With Jack Cowan

ROGER BINGHAM: So we're thinking out loud with Jack Cowan who is a professor of mathematics at the University of Chicago. Jack, let's go back on the trajectory of your life, let's go right back to the beginning and ask those questions: Where were your parents from how did you get into science?

COWAN: Well my father was from Edinburgh. My grandfather and family emigrated from somewhere near Vilnius, a little town called Cownas, in about nineteen-hundred-and-eight. I guess my grandfather was a conscript in the Russian army and managed to get away and got on a boat and landed in Scotland, and I don't know what he did in Russia, but he just sold fruit from a horse and cart in Scotland, Edinburgh. My father was a baker, a master baker, and we had a little grocery store, a semi-kosher grocery store in Edinburgh. Apparently I was born in Leeds where my mother's parents live. They had emigrated from a little town in Poland and my grandfather had a tailoring factory that made cloth for tailoring and the family business was tailoring. And my mother apparently was pretty bright and was offered a scholarship to university when she was a kid, but she was needed to work in the family business so she didn't go.

BINGHAM: You're wearing a tie today which looks like a tartan.

COWAN: Yeah, well, it's actually the Calhoun or MacDonald tartan. It's the closest I can get to Cohen, which is my real family name.

BINGHAM: So semi-kosher, so a Jewish background. Long distant, I assume. Do you have any particular religious faith at this point?

COWAN: Well I, I'm not a member of any synagogue. I would say no, but on the other hand, I'm not unsympathetic to the social aspects of religion.

BINGHAM: What I'm thinking, the work that's occupied you for 40 years basically looks at trying to blend together statistical mechanics and the brain. I mean, these are large topics: you bringing physics together with biology. And that's the sort of thing that might have given you a sort of wider perspective on all these issues.

COWAN: It's true, I mean when I was, I mean apparently… So I was born in Leeds and we moved in 19… I was born in '33 and I think we moved in '38 or '39 to Edinburgh. My father, because he was a baker and he was not called up, and so we, the family, we lived in Edinburgh all during the war. My mother is, was and is a typical Jewish mother. She was always pushing me to academic achievements. Once, I remember, I used to score 100% on chemistry exams, but the master never really liked writing down one-hundreds so he would always mark me down to 98 or something like that, and the rest of the class too because of that, so I was not popular.

But when I would take the report card home, my mother would look and she would say, *well what happened to the other two percent?* And so I was in an environment where both my parents were pushing me because I was very good when I was a kid at school, and my mother claims I was reading when I was 18 months old, but I don't really believe that, but nonetheless I apparently, I was always first in the class in everything and so I ended up getting a scholarship to George Harriet school in Edinburgh, which is one of the better schools in Edinburgh at the time, and had a very happy ten years then. Then I got a fellowship to Edinburgh University to study physics.

BINGHAM: Why physics?

COWAN: Well I got interested in physics when I was probably about 11, when I went into the first year after… in Scotland there wasn't the eleven-plus exam, but anyway, I ended up in the science side rather than the classics side. And I loved the physics and math courses and the chemistry courses and the biology, so I liked all of that stuff. So I used to spend my time going to the natural history museum in Edinburgh and the science museum and pushing all the buttons and playing with all the gadgets and learning about all the stuff. So from early on I got interested in science, particularly physics.

BINGHAM: So you were born in 1933. I think we talked about this before; the man who would become a sort of mentor to you, Dennis Gabor, was actually in Germany in '33.

COWAN: He was on his way… I think in '33 he came to England from Berlin. He'd been a post-doc for a time with Einstein. He'd also worked there, I think, for Siemens. He was within a few weeks of inventing the electron microscope. He actually had the rig that was later worked on by the people who actually got credit for inventing that microscope, and they eventually got the Nobel Prize for it. And he was, I think he was a bit irritated by that fact that he hadn't finished it. And, I mean, he was a remarkable man and, of course, he invented holography in the late 1940s. Anyway, I ran into him in about 1955 when he gave the lecture in the physics department—and not anything to do with optics and holography but on his other interest which was communication theory and learning machines. And he anticipated a lot of the work that goes on now. I mean, he was a really remarkable scientist.

BINGHAM: So where were you in 1955?

COWAN: I was still, at that time I was actually working. Although I had been a very good student in high school, my undergraduate career was a disaster and I really didn't do well at all and didn't take to the physics environment, the teaching at Edinburgh University at the time. And so I was actually working in Ferranti, which was the instrument and fire control laboratory of Ferranti, which was a company which was pioneering computing. And I was asked by my first mentor, who was my boss then, a man called Smith, J.B. Smith; he and a colleague had invented a little machine which solved logic problems by trial and error, which was an interesting idea. Anyway, the machine was gathering dust and not being used, and he asked me to take it, first of all to get it working and make sure it was running properly, and then take it to London, to the Imperial College of Science and Technology, where there was, where my second mentor was, a man called Arthur Porter, a professor of electrical engineering there.

Porter was a very interesting man, is still alive today, I saw him a couple of years ago. He was about 90 years old and hale and hearty, lives in Florida. And he had been one of the people who constructed the first British differential analyzer, as it was called in the early days of computing in the 1930s. He had been a student with D.R. Hartree, and I took it to him and got it working, and he was very interested. And he was the professor and there were two readers at the time in the department. One was Colin Cherry, one of the pioneers in Britain of information theory and communication theory, and the other was Dennis Gabor. And Gabor and I hit it off, and he developed an interest in me and my career and so I went… So my boss at Ferranti, he arranged for me to spend a year at Imperial College with Porter and Gabor. As a result of that, after another year back in Edinburgh, I got a fellowship from what was then called the Hollerith Company, another pioneering British computer company, to MIT. And I found myself studying with Shannon and Wiener, and eventually I became the research assistant to McCullough and Pitts, the pioneers of work on neural networks, mathematical work in neural networks.

BINGHAM: So this was the famous Building 20 at MIT, an extraordinary collection of talent.

COWAN: Yeah, Chomsky was just down the hall. Jerry Letvin and McCullough and Pitts and Wiener and Rosenblith and all kinds of people were there; it was an amazing place. I mean, some of classmates are people who invented all the compression algorithms that we now use in computing and all kinds of stuff there. Those electrical engineering classmates were amazing; some of the people invented the GPS, all sorts of things, so it was an absolutely amazing place in the late 1950s, early 60s.

BINGHAMM: What kind of people were they, McCullough and Pitts?

COWAN: Well they were a complete contrast. McCullough was tall, thin, looked a little bit like Jesus Christ or Abraham Lincoln. He had a long beard, prided himself on his Scottish ancestry, and therefore took a shine to me because I was from Scotland. Walter Pitts, on the other hand, was reputed to have run away from home in Michigan and been rescued by Bertrand Russell, who was visiting the University of Chicago at the time, who introduced him to Rashevsky, who introduced him to McCullough. Pitts was about 17 years old, McCullough was about 43. This was in 1943 or so and McCullough for 20 years had had the idea that somehow logic could be translated into the operations of neurons in the cortex, and he got together with Pitts and Pitts, who was a brilliant mathematician, self-taught, solved the problem of how to do this.

So they wrote a paper in 1943, "A Logical Calculus of the Ideas Imminent in Nervous Activity," which was the foundation for a great deal of work on neural networks. And it had a profound influence on John Von Neumann, who… because McCullough and Pitts and Wiener and Von Neumann had launched a series of conferences sponsored by the Macy Foundation in the late 1940s, which fostered the development of what was then called cybernetics. And so when I got there this first flush of this was over a bit, but nonetheless, there were all kinds of things going on. McCullough and Pitts' work influenced Von Neumann in his work on the design of computers. And, in fact, the early papers of Von Neumann on how you would set up a general purpose computing machine were actually written using the formulism of McCullough and Pitts, but later on it was modified. And so McCullough was very interested in the problem that Von Neumann had worked on: how you build reliable computers from unreliable elements. And so he played an audiotape for me, of a lecture on this topic by Von Neumann. Now I had actually already read Von Neumann's paper while I was back at Imperial College and I had gotten interested in it because I had by then developed a way of thinking about these problems that used what's called many-valued logic—logic that doesn't deal with just true and false, but true, possible, probable, false, things like that. And it seemed to me like it could be used to look at parallel computing devices.

Anyway, it turned out that I with another graduate student who I recruited to McCullough's group called Shmuel Winograd, who is now an IBM fellow. We actually made a fundamental advance in that subject and introduced what is now called parallel distributed computing architecture for networks of interconnected McCullough-Pitts units that solved the problem that Von Neumann had been working on and made contact with Shannon's work on the noisy-channel coding theorem of information theory. And so we both of us got known for this kind of work. And so I actually was asked by the Office of Naval Research in the US, part of the US Department of Naval Affairs, if I would like grant support. And they gave me a grant, which I had when I went back to England for a few years. I went back to Imperial College, just down the hall from Gabor, who still I used to talk to every day, in fact, shared his secretary and gave a few lectures for the department and supervised a student or two, but I really wasn't a faculty member, I just had my own grant and was independent. And then I spent a year at Teddington in the National Physics Laboratory there with Albert Eliller, another one of the very interesting British pioneers of machine intelligence.

BINGHAM: Did you have any sense in that time where computing was going to go? I mean, you just gave a talk today; you brought your Apple Macintosh with you… The amount of power packed into one of those…

COWAN: Yeah, I know. Well actually, in 1965 or thereabouts, Minsky… So I think I mentioned to you, I helped get Seymour Papert to MIT and he ended up working with Marvin. Anyway, Minsky was heavily involved with the early ARPA work on remote log-in computing and stuff.

BINGHAM: This was the advanced research projects agency?

COWAN: Yeah, and they offered me a terminal, in England, but I chose not to…

BINGHAM: Which became the World Wide Web?

COWAN: Yeah, which became the web, so I could have had one, but I thought I'll concentrate on what I was doing. The great thing about my grant was it freed me from having to get a job and teach and do all the things you do as a junior academic, and so I was able to sit and think. And I had been triggered by Norbert Wiener and Walter Pitts principally, both of those guys, into thinking about how to develop differential equations, the equations that deal with the mathematics of continuous systems, rather than computer science mathematics, the mathematics of things that aren't continuous but have jumps in them. And I spent those five years trying to figure out how to develop differential equations for networks of neurons and how to use the methods of statistical mechanics, because various people had suggested that this was how to tackle the description of brain dynamics, but they never really had quite done it. And so I was actually able to sit and think all that through, and in fact, I got one of my best ideas on that when I was on my way to the 1966 World Cup final in London between England and West Germany, and it was quite sudden. I was sitting in the London Underground, on the train; it was quite heavily… there was a lot of people on the train, it was quite packed, and I was sort of compressed, and I just remember at the time, I was sitting surrounded by German football fans, and all of a sudden I had this idea about how to do some stuff and after that I knew how to proceed on some of the details.

BINGHAM: So that's one of those eureka moments, one of those moments we've talked about in science.

COWAN: Yeah, it was quite a moment. And so what I was able to do then was produce the first reasonable treatment of the mathematics of large-scale brain activity. And I was very fortunate; when I got to Chicago and I became the chairman of the Committee on Mathematical Biology, as a result of my early work in McCullough's group, it was my first academic job. Technically I was still a graduate student, and I went straight from that to be a full professor and chairman of an academic unit of Chicago. I mean, it was greatly to their credit, that they were willing to take a chance on someone like me.

BINGHAM: So did you meet—just backtracking a little bit—did you actually meet Johnny Von Neumann?

COWAN: No, unfortunately he died before, just before I got to the US. I got to MIT in 1958, Johnny Von Neumann died in 1957, so I never got to meet him. And similarly, I never got to meet Turing, although I'd read a lot of stuff by Turing and about him that I just… He died before I got really interested.

BINGHAM: What about Claude Shannon?

COWAN: Well Shannon was one of my teachers, and I used to go, once a week I would go and talk to Claude about various things I was doing. He thought I was a little glib, actually. I remember because I said, *well I'd been thinking about*… He told me he was thinking about two-way communication channels and stuff, and I said to him *well I've been thinking about that too,* and he smiled and he thought I was being young and brash, which I probably was, but anyway, I was thinking about things that he was thinking about.

BINGHAM: So there was this long period of just being able to sit and think.

COWAN: Yeah, I was, in retrospect, it was really a great benefit to me.

BINGHAM: So you tell me this story about Rabi, when you…

COWAN:
Yeah, and years later I became a member of a very interesting group, the organizing committee of what was called L'Institut de la Vie, which was set up by a physicist, Herbert Frohlich, one of the pioneers of superconductivity, and a French anatomist by the name of Maurice Marois. Sometime in the mid-'60s, they got together with Madame Frohlich, as well, who was a UFC graduate actually, in English, and decided that they would set up a little foundation which they called the Institute for Life, l'Institut de la Vie, to study biology, but from the point of view of physics. The idea, the original idea, was that there would be talks by biologists, and all the distinguished physicists would listen to them and solve the problems, but it never worked out like that. But they, it was an amazing group of people. There was an organizing committee of 40 people eventually, of from 32 were Nobel Laureates, fairly elderly. And then there were some younger people, many of whom became Nobel Laureates, and there were a few other people, like me, who actually did some of the work, helping to organize things. And it met every other year in the Chateau de Versailles, the palace at Versailles, in the Salle Marengo room. And they had these amazing meetings, and every other year they would go to some interesting place to organize the meetings.

So one year they went to Italy as the guests of one of the organizing committee members, Giorgio Careri, and had an audience with the Pope. Another year we went to Belgium and had an audience with the King and Queen of Belgium because, Prigogine was the host for that meeting and he was the Science Advisor to the king and queen. And another year we were entertained at Wolfson College, which had just been set up, with Dorothy Hodgkin as the host. It was very interesting to meet all these people, and people on the committee were people like Rabi and Wigner and all kinds of really distinguished physicists and chemists and biologists. So for me it was a great experience, and what I didn't realize was that the meeting was being used by the Nobel Foundation as a kind of vetting of potential Nobel candidates, for screening.

BINGHAM: Now I was thinking when you were talking about your experience of having the time to think…

COWAN:
So Rabi told me the story of how he had started in his career and it ended up in the *New Yorker*—I think it was a long article in the *New Yorker*, I think it might have been Jeremy Bernstein who wrote it up. But Rabi told me the following story, showing how coincidences affected him. He went, he couldn't get a job, he decided he would go after he graduated, and he decided to spend some time in Germany. So he went, he ended up in Berlin, I think with the, with Frisch, or I'm not sure, it might be Otto Hahn or somebody like that. Anyway, Heisenberg happened to be visiting, and so Rabi was introduced to Heisenberg, and then Heisenberg went to the US and he was asked by the chairman of the physics department at Columbia if he knew any American who could teach the new subject of quantum mechanics. Well Heisenberg didn't know anybody here, but then he remembered he had met Rabi who he'd been impressed with, and so he suggested Rabi. And so on that basis, Rabi got a job at Columbia University in the physics department to teach quantum mechanics and also to do research. And he was given a large room which he could turn into a laboratory. But he told me something really interesting, which struck a chord with me. He said all he did was put a table and a chair and a pad of paper in there and he sat there for about 18 months, thinking. And then he had an idea. He rapidly equipped his laboratory, he carried out the first measurements on nuclear magnetic resonance, and of course eventually got a Nobel Prize. But it's a great example of the way to do science. First think then do the work, which I think is a great way to do things.

BINGHAM: Going back, there was another great itinerant mathematician that people recognize called Paul Erdös…

COWAN: Ah yeah, I never met him, unfortunately.

BINGHAM: Who used to turn up at various places and say, *my brain is open*. But there was also a phrase that is well known, you can even look on the World Wide Web, now on Wikipedia, and you'll find an entry for Erdös number, and that was, an

Erdös number of one was meant that you'd call for the paper with Erdös in it, two, it was degrees of relativity.

COWAN: That's right, my Erdös number is two because my graduate student collaborator from MIT, Shmuel Winograd, he wrote at least one paper with Ehrlich, maybe more, so since I wrote a monograph with Shmuel, my Erdös number is two.

BINGHAM: You also have an Einstein number.

COWAN: Yeah that's right. Well Gabor spent time with Einstein, and I spent time with Gabor, so. It's extremely important in a younger scientist's career to have mentors, and I was very fortunate in having such amazing mentors as Gabor and McCullough. I mean, they were really remarkable people. I bet all the other people at MIT were fantastic. Shannon and Minsky and Peter Elias, who was my nominal supervisor as a graduate student, a whole bunch of people there were just remarkable people. And they were so free with their time, and if you listened carefully, you could really learn stuff from them; and so I've always been aware of that when I have my own graduate students, how important it is to do your best to help them.

BINGHAM: Do you have a simple explanation, something your mother could understand, to sort of explain what you've been doing these past 40 years?

COWAN: Well I've just been trying to apply the methods of mathematical physics to thinking about how the brain works. By that I mean that there is a way in which physicists approach the world, theoretical physicists, that I think really, really works and is really interesting. They don't try to put in every detail of what the phenomenon is like. They, if they have good taste, they select only those details that are really important for the questions they want to answer. And they construct what are sometimes called toy models, which aren't facing reality, to quote the title of a book by a friend of mine, Sir John Eccles, but they abstract from reality just what is needed to understand something. And I think that's what I've been trying to do with respect to brain mechanisms: try to make toy models that contain enough details to answer questions about and give you ways to think about what's going on in the brain. It's not, I mean, it's not something that's commonly done. A lot of the time people do computational neuroscience where they put in a lot of details and make simulations and study what goes on. I don't do that. I tend to put in as few details as possible and say things that are interesting with few details rather than put in a lot of details.

BINGHAM: You were telling me that Terry Sejnowski, who is of course an esteemed computational scientist, came to you once and gave you some instruction about it.

COWAN: Yeah well, so I have a bad habit—well I had a bad habit, I still have the same bad habits—of sitting on problems for a very long time before publishing. And in fact, there are quite a few things that I haven't published which I should have. But basically, so Terry wrote down some rules for me. The first rule he wrote on the board with tongue in cheek, I have to say, was publish first, think later. And the second rule was one idea, one paper. And I tend to do the opposite. I think first, then I publish, and I try to put 10 ideas into a paper rather than one. And both of these are very bad habits. And of course, Terry is amazingly successful.

BINGHAM: Which other scientists that you've known have you, have had the most impact on you, the ones that you've most respected? We've talked about Rabi, we've talked about, I think it was Manfred Eigen…

COWAN: Well so Manfred was another one I considered a mentor. I met him, he was part of the organizing committee of l'Institut de la Vie, and I met him there and got to know him a little bit, and then one day I was visiting someone else whom I had originally met through l'Institut de la Vie, that was Gerry Edelman. I was visiting Gerry at the Rockefeller and I bumped into Manfred and I intro… and I said *oh hello,* and then he said to me, *did you write a paper recently that appeared in the biophysical journal?* It was the first paper I had written with my very clever post-doctoral fellow Hugh Wilson in 1972, this first real treatment of the mathematics of large-scale neural networks. And I said, *yes, that's me*; he said, *well would you like to come and give a talk on it at the little seminar I run in Switzerland every winter?* So I found myself talking on the subject at Manfred's group in, at that time it was near San Moritz. And it turned out that seminar had been running for about eight years then, from about 1965, and it ran for over 30 years, every January. And it turned out to be a very, very interesting meeting all about theoretical biology in, at every level you could think of, and it was a wonderful meeting. And I developed a great admiration for Manfred and for quite a few of the people at that seminar, and learned an enormous amount from them. Manfred—another person there I got to know extremely well, although I knew him before I went to the seminar, was Werner Reichardt, who had done beautiful pioneering work on the nervous system of flies. A very German tradition to study organisms like flies rather than cats and monkeys and humans, because you can learn a lot from studying simple networks rather than complicated networks.

BINGHAM: Did you enjoy teaching? Do you enjoy teaching?

COWAN: Yeah actually, at first I didn't really, but I gradually got to a stage where I like to teach.

BINGHAM: Which subjects do you teach?

COWAN: Well I've taught a variety of things. I used to just give lectures on circuit theory and then on mathematical biology, but then I joined the mathematics department and in about 1980 or thereabouts, I moved to the mathematics department because by then theoretical biology in the US was somewhat dormant and NIH had withdrawn support for biophysics and theoretical biology for a period because all the kids were getting more interested in genetic engineering and stuff like that. And so I found myself teaching mathematical methods in the physical sciences to the physics and chemistry and biology students at the University of Chicago, and it turned out I was good at it and I enjoyed it and I did that for quite a few years. And then I also was teaching graduate courses to my students and so on. And then about a decade ago, the department decided it would teach a course in mathematical finance, and I volunteered to teach it because it turns out the mathematics of mathematical finance is stuff I know anyway; it's the same kind of mathematics that I used to do brain research. So I became an expert on option pricing because of that. But then, I've now returned to, I'm just starting again to teach the mathematics of physics and chemistry.

BINGHAM: Do you have any thoughts about… I'm thinking about the recent National Academy's report on the state of science, which sounds a dire warning…

COWAN: Well I'm a person who… I've had a few grants, but mainly from private foundations. I got money from the James S. Macdonald Foundation for a few years, and I got money from the US Navy for a few years, two separate times, and I got money from a couple of other foundations. I've always had problems getting money out of NIH and NSF, and I used to be on their *ad hoc* committee for a while, but I got thrown off it. I don't like the set up with the funding. It tends to not support people who are trying to push the envelope. It tends to support people who are in the mainstream, and I don't think it's a good idea. I think there's far too much top-down administration of science, both in this country and the UK. I think it should be much more bottom-up, so I'm a great fan of individual investigator grants rather than programs and institutes and forcing people to collaborate and all kinds of things; I'm very much against that. I still think the individual investigator approach is the best one.

BINGHAM: How do you create an environment which is supportive of innovation, which is what you're talking about?

COWAN: Well I think you just support people rather than projects. I mean, that's what I benefited from. I mean, in 1962 I gave a lecture at, interestingly in the Museum of Science and Industry at the University of Chicago, just near the university. After I had given my lecture that evening, I was approached by two people from the Office of Naval Research. This was 1962, and they said, *would you like a grant?* I said yes, and I ended up being paid to sit and think for a few years! I think that's the way, an ideal situation. Of course, it's not practical to do it in the large today, but I certainly think it's very hard if you try to push the envelope, to get real grant support these days.

BINGHAM: The field of cybernetics, that we are so used to now… you actually knew Norbert Wiener?

COWAN: Oh yeah, in fact, Norbert and I were actually going to work on the statistical mechanics of the brain. Like I said, I was triggered in part by Pitts, remarking to me about the need to do continuous mathematics and to study these things. And I, he gave me a fragment of a manuscript in which he'd tried to set up the problem along certain lines that involved statistical mechanics, but it was only a fragment and it wasn't really the right approach, but it was very interesting. And I also studied, read very carefully, Wiener's book on cybernetics, and then I got to know him. He came to a lecture of mine once, and he stood up after the lecture and made a comment which I thought was a comment on his own work disguised as a question on my talk, but in fact, in retrospect, when I read the transcript because it was published, it was a series of very penetrating remarks about what I was trying to do. And so we actually ended up planning to work together on this problem, but then he died, unfortunately.

BINGHAM: What was he like?

COWAN: Oh, he was wonderful. And I did have a few, spent a few times with him that I thought were great. He once sang vaudeville songs to me from the 1920s when we were in a little café in Amsterdam. I was amazed, but it was beautiful. And then another time we took a walk along the river Charles near MIT, and he claimed that the eddies in the river were what triggered a lot of his ideas about random walks and noise—which I thought was amusing because he had very poor sight—but I found him really interesting, really clever man, a very, very smart man.

BINGHAM: What field would you have gone into, had you not gone into science?

COWAN: Um, I can't think of anything I would have done rather than what I've been doing.

BINGHAM: So what are your favorite pastimes?

COWAN: Well, I have, over the last few years; I've finally gotten interested in astronomy, so I'm a very keen amateur astronomer. A very frustrated one because I live in Chicago and we have big problems with light pollution, so I would love to be able to live in a place like Santa Fe, in the outskirts there in New Mexico where you can see the Milky Way at night. But I compensate with astrophotography. And the other thing I like to do is, I fly fish. So I drive five hours with a colleague to Michigan and we fly fish. And there's nothing like fly fishing for getting on the water early in the morning before the light comes up with the mist rising from the river. You can't think about anything when you're fly fishing on the river. And I get a complete relaxation there. So, I do that. And then I come back and I find myself re-energized to think about the problems I work with.

BINGHAM: It's interesting this attempt to blend quantum mechanics and brain mechanics, if you like. This makes me think of the underpinnings of consciousness, the "c" word. Francis Crick of course was here at the Salk Institute, and Francis used to say that there was a sort of faulty syllogism. People thought quantum mechanics was a mysterious and almost intractable problem, consciousness is an intractable problem. Therefore quantum mechanics must explain consciousness.

COWAN: Yeah that's basically Roger Penrose's…

BINGHAM: Penrose and the whole notion of microtubules with Stuart Hameroff. Do you think there's any likelihood that they're right in finding very microscopic quantum mechanical effects in cells?

COWAN: I'd be very surprised if they were right. I really would be surprised. I mean, they were talking about stuff that usually occurs at very low temperatures and the brain is just too warm. But I think if you turned the thing around and say what insight was it that Penrose had about the nature of mathematical science that would lead him to think something like that… You see, quantum mathematics of quantum mechanics, if you look carefully at it, it really is the mathematics of random walks and probability theory with one little exception that contains the whole difference between the physics of atoms and the physics of Brownian particles. The physics is totally different, but the mathematics, minus a little mathematical transformation is the same. And so, to that extent, if you had a really good mathematical theory for quantal phenomena, and you knew you could transform that theory for quantal phenomena into a really good theory for statistical processes, like Brownian motion and others, then you would indeed have a way to look at the brain. And that's what I've been trying to do for 40 years or so—triggered by Pitts and McCullough—is to use that kind of mathematics to say things about how the brain works.

BINGHAM: So maybe Penrose and Hameroff were actually looking at the wrong end of the spectrum, the wrong scale.

COWAN: That might be a reasonable way to think about their work. So I don't think the original idea is all that bad. I think it's actually good, but I think the physical phenomenon that they're suggesting as being the basis, I think that's wrong.

BINGHAM: So you would look where…Large-scale cooperative activity of the brain?

COWAN: Yes.

BINGHAM: Could you elaborate on that? Because I see you being invited to the next Tucson conference…

COWAN: Yeah, I don't know that I want to go to conferences on consciousness. I went to one in Japan in 1997. Horace Barlow was there and quite a number… Mike Gazzaniga was there, and it was organized by Ito, who was known for many years. It was quite interesting, there was a talk on the cognitive robot that they're building at MIT that…

BINGHAM: Rodney Brooks.

COWAN: Yeah, and the philosopher at Tufts, Dan Dennett, he gave a nice talk on it. I pulled off a great pun at the end of his talk. I said, *you've been talking about cog to an audience organized by Ito*.

BINGHAM: I think I know where you're going with this.

COWAN: I used the Descartesian, "cogito ergo sum". And what was interesting was that all the westerners in the audience laughed, and no Japanese person laughed. And of course, it's a very interesting cultural difference, but I thought it was funny. But I didn't get a lot out of the meeting and I decided that the mathematics and the framework for understanding large-scale nervous activity didn't then exist that would allow one to say things about whatever phenomena lie beyond the obvious things we talk about these days, about… You have networks that have fast long-range connections to other areas of the brain; there are some areas that act like hubs that get lots of inputs, there are other areas that don't. In other words, there might be a scale-free network associated with the large-scale connectivity of the brain, in the sense of the World Wide Web. And that might actually be playing a role in the integration of all experience into a picture that contains everything and that has something like the phenomenology of consciousness. I mean, Terry actually, and a former student of ours in Chicago, Sidney Lehky, wrote a very interesting little paper on a population coding approach to qualia that I thought was quite promising, but I've never really thought hard about it yet. I'm trying very hard to master the mathematical methods required to study what might happen in the cortex as brain states are altered. And once I've got that in hand, and I'm almost there now, then I might be able to make some contributions to the subject, so it'll take a while.

BINGHAM: So why the fascination with hallucinations…

COWAN:
Well hallucinations, so the reason I study hallucinations is because, well there are two ways… several ways of studying the brain. One way is you give a stimulus and you study the response. And Hubel and Wiesel pioneered receptive field measurements, well actually it was Lettivin and Maturana, McCullough and Pitts who wrote this wonderful paper on what the frog's eye tells the frog's brain that triggered a lot of things, but they were to some extent, overshadowed by Hubel and Wiesel's discoveries in the cat. And they built that, their work on the cat, and the monkey visual pathway is wonderful work, but it diverted people thinking about the other aspects of what goes on in the brain.

Ninety percent of all the connections to neurons in the cortex come from other cortical neurons. Only ten percent of the connections come from the outside world. So people have been studying the effect of stimuli from the outside world on the nervous system and they haven't spent much time thinking about what the other ninety percent of the connections are doing. In other words, the internal dynamics of the brain is very important. Now how can you get studying the internal dynamics? Well my insight was, well you can study what happens when the brain state becomes altered, when it goes through the action of hallucinogens or mediation or through the sleep-wake cycle. All kinds of things can cause changes in brain states, and one manifestation of changes in brain states, at least in the visual pathway, is people see things; they have visions. They see fretwork, they see tunnels, funnels, all kinds of imagery, and people have been reporting on this for… ever since reported history and even before. You see it in cave paintings and rock art, and everybody seems to see the same kinds of imagery and it seems to be rather geometric. Well what that told me was that if you see geometric patterns, the architecture of your brain must be reflecting those patterns and therefore must itself be geometric. And it turns out Hubel and Wiesel's discoveries on the architecture of the visual cortex of cats and monkeys and, by inference, humans, are consistent with this point of view. So I regard the study of hallucinations as a way to study the, it's like spectroscopy. You study the emissions of stars to find out how they're composed. You study the patterns that people make themselves in their heads as, and see as hallucinations. That's the output of the brain when it's not stimulated, so they tell you information about the brain. It's a kind of mental spectroscopy.

BINGHAM: So the brain is a pattern generator and a pattern identifier?

COWAN: Yes, it's an identifier because it probably correlates the external stimuli with the patterns it makes internally.

BINGHAM: Does that translate to, I suppose, to pathologies like epilepsy…?

COWAN: Well epilepsy is a very similar dynamical… So hallucinations are a dynamical disease that, if you like, that doesn't really have any bad effects on people, at least we don't think so.

BINGHAM: Or we can get you a job as a shaman.

COWAN: Yeah, but epilepsy is a dynamical disease. What does that mean? It means it's an altered brain state. It is an instability of the brain. But it turns out that all brain states, the normal brain state can go unstable, and very easily because normal brains are kept in balance by the balance between excitatory and inhibitory influences in there, and if those are disturbed somehow then you get altered brain states, and you get switching to new states. These new states can be things like hallucinatory states. They can be epileptic states. They can be sleep states. They can be hypnotic states.

They can be all sorts of states. And the mathematics that I've been trying to develop has been aimed at getting a grip and a handle on all these kinds of things. To think of the brain as something that generates all kinds of states, and they have manifestations for all the things that we do.

BINGHAM: That sort of investigation, I think, is what intrigued Aldous Huxley…

COWAN: Absolutely, Aldous Huxley was very interested in questions like this, as were a lot people, but Aldous Huxley was particularly ahead of his time on this.

BINGHAM: I'm thinking also of, a long time ago, being at John Lilly's house; John Lilly, the researcher working on dolphins, who had also set up the isolation tanks, which I recall Richard Feynman used to do.

COWAN: Yeah that's right; he thought he could have a crack at the nature of memory. Well yeah, he had hobbies, Dick Feynman. Every summer he would do something different. One summer he almost cracked the genetic code, another summer he learned to play the drums, another summer he took up painting, but one summer he decided he was going to work on memory. And his way of doing it was to get into one of these Lilly tanks and just relax and let his mind go and see what would happen. He didn't learn much about memory, but he said he had some very interesting hallucinations while he was in the tank.

BINGHAM: Have you ever tried that?

COWAN: No.

BINGHAM: Just a couple more things and then we'll wrap up. What's the last book you've read, for leisure?

COWAN: The last book I read for leisure was *Lord of the Rings*, which I never read when I was young. And I never read the *Chronicles of Narnia* when I was young, but I've read all the other C.S. Lewis books, but not that one.

BINGHAM: Who would you say is the most interesting person you've ever met?

COWAN: Hmm, very difficult. It has to be Dick Feynman, I think, just because he was so clever, amazingly clever. I mean, I used to meet him in the summers at Aspen in the '70s and he would ask me what I was working on, and within about half a minute he was telling me what I was working on. I mean, I met so many interesting people, but I think Feynman was probably the most interesting. I have great respect for many, many people though. Murray Gell-Mann I think has a fantastic intellect. Freeman Dyson is another one, and Francis. Francis used to come to all my lectures, Francis Crick, and he had this very interesting penetrating eye gaze. You know when you, when I looked at the audience, there would be Francis, looking at me and really with that penetrating gaze. I always said to him… I'd be very careful and tried to explain things as best as possible because I knew that Francis had a very low tolerance for people who obscured things. So he also, I found him to be extremely interesting. Manfred Eigen was another one that I thought was amazing. Norbert Wiener is probably up there too. I mean, they're all fantastic people. I was very fortunate with the many people I met, from Gabor onward.

BINGHAM: Who would you have liked to have met that you didn't meet?

COWAN: I would have loved to have met Von Neumann; I would love to have met Schrödinger. I did meet Heisenberg; I heard him lecture in Edinburgh. I heard lectures by Max Born. I met Dirac. He and I took part in one of these Einstein centenary meetings a few years back before he died. He was very interesting and had a wonderful knack of turning not very pertinent questions… he gave great answers to not very good… I mean one young person asked him what should he work on if he was a beginning graduate student, what problem should he work on? And Dirac thought for a moment and said "*whatever interests you most*". And I thought that was a great answer, and every time a student asked me what they should work on, that's what I'd tell them. Just, do what most interests you and you'll be rewarded, that's what you'll like and work hard at.

BINGHAM: One last question, which is if the theory that you're working on now turns out to be correct, applicable and so on, what would be the consequences of that?

COWAN: Well I might be able to, for example, people measure all sorts of things these days in brain statistics, and they calculate the statistical nature of the spike trends in neurons, the things you record from EEG, what are called evoked potentials. They measure all these brain potentials and they calculate the statistical distributions that they see. There's no overall way to interpret these things and to relate it to simple vault properties of the brain and the states they're in, and I think what I'm doing now might supply a way of answering those questions. The point is the brain is immensely complex; we're only scratching the surface of what it's like. I mean, I think it's going to take… I was once asked how long I think it would take before we understand the brain. I think it's going to take a millennium to understand the brain. I don't think it's anything… I mean we're just barely scratching the surface of it.

BINGHAM: But you still get the impression now from new technology, fMRI and so on, people are now talking about mirror neurons, they're talking about…

COWAN: Oh yeah, there's a host of things, but it's immensely complicated. I mean, I was interviewed by John Horgan a couple of years back before he wrote an article about the Santa Fe Institute, and before he wrote his book about the end of science. I mean, he basically said something similar to what was said at the end of the 19th

century, that all the big questions in physics had been answered and there was not much more to do. And look what happened in the 20th century. I think the same thing will happen in both physics and in biology and… in the 21st century. It will turn out there's all sorts of new phenomena, new questions, new ways of thinking about things. Now I don't think the process will ever stop.

BINGHAM: Jack Cowan, thank you very much.