

David: So, Mitchell, at some level, the role of funding is critical for research and discovery and In some sense, it drives, or perhaps distorts, the areas that are looked at. What can you say, based on your own experience, about this?

Mitchell: I always regarded the issue of funding as problematical. Personally, I would prefer never to have any, and in some way, I managed to do that. But of course, someone has to pay for it. The most obvious difficulty is that when you write a grant to get funded, you need to propose what you intend to do, and since there are going to be reviews, you want to propose something that you're pretty sure you'll have something to show at the end of the year.

Mitchell: And to me, that's antithetic to doing original research rather generally. It's also complicated. There is no life professionally. There are lots of interesting problems that come up. It's pretty straightforward. That you'll figure out some limit, whatever, and execute it. Those interesting things and that's part of how we function. That's easier to write a proposal for. Then there are open ended things, which is the more heavy duty stuff. You don't know in advance. If you say, "The problem is this, now we just have to go and do it." At the 99% level, it's junk. We never know what comes out. However, we start with that understanding, as we progress, the original thoughts always turn out to be wrong. It can take three months to realize that math won't work. And you know, when we're professional, we have a better idea which ones are going to run farther down, but we're under no obligation to solve the original problem, which was, in retrospect, probably a stupid one.

Mitchell: In the course of doing it, you realize there are connections different from what you first guessed. You saw it tangentially following those. If you're lucky, you'll see them after a year. Most good things don't pan out, but if you're lucky, something reasonably substantial will come out. I don't have a clue how to write a grant proposal to that.

Mitchell: There's a 1968, if I remember correctly, so in the time just before the Mansfield Amendment, was the announcement of the beginning of NSF funding and there'd be physics grants. I called up to make an appointment, to talk to my thesis advisor because at MIT, it was impossible to just walk in. Graduate students and post docs had to make appointments at the front phalanx of secretaries. So I made an appointment.

Mitchell: And who knows? It was a week or so. I think a Friday, and we had the colloquium. In front of the auditorium are the toilets, and Francis came in, ready to go to the toilet.

David: This is Francis Low, your thesis advisor.

Mitchell: Yes, exactly. Come in ... What do we want to talk about? I said, "If we take this money, it's the end of our business." But anyway, I'm crazy. They're not compatible unless it's a truly open-ended funding. I don't see how it's compatible to endow our original thoughts.

David: So do you see anything in other countries where there is a, perhaps, better system? For example, the Max Plancks in Germany. Once a person is appointed a director, a certain

amount of money is given ... As I was saying, the Max Planck Institute directors receive funding, for which they are not required to make detailed presentations after they're first appointed, and that allows them a great deal of flexibility and freedom in running their groups.

David: Can you imagine anything like that in the U.S., and how would it work, if you can imagine it?

Mitchell: There have always been different models. How well they work, I don't know. That is, there have been Max Plancks with some successful people, and ones that probably didn't amount to much.

Mitchell: There's a model as at the institute [nb: The Institute for Advanced Study in Princeton], where they're fully, internally funded. Now, that sets in a notion of high elitism, and our colleagues there, especially going back to the past, somehow ended up with the feeling that they were was basically in the shadow of Einstein. That's slightly castrating. So it's also not a brilliant model. I think, with some flexibilities, some places have managed to work well. And again, there's no question what needs to fund. One would hope high universities and now big corporations ... We used to teach, and somehow there's more enough, more or less, (enough ambient?? [inaudible 00:07:43] money, that we functioned very well, but we were poorly paid. Now, we're very well paid.

Mitchell: You used to be able to travel everywhere, run a seminar series with international people, and it was nothing. We stayed in flop houses. Often, in rural places, we stayed with colleagues or friends. That model worked probably better than the present one does, but the economic arrangement of academia is a completely different ... My voice is ... What are your thoughts?

David: For what it's worth, my thoughts about the situation are ... I'm in complete agreement that our universities have become essentially corporations, where the primary goal is to increase their reputation and rank and maintain their endowments, if they're private.

Mitchell: Remember, you're speaking from a different position. For myself, who's never administered.

David: Sure.

Mitchell: You've been a provost.

David: True. I've been a provost, so I've seen the dark side as well. It's a dirty little secret that most universities fund their research not completely from external funding, because it's inadequate. The amount of money recovered does not cover the cost of the buildings or of running the universities. And so various universities fund it different ways. The state universities formerly funded it by funding from the state. That's no longer the case in general. The extremely wealthy privates, like Harvard, funded it from their endowments, and the privates without large endowments, like Boston University, funded it actually from undergraduate tuition. My estimate while I was provost, that for

every dollar of federal funding we got, we had to put in between 8 and 20 cents of our own money to fund for the whole enterprise. So it's a very tricky, complicated business, and it is designed to reach shorter term goals, not the long term goals that you mentioned.

Mitchell: Well, maybe things are slightly different now. Let's say a decade ago or 20 years ago, you call up a colleague ... Well, they're answering the 400 emails, and of course, they're Texting something or another, which might be the grant proposal, because most of the time, the grant proposals, every few weeks, had to go to Washington. The actual time involved was on par with the research. In some way, we didn't want things to gestate. It can't be interrupted that way.

David: Let me switch gears a little bit and take you back to Los Alamos, where we used to joke, whenever Pete {nb: Peter Carruthers, Director of the Theoretical Division] came in with a suit on, he was headed to Washington to get money. Those of us who were under Pete actually did not have to do a lot of grant proposal writing, and things were very free.

Mitchell: Zero.

David: Can you speak to that please?

Mitchell: I had a few options in 1974. Sitting there for a year has been the possibility of Los Alamos coming from Pete. The thought of going to Los Alamos was feebly repellent to me. It was something I never expected. Even spending a month there, looking around from the vestiges of the past, hurricane fences with signs on, "Do not pause here."

Mitchell: It was not where [I] wanted to go, but the people going through were extraordinary, and the job would mean a real job, as close to tenure as existed for a staff member. It was clear that it was an opportunity that I couldn't pass up. Pete had me hidden in the woodwork, so I was in T-DOT most of the time.

David: Give us those acronyms. We have too many TLA's as Los Alamos. Three-letter acronyms, remind the people what that is.

Mitchell: T-division Office Theoretical, I think. Only temporary. However, it was an amount that was freedom to travel, no [inaudible 00:13:25] whatsoever to funding. Again, part of it was Pete. It was very close to [inaudible 00:13:36] 100% support. To think wherever I was going, and that's what should be, but it's somewhat irreproducible.

Mitchell: Some of us were lucky. I had barely any publications, but there were a number of the most luminous colleagues who thought I had some unusual potential, which certainly was Pete's thought. Some of us were spelled that way. That talks about a much older system, the 19th century, where the very few professors, 5 or 10 in the country, in a subject, with extraordinary quality. And one formed the best helm that could exist, and offered power and freedom and that makes an impact on people.

Mitchell: I don't see why or how to return to that. If our spectacularly rich people would take hundreds of billions of dollars and put a thousandth of a percent into such things, that would seem to be worthy.

Davis: What about these recent prizes that have been given by the various extremely wealthy people, including Russian oligarchs? What are your thoughts of that? Is that simply rewarding past performance?

Mitchell: Yes. They never support "will be's". These people receive either through friendship or whatever ...

Speaker 1: What about the Simons Foundation? What do you think of what they've been trying to do?

Mitchell: I don't know how to evaluate. Mister Simons seems more removed from the operation, from his company and my interacting with him, as a trustee at Rockefeller, with interest in our center. He has a much more pragmatic mind, and basically insisted that only right people to hire were cryptographers.

David: Were what? Sorry.

Mitchell: Cryptographers.

David: Cryptographers.

Mitchell: As having the right talent to use mathematics in new places, which I totally disagree with. There exists a problem that Mister Simons was a fine mathematician.

Speaker 1: Extremely distinguished, of course.

Mitchell: A fine mathematician, and ran Stony Brook, but mathematicians have a very bad connection to physics.

David: Let's try to weave two threads together, one of which of course involves your most famous work, and the other is your time at Los Alamos. When you went to Los Alamos, what had you known already about the logistic map?

Mitchell: No. The scheme was after I got there, Pete was having us meet once a week at his special meeting, where he and a person were meeting, to see what was going on at Los Alamos. We, knowing other theoretical thoughts, while it looked as though we could bring some new blood to various of their important problems, and Pete rapidly decided because I had been highly interested in the renormalization group, that maybe the time was right, that I could say something about turbulence, which was of high interest to the lab. [I can't talk in the moment.]

Mitchell: So Pete thought it was appropriate to try to use the normalization group to understand turbulence. He already knew of ?? paper. (Mitchell needs to listen to the previous

sentence). There was some classic attempt, using field theory to discuss turbulence. I started to think about it. And you have to know about the high frequency excitations. I had to learn something about [inaudible nonlinear oscillations. There's a big, famous, old book by Matt Sikorski [Sequiski 00:19:16], something very close to that.

David : Sarkovski? inaudible 00:19:21]?

Mitchell: Something like Sikorsky [Sequiski 00:19:23].

David: Mm-hmm (affirmative).

Mitchell: In any case, I read an amount of that. It was immediately clear it was a waste of time to think about differential equations, because I had already known about Newton's method, starting from junior high school. I knew that it could reproduce funny oscillatory behaviors if you started at a right point.

Mitchell: And so, the first thing I did was to see ...(pause)

Mitchell: I wanted to see things that had more complicated behaviors, it was sufficient to look at a map. Obviously, the easiest is $A + X^2$. And so I drew a graph of $A + X^2$, drew a $Y = X$. Evidently, you bring it on from high, low enough, there's a tangency. You can produce two fixed points, one small and the other smaller. Usual story of tangency birucation.

Mitchell: Then, by graphical methods, went up to period four orbits.

David: Let's decide what time ... Is this what period of time?

Mitchell: September, October, 1974.

David: Oh, okay. Just after you'd arrived.

Mitchell: I was able, by graphical means, to begin to see there could be lots of orbits, which gave me some pause, because I was looking for initially a simple thing to analytically understand, to begin to see what things mattered in oscillations.

Mitchell: That's what I was following. At this point, Pete came into my office. Pete got there at the end of summer '73. He had been promised 10 positions by Harold.

David : Agnew, Director of Los Alamos.

Mitchell: When he arrived, Harold informed him, yes, but one for one. He had to fire a person per position. Pete went around for his five guys, having group leaders and the people he knew go through and introduce themselves. "Hi, I'm Pete Carothers. You're fired."

Mitchell: And then, this is now a year later. Pete had a few more positions designed to fill. He was going about and Paul Stein came up. Pete said, "Why don't I {Mitchell} go and talk to

Paul and evaluate him." I saw Paul and we had a discussion, and I mentioned this normal parabola and discovering that it has reasonably large order periodic solutions, and trying to make sense out of them. And he says, "No, well he doesn't know what happens in the concave one, but the convex parabola, they just found this quirk a year ago, following the boundary of a 2-D map, done with Stan [Ulam] the year before that.

David: This is the Metropolis Stein and Stein paper.

Mitchell: That was after.

David: After Ulam.

Mitchell: Stein and Ulam

Mitchell: So it was in a way a simplification of that idea. For this upside down one, then they discovered there were these infinite sequences. They didn't have a clue what happens at the end of them. But in some qualitative way, they'd understood that. I left, and I wrote it back to Pete. This guy is okay, and he's smart. That saved his job. For me, it said this thing was a can of worms. That's what I was looking to avoid. I needed something simpler to get into.

Mitchell: I just started doing other things, and I was on hold for more than half a year until I went to Aspen.

David: This is the summer of '75? [Paused]

[Resumed]

Mitchell: It was an HP65, came in December, which I regarded as a birthday present. [Mitchell's birthday is December 19]. In pretty fast order, I realized what we'd learned about, integration, Simpson's method, that you could do better jobs by approximating the function was completely preposterous, because you think of a function, that part of it that's odd on an interval, which is how the function integrates to zero.

Mitchell: When you pursue that, which I did, there's this spiffy way with end points. You can exactly do to degree to n (??) [inaudible 00:26:27] minus one. That was Gaussian quadrature, which I used as a method. I didn't know any fancy numerical methods, and I just started inventing them and a bunch of number theory, then related it to random number generators. Which was a great guy – the two statistical guys.... Short, blondish, pulled back hair, much older than us. Was one of the greats. Bill ... Was the great stat mech guy we had.

David : You mean at Los Alamos? [Thoughts: George Baker? Bill Visscher? Jim Louck?]

Mitchell: I'll remember. They had done work on multiple good of congruence(?) random number generators. So I had discussions with them about that. I was building up a bag of tools,

but I was also thinking about complex things. That's probably for the winter, spring in 1975. These are general discussions.

[Paused and resumed] Need to fill in this gap and continue the trail of the period doubling.

Need to place the following in context.

Mitchell: Probably the hardest thinking I ever did was the year before that at VPI. For over a year, I was trying to craft a theory of time that was discrete. It was immediately clear such a thing is not a lattice. It's predicated on what you know. If there's a discrete process density, then there's no new moments of time until some process has changed state.

Mitchell: Then, paying attention to relativity, that's a local time. In all cases, they are not, in any way, synchronized one to the next, at different places, so it's not a lattice setup. Of course, the space needs to be discrete as well. Otherwise, you have infinite process density. We might as well have a continuum time, which you infer by motion.

Mitchell: No, was any model you could make up. When you have enough processes, you don't limit what you produce. Regular time ... You can ... Nothing I can think of. It would be like a correspondence principle. Wherever it wasn't poor (?), you're looking from the classical regime to the near quantum. Given these discrete entities that have to mutually measure one another, because what you need to erect a time ... Time is a consensus, a mutual consensus of lots of processes.

Mitchell: So if they're quantum, are they measuring each other? That, of course, is where quantum mechanics is wrong. It's up in the air. So, you follow the Schrödinger equation, unitary evolution, until you do a measurement. Measurement is in principle by a device. No one, of course, has ever worked out what that boundary is. The theory you can build self-consistency against it has always been wrong. That was a complete conundrum. This presumably quantum mechanics is all we knew about things in the little.

Mitchell: You'd have to use [inaudible 00:31:36]. Little guys measuring each other. It was exceeding hard thinking. I could never get enough tacked down things to go anywhere. It was the best thinking I probably did in my life. But that was connected to all of my interests in neurophysiology. I'll take a break.

[Paused and resumed]

Mitchell: My interests had been moving toward big, complex objects. It started as an undergraduate. The part of quantum mechanics not interesting to me were two-body collisions. Then, to my horror as a graduate student, all high energy physics, all the experiments, were gathering experiments. It was just boring geometry, a boring set of objects.

Mitchell: Big, statistical mechanics, which I took a course in as an undergraduate, professor told me, I thought ... This is a subject that I could be interested in working in. He said, "Totally dead subject. No jobs." Which, of course, is completely true until Ken Wilson. Whatever there was in statistical mechanics were in chemistry departments.

Mitchell: When I went to MIT, I decided I would do general relativity. Who became my thesis advisor? Francis. I saw him at the end of the year, to do a reading, which was fine. He said, "As interesting as general relativity is, so is high energy physics." General relativity, of course, is dead, and there are no jobs, which was true until the finding of the black holes.

Mitchell: The two things that struck me as being buzzier and juicer, they were dead and anyway, statistical mechanics with a gazillion parts , you only ended up talking about averages.

David : If I may interrupt, many fields of physics, subfields in particular are "dead". Remember the death of atomic physics until lasers came back.

Mitchell: And nuclear physics.

Speaker 1: And nuclear physics.

Mitchell: Is still dead.

David : We'll see whether the quark-gluon plasma eventually wins. Anyway, sorry.

Mitchell: Kerman died, if you noticed.

David: Who?

Mitchell: Kerman.

David: Arthur Kerman. Yes, I did notice that. Yeah, that's right.

Mitchell: Arthur was a good guy.

David: He was.

Mitchell: He was a very bright nuclear physicist, but a real nuclear physicist.

David: He trained a lot of very good ones too.

Mitchell: He was a very good guy.

David: A total aside, the stuff I did in the model I could solve with semiclassical methods, I developed a collective coordinate approach independently and Arthur said, "Oh, that's pretty good. You developed generator coordinates that nuclear physicists have been using for years."

Mitchell: Anyway ... How large objects work was most interesting to me, and the physiology, what a brain is doing. What is the description to know what sort of equations you should be talking about when you have a million interconnected things? Then, going to a sea wall in a storm. Near equilibrium, statistical mechanics, is so to speak dry Navier Stokes.

Mitchell: And then sort of on general principles. You look something like that. You see this foam in breaking waves, millions of parts. What should you be describing in it? Certainly not just a few moments, but it can be. Is it number of bumps? What is the language in any of these things? The time was that same story. You have to somehow make a consensus out of lots of these disparate things. What sort of equations should such things obey?

Mitchell: And so, the idea of turbulence, to work on it, was decidedly of interest to me. But all that was sitting there, was, on the one hand, rather uninteresting engineering. A rather foolish thing: the homogenous fully developed turbulence theory. And then, theories beginning at ends of a purely physical theory where Navier-Stokes is sitting on a purely scholastic philosophy field, which you make up.

Mitchell: But when you look at the waves, the foams, there are structures and coordination in all of that. It's not the stochastic discussion. And when you set up eddies, the eddies persist without dissipation forever; it's a principle instability of a fluid. And you don't want it to be just an arbitrary, random thing. Of course, all the papers so Abarbanel was doing this ., bunches of people caused Gaussian noise. You're looking, then, at the passive response of the Navier-Stokes equation to this (bin?)bashing noise, which of course didn't agree with any of the exponents or known measurements. This stuff has its eternal life.

Mitchell: As you think about it, none of these approaches is appropriate. All of these things make the thinking about ... the parallelism is interesting, but if you're going to think of the ensemble of all its different funny, periodic motions, that's an infinite dimensional problem. And it was enough to make it interesting. Because otherwise, you're schlepping around with another silly equation.

Mitchell: Once I discovered what things looked like, are they iterated, the universal function describing the fixed point ... Before I knew anything as to its equations, I didn't have a clue what equation could have a solution that looked like that. It's nowhere in the vocabulary that we had. And that's where the juice was.

Need to complete the story of universal function and role of collaboration with Predrag.