could at least as a third pole of synthesis, which we hadn't really thought of before, that you could try to extract principles from working devices. You could try to use existence proofs, and you could try to actually do more than just watch them. You could try to experiment with them and try to understand – you could try to assert hypotheses about what it was they were doing or what it was they couldn't possibly be doing, and you could try to refute those hypotheses, and you would be advancing biology if you did it well, and of course you would be greatly deepening your understanding of what your design could look like in a way that was neither experience nor authority. It was neither using mathematics to reason purely from a formal point of view nor was it using your students' – not mine, but one's students' deep intuition about how to build things or what could be built. So it's a third pole. So it's emerging. So this idea that bioinspiration is emerging as a third pole of design, greatly enriching engineering. A lot of foolery about it and a lot of not so much intentional but a lot of quackery, unnecessary, unfortunate quackery about, oh, you know, the cockroach has these little hairs and so we're going to put these little hairs here. But the cockroach has constraints that artifacts don't have. The cockroach has to reproduce. <laughs> The cockroach has to eat. The cockroach has to develop and molt. And there are these historical accidents that are – it's very hard to understand whether that hair is there because it's functional in locomotion or whether it's there because the cockroach also has to mate and reproduce. So it's very clear how treacherous bioinspiration is. Biomimicry is never going to work and bioinspiration is very treacherous, and so if we wanted to take that seriously as important as it is to have mathematical collaborators it would be very, very important to find really, really, really powerful biology collaborators. Otherwise it was clear to me that you're not going to do very much interesting. There was this –

Q: What were some of the biologists you collaborated with?

Daniel Koditschek: The guy who's taught me the most is Bob Full at Berkeley. He's taught me an immense number of things. He understands mechanics certainly better than I do. <laughs> he started life as a biomechanics guy, but he's quite a bit broader than that. And he had been – how long are you going to keep this on me? I'll talk the whole afternoon.

<laughter>

Daniel Koditschek: Just help me understand what my –

Q: Our next thing is at two so we have to –

Daniel Koditschek: When is my next thing?

Q: We have to move at two, and we have about five more minutes before this tape will run out.

Daniel Koditschek: Okay, let me just see what my constraints are. I think I'm okay until one thirty, but what happens at one thirty? I'm the chairman of the department, you know, so my life is not my own anymore. Oh, no. I'm okay. I left us until two, okay. So my pumpkin is your pumpkin. Okay, so I'll monolog on for another 10 minutes and then I think maybe it would be better for me to be more interactive in the discussion to try to make sure that we're getting in directions that you think are useful. Bob came out of a tradition of biomechanics that tried to understand why it is – he came from two different traditions that were both immensely appealing to me. One of his traditions was what are the energetics? Tell me what it costs to run. Tell me what it costs to walk. And let's measure the oxy – so miles per gallon for biology is measured by metabolic, by how much oxygen, you know, what you're doing with the oxygen. So they could measure it under controlled enough circumstances. So he came out of that tradition of trying to understand what the animals know how to do with their energy. He also came out of the tradition that said it could be that it's not just the energetics that's a constraint. It could be that it's the control of the behavior that's a constraint. And the intersection of those ideas was a tendency in biomechanics to try to – so it's an empirical fact that whether we run over a scale as humans or whether birds run over the scale or mountain goats run over the scale or elephants run over the scale, if an animal is running over a force plate, a scale, and you look at the ground reaction forces that the scale is reporting you'll see something that looks a lot like a pogo stick. Raibert swears that he didn't read the bio literature here, and of course I believe him. He's a very, very brilliant guy, so he doesn't need math and he doesn't need biology. <laughs> But there was this long tradition going back into the fifties of studying the way in which animals when they run, when they're locomoting in a very dexterous dynamical way they fool the ground into thinking that they're pogo sticks. Bob Full had spent a lot of his career measuring animals and trying to see whether this was just approximately true or how exactly it was true. So my way

of getting his attention was I could give him ideas about how to identify. I had to do even more – I know about dynamical systems, and pogo sticks are dynamical systems, and I could help him think about how to measure whether his data really was anecdotally or really was exactly a pogo stick. And then of course he could be enormously helpful to me <laughs> thinking about how would you probe the animals if you were going to try to figure out whether they're really controlling themselves to be pogo sticks or whether it's just some accident, because after all an animal like a human, the mass is way up here and the legs don't have very much mass, and the tendons are kind of springy, and so isn't it obvious? I mean is it so surprising that when we run over a force plate the scale thinks that it's a pogo stick? But then you start thinking, well, wait a minute. The cockroaches also fool the scale, the thing, and the cockroaches don't look at all like a pogo stick. All right, so there are people who think that this is obvious, and to the extent that it's true it's obvious and to the extent that it's more mathematically careful then it's not true. And then there are people like me who think, yeah, when you start really, really pushing these are not Hamiltonian systems, like the pogo stick ideally would be a Laszlo system, but the metaphor of a pogo stick is such a powerful and useful thing that we should try to capitalize on the ideas of dynamical systems theory and begin to set up a whole maybe house of cards, maybe house of stone.

<break in recording>

Daniel Koditschek: –like to be, we'd like to have flashy things that – you know, in those days there wasn't YouTube, and by the time Rhex came along there began to be Slashdot. In the old juggling days there wasn't even Slashdot, so we wanted to be flashy but we couldn't be flashy. And that's essentially what we've been doing. We've been taking these directions and we've been trying to be careful about them and trying to advance the field and trying to understand what the fundamental limitations would be and why it's so hard to get your hands on the fundamental problem of robotics, or not just robotics but any analog circuits, neuromechanics. If you're trying to do systems biology at the organismal level these problems will all begin to come together if you take this point of view, and they're really, really hard and we're scratching, we're digging. I would say more than scratching the surface we're digging into the surface, but only, only, only very slowly.

Q: How did you end up meeting Bob or learning about his work?

Daniel Koditschek: Yeah, you'd think I would know that.

Q: I'm just curious because it's a different field, so I was just curious of how you would be familiar.

Daniel Koditschek: I should know that. Let me just think a minute. I had a student – Bill Schwin was writing a thesis on the Hoppers and somehow we became aware that there was this biology literature on pogo sticks. I can't remember now how we became aware of that and as soon as we became aware of that, you know, Bob Full (Robert Full) is the king of that world. You wanted to talk to the – if the king will receive you, then you want to talk to the king and fortunately, luckily for me, Bob was willing to spend time talking. I think that's what happened. We realized suddenly that the biologists are also thinking about these things from a very, very different perspective and who are the biologists? Well, it goes back to Kavania [sp?] and Taylor in the sixties and seventies. And who are the people who are doing it now? Oh, it's Bob Full and Reinhard Blickhan and Bob's students. So I think that – I could check and try to be more careful. I think that's true.

Q: So when did you first come to UPenn and who else was doing robotics when you came here?

Daniel Koditschek: I came here because all of my friends – so I was at University of Michigan for a very long time, which is a wonderful, wonderful, powerful, great school. I came here because robotics was very strong here. Everything is strong in Michigan. Michigan is the third, fourth, fifth strongest engineering school in the country depending on who you ask and what field you're talking about. I had not been very successful in getting colleagues at Michigan to be – to think that robotics was a field. People respected my work, they liked me, they treated me enormously well. I have friends, I love the place, I love my colleagues there. I love them. I mean, it's just a wonderful school. But I think I was considered to be harebrained – not harebrained. You would never entrust Koditschek to hire people because Koditschek had such idiosyncratic ideas about where to go on the one hand. On the other hand, you would never make the mistake of thinking that robotics was something that would be a fundamental problem domain, it would be something that every engineering school needs to have. It's a place where computer science meets mechanical engineering meets electrical engineer. That's what it is. It's a technology. Koditschek has these affectations that we'll forgive. We'll forgive them because he brings in enough grant money and he publishes enough papers and he turns out students. But at Penn, simultaneously, they had formed a new department. They had taken two old departments – electrical engineering and systems engineering. They formed a new department called electrical and systems engineering and they were looking for a chairman. At the same time, Penn was a place where some very, very important – the only more important place in robotics has been CMU, arguably, before I showed up. Not with me here. CMU was larger and remains larger, but the intellectual density at Penn in robotics was very, very pronounced. I was very scornful. I remain very scornful about academic officers. I've always told my students that chairmen of departments are pathetic, that I would never, ever, ever do such a thing because you have all this responsibility and no power at all. It's absurd to be an academic officer I have always felt. I still pretty much feel like that. But the Dean here is very unusual. You probably won't meet the Dean. You must be talking to other people. You're going to talk to V.J. Kumar, you're going to talk to Mark Yim.

Q: I don't have Mark Yim.

Daniel Koditschek: Very important guy – modular robotics. Modular robotics – very important guy.

Q: I know of his work.

Daniel Koditschek: Yeah, it's pretty important, I think.

Q: We have George Pappas.

Daniel Koditschek: George is a very important guy.

Q: Who else do we have today? Oh, we have Kostas. [ph?]

Daniel Koditschek: Kostas is a very good guy. A lot of the folks here are really – long before I showed up, Penn was one of the most important places for robotics. Part of the reason that Penn took off was because Ruzena Bajcsy. I'm sure you have gotten to know.

Q: We talked to her.

Daniel Koditschek: She started the robotics group here. It's her thing. I'm just a transient. Part of the reason that she could be so successful was because she's so smart and capable. But at the time that the place was really taking off – she had been working hard for decades, but certainly my whole career, she's been in front. You could argue that the Dean here has been extraordinarily important for robotics and for many other endeavors. So I met this guy and I've worked with a lot of Deans, but this guy, Eduardo Glandt, I'd never met a person like this. And he said, "Well, why don't you come and you can – you'll hire all sorts of people and you'll redefine what electrical means and what systems means. We'll still let you do robotics." So I found him very beguiling and I broke my promise to my students that I would never waste my time in academic management, which is – so I came for those two reasons. In academia, it's very hard to – my wife is a professor and turns out, the Dean of the nursing school – fell in love with my wife and I couldn't have come if that hadn't happened. So that all worked out well.

Q: What year was that that you came?

Daniel Koditschek: 2004.

Q: And how long were you in Michigan?

Daniel Koditschek: 1993 to 2005. I came here in the fall of '04 and then I took – I accepted the position in January of '05. And before that, I was a professor at Yale. I got my degree – my Doctoral degree in 1983 and I was hired on the faculty in 1983.

Q: And you were there until '87?

Daniel Koditschek: No, I was there until 1991. Well, Penn is a really important place in robotics. Well, we'll have to do better. We'd like to overtake CMU, but our friends at CMU are so wonderful that they had the head start and they have wonderful, wonderful people there too. But certainly Ruzena got something off the ground here that very, very few people have been able to pull off. I was not ever going to be able to accomplish anything like that at Michigan. Ruzena probably could have. <laughs> I couldn't.

Q: We were just reading through your CV. I think you had some connections with Japan.

Daniel Koditschek: Yeah, Japan's really important. Since I've been chairman, I've been traveling very little. I haven't been to Japan recently. I haven't been anywhere recently. I've been here, being a chairman. In those days, we thought that Japan – this was before Japan's bust and it looked like – we watched Japan – in the period of the eighties, you could see Japan emerge from being a developing country to a developed country, where the cars would get bigger every year and the clothing would get more expensive every year and the food would get more lavish every year. This was in the eighties and I was traveling once a year, twice a year, sometimes three times a year. They were just wonderfully hospitable. It was clear that they had ideas about robotics that were quite different from ours. They were interested in our ideas. We were fascinated in their ability to build things. Well, particularly now, we feel so bad for our friends and our colleagues who've suffered so terribly with this disaster. But the economic disaster also hit them very, very badly and hasn't stopped the development of robotics in Japan, but really slowed it down, really slowed down a lot of stuff that was going on in Japan. I would continue to travel there if I traveled at all. I don't travel hardly at all right now.

Q: So how did you become involved with them? Did you meet somebody?

Daniel Koditschek: The closet connection that Oussama and I still have is he's – he leads this International Federation of Robotics Research, which in our day was the most important

international locus of robotics and it was the only place where the three continents really tried to have an exchange in a formal way. Ruzena and her generation had been very careful to work with the Europeans and with the Japanese to set up this IFRR, which Oussama is now the president. And so I met the Japanese researchers because Ruzena and her generation were so supportive of my generation. They brought me in, they brought me to the meetings, they introduced me to these people. Not always will the main figures in a field be so accommodating and have such an idea of that mentorship for their younger cohorts as Ruzena and her cohort and her colleagues. Very unusual – Takeo Kanade – these people I'm sure you've heard their names. In addition to being wonderful, fabulous scientists, they were extraordinarily careful about the field, about creating a field and creating the circumstances in the field so that young people could be – could move ahead and could get careers. I don't think that I have been – maybe my generation – certainly I have not – I have not been as careful. I've done very well with my own students, I think, but I don't think I have been as attentive to the field as they were. Oussama has been, right? Oussama has. Oussama has that idea and I probably have it and maybe my excuse is, "Well, it was really important then. It's not so important now because the field is" – but that's just an excuse. So it was Ruzena and Takeo and Bernard Roth and that generation of people who introduced me to the Japanese researchers, Inoue, who's now a national treasurer in Japan and his students and Arimoto, who's retired now had a huge impact on my thinking. He's probably the guy who's closet to me intellectually. Yeah, they were really, really important to us. They have been very important in the field and they've slowed down because the country has been slowed down.

Q: So where were you when you were in Japan?

Daniel Koditschek: I spent most of my time in Arimoto's lab at Tokyo University. He arranged for me a tour of the whole country. I met all that generation of robotics people in that tour that he arranged. Arimoto's another one of these guys. Inoue, Arimoto – these people also had this idea of international exchange and preparing the next generation of people. As I speak about it, I have to say I have not been nearly as thoughtful as those guys were, about the international exchange and about the – establishing mechanisms in the field. Yeah, these guys were just incredible. The field exists probably because of that generation. It's, I think, a lot of the academic – the rapidity of intellectual developments and cross fertilization's been because of there, because of the tone they set.

Q: Are there other Roboticists that you've collaborated with over the years?

Daniel Koditschek: Well, I guess my most important robotics collaboration has been my students. I certainly would be nothing – I would not have been able to build anything. All of them are better builders and designers and many of them have been better applied mathematicians than I – I've asked good questions. I think that's probably my claim, that I ask

really provocative questions, I think. Who've I collaborated with who's not a student? I've not collaborated with people in the field. I've collaborated mainly with people outside the field who I thought would be – would bring tools that I didn't have. I collaborated pretty closely with Robert Ghrist, who's a computational topologist, who's also making discoveries in robotics and domains that you might want to talk to. I don't know if he's here now, but he's a pretty unusual guy. He's one of the spearhead leaders of this computational topology. He's got a joint appointment in mathematics and engineering. I collaborated with him pretty closely because of his mathematical – I wrote papers – some papers with Morris Hirsch, who was a mathematician at Berkeley, who had a large impact on my thinking. Bob Full, his students, Dan Goldman – I've sent him my students, he's sent me his students. My students have been post docs in his lab, his students have been post docs in my lab. I don't know if we invented the word neuromechanics, but I think many people would credit us for having created that domain or that group of students and people. Yeah, it's interesting. I think mostly I've collaborated with people not in my field. I've collaborated with people outside, maybe because I'm so pigheaded about what's the right way to do the work. I think it's probably hard to collaborate with me if you're not my student or if you're not in a different field or maybe I'm just so frenzied about getting it right and doing – yeah, that's interesting. I hadn't thought about it. <laughs>

Q: Just to wrap up since we're going to run out of time, what do you think are s ome of the challenges for the future in robotics? Where do you think the field is going? And when you think about challenges, they can be intellectual, psychological, social – any of those that you think are important to how it's going to develop.

Daniel Koditschek: So intellectually, I think we still don't have good understanding of why power density is a scarce resource and there ought to be some thermodynamic reason for this that nobody seems to be – I don't know what it is and I don't seem to have the right equipment to get my hands on it. So intellectually, those kind of problems, we still don't know what it means, why robotics is a field, how it is – what it is that makes robotics different from – there are people like me who claim to know but I haven't delivered on that claim. And then the people who don't think there is such a thing, so there are those intellectual problems that really, really have not been addressed properly. Then there are technological opportunities that represent immense challenges, that we have the possibility of – so the world of perception is wide open for robotics. The world of computer vision has been in a morass because no one can define what the problem of computer vision is. Whereas in robotics, it's not computer vision, it's robot vision and the job of perception is to close loops around things. It hasn't been up until recently that we had the technology to enable us to be empirical about those kinds of problems because he couldn't do things in real time that involved real sensing. Now you can do these things. The Caucasians showed up in the eighties and nineties and the sensors showed up in the nineties and the aughts. So now the only thing we lack is power density. We don't have good enough actuators. That's not going to change. But we do have sensors and the problem of perception – the problem of robot perception is just wide open and many, many – the best people in the field, I guess, are working on. I'm trying to reform myself and turn myself into something of a perception person.

It's very painful and so it's not how I – so technologically speaking, I think the time is now. Everyone understands that the time is upon us now to try to understand robot perception in a combination of empirical and mathematical ways and so that's happening and that's going to happen. Sebastian is really, really a powerful thinker in that domain and has made major contributions in that domain. Who else? People younger and more agile than I am are doing great work and hopefully I'll be able to catch up. So technologically, I would say that's a very important domain. The other thing that's happening in robotics right now is we're probably reaching the IBM 360 moment, probably. We thought that we got there before, but it looks like the robots are going to become useful, not just to military. They're probably going to become useful to police departments and fire departments and not just automation in companies, but for more flexible – very, very flexible automation for biological automation, biological laboratories. So the advent of commercial robotics is represented, for example, by Raibert's company or iRobot. iRobot kind of got there first, but I don't think they have quite the cutting edge. Raibert's company has – they have Raibert to start with, but they're thinking ahead of commercialized, these very, very elaborate technologies that if you look at them, you'd think they couldn't possibly – you could have that in a laboratory. This kind of thing, you could have a laboratory but no one would ever, ever pay any real money for it because it would break after a day or two. So that's happening and that's got to have an impact. It's going to have positive and negative impacts for academia, the way that the revolution computer engineering demolished a lot of computer – a lot of research in computer engineering got ripped out of universities and trapped in the big Intels and the AMDs. That's happening and that's probably happening in robotics right now. It'll change the way we think about research. So we could go into greater detail on any of these three, but I would say those are the three. If you think of the intellectual problems, sort of the technology opportunities and then the social impact that we're about to start having, I think those are the three big pieces of news for this coming decade.

Q: Can you talk a little bit about this robot?

Daniel Koditschek: This one? This guy – this is the 2001 RHex that Martin Buehler built. This was built by Uluç Saranlı, who's now a professor at Bilkent University in Turkey. He and Martin built this. Martin Buehler was a professor at McGill. We were collaborating together on a project and this is the first machine that ran at more than – if anything, close to a body length per second and the first machine that ran over ground that wasn't level. This is the first legged machine that has actively tunable compliance. This is a Ph.D. thesis supervised by Mark Yim, by my former post doc, Johnathan Clark, who's now a professor at Florida, who was a student of Mark Cutkosky. You surely are going to talk to Mark Cutkosky. Yes? Is he on your list? He's at Stanford. Mark Cutkosky. You've got to talk to him. He's a brilliant mechanical designer. So a lot of the algorithm for the robot is not in the digital computation, but in the analog spring mass damper. So this was the first system to be actively tunable. The compliance or the spring could be actively tunable. This is a project that was also supervised, in part, by Johnathan Clark, is the Ph.D. dissertation of – Goran Lynch was the first dynamical climbing. So we built a RiSE robot with Bob Full and Mark Cutkosky and a couple of other people. But this was the first

vertically climbing machine to be dynamical, actual use kinetic energy. So those I guess are my favorite. I mean, I have jugglers, but the jugglers we couldn't afford to keep. They take up too much space. These are my, I guess, my favorite creatures from the past.

Q: Do you see them applied in real world circumstances?

Daniel Koditschek: If I were a better researcher, we would have – this thing would have been commercialized by now. It should have been used in search and rescue. It should be out in the field by now. It's probably – still, the price performance is still not good enough apparently. So I mean, search and rescue, surveillance, obviously military – legged machines have the ability to manipulate as well as to locomote. Yeah, I'm disappointed that we haven't – I've seen two rounds of companies come and go with trying to commercialize these technologies. I am really good at certain things and apparently really not good at commercialization.

Q: What's your advice to young people who want to get involved in robotics?

Daniel Koditschek: Dream. <laughs> Don't lose sight of the focus on the science fiction and what's cool and why we still are not cool. Try to be driven by how uncool robots are right now and how the animals and how science fiction and imagination about robotics is cool and why is it that we're so pathetic? So try to really keep in sight – try to postpone the day that you become just a person to publish papers and try to keep your eye on how amazing it would be to have real robots. If you really want to make a dent in that, you have to become an expert at least in something. You have to have deep expertise either in computer science or in mechanical engineering or in electrical engineering or in mathematics, but that's – or in biology, in neuroscience. It's increasingly becoming important. But in robotics, it's not enough to just be an expert. You have to be an expert in something. Otherwise you'll never make it. But you also have to know a lot about these other disciplines. You have to really, really commit yourself to interdisciplinary studies in a very painful – very, very painful way. And so it's still a very treacherous field for young people. Young people need to get really good mentors and the old guys, women need to be – they need to be – like Ruzena and Takeo Kanade. They need to be really careful about taking care of and bringing people along because you can become just a gadgeteer or you can become a sterile applied mathematician or you can become – there are many, many ways to go wrong in this very complicated domain. So yeah, I would say the first and most important thing is don't ever lose sight of how pathetic it is what we can do and how huge it would be if we could get – and then the second thing was look at the immensity of knowledge that you're going to have to gain and the kinds of collaborations that you'll have to undertake in order to really realize that. Strangely, I hadn't thought about that question.

Q: Thank you very much.

Daniel Koditschek: Thank you so much. I really appreciate your effort and if we can be helpful to you.