Oral History Interview with

Don Norman, Ph.D.

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La Jolla, California

Conducted by Jeffrey R. Yost, Ph.D.
Charles Babbage Institute

Abstract: This oral history, with one of Human Computer Interaction's (HCI) and Cognitive Science's foremost pioneers Don Norman, is part of a CBI project done for ACM SIGCHI. It briefly addresses Norman's early life, before focusing on his graduate education and prolific career. This includes Norman's discussion of his mentors and influences, career launch, leadership in Cognitive Science, and his intellectual and organizational contributions to HCI as field and ACM SIGCHI as an organization (the field and SIGCHI's evolution). He recounts his leadership of UCSD's Design Laboratory, his Chairing the UCSD Psychology Dept., educational philosophies, research management, and collaborations (with George Mandler, Danny Bobrow, and many others). He also touches upon his consulting, and his time working in industry (Apple, HP). Finally, the interview offers contexts on some of his principal publications, including his seminal book *The Psychology of Everyday Things* (POET), which along with his other scholarship developed and propelled forward a science of design.

Yost: My name is Jeffrey Yost, I am the director of the Charles Babbage Institute (CBI) at the University of Minnesota, and a historian of Science, and I'm here today on February 12, 2020 with Professor Donald Norman. We're at the UCSD Design Lab that he is director of. Don can you begin by—I understand you were born in 1935--where were you born and where did you grow up?

Norman: First, let me comment that I now try to go under the name Don Norman, trying to change all my publications to that.

Yost: OK.

Norman: Well, yes, I was born in New York, but I don't remember anything about that. My father worked for the US government in the Public Health Service, so, every two years or so, we were transferred, and so when people ask me where I'm from, I say, "I don't know." I lived all over the country, including El Salvador, and when I went to MIT, that was the longest I'd even been in one place: four years.

Yost: Prior to going to MIT, so in elementary, junior high and high school, were there particular subjects you were especially interested in or felt you had a special affinity for?

Norman: Yes, at some point, I discovered electronics. And what fascinated me about electronics was that you couldn't see it, as opposed to all the mechanical devices that existed. There was this wonderful book that I cannot remember the name of anymore, but it was a book written for beginners, and it really did explain basic electronics. It was a practical book, though. It allowed you to build things. So, I became a radio amateur, got a radio amateur's license, learned Morse code and all that—maybe in junior high, certainly in high school. And so I decided to become an electrical engineer because that was the field that did that.

Yost: Can you tell me about your decision to choose MIT?

Norman: Oh, well, my father got transferred to the Woods Hole Oceanographic Institution and so we lived in Falmouth, Massachusetts. And this was roughly 1956. I went to high school there. It was Falmouth High School. It was called Lawrence High School at the time; it's now called Falmouth High School. I learned nothing in that high school. In fact, my father was then transferred to work with the State Department and to go to El Salvador. The whole family left in January to go to El Salvador, so I never finished high school. And they told me, "If you get into college, you'll be one of the first people to go to college, maybe the first, so we'll give you your diploma." And I just applied to a bunch of schools. I was really quite ignorant of them. I was a good friend of a person who live across the street from me who was a scientist at the Woods Hole Oceanographic Institution, which is now owned by MIT, and somehow MIT came in, and I learned about MIT, so I applied to MIT. And I got accepted but I got accepted when I was already living in El Salvador

Yost: So, MIT, was of course the site of Project Whirlwind and was already doing pioneering work in computing by the time you arrived. Had you thought about computers at all when you entered the electrical engineering program?

Norman: No. I wasn't aware of computers.

Yost: OK.

Norman: But it's true. When I was at MIT, I soon learned of these things called analog computers and these things called digital computers. And it's difficult to imagine this today but we—that is the freshmen, and my friends—had great trouble understanding the difference in what they were. And actually, there was a differential analyzer as well, which complicated matters because it was kind of a mix.

Norman: But yes, that was fascinating. So actually, to continue the story, I did get my degree in electrical engineering and there were a couple of EE professors, a man named Ernst Guillemin who had, as far as I can tell, he completely revamped the entire curriculum in electrical engineering and wrote the basic textbook—and of course I took this elementary circuit theory course when I was a sophomore, and all the graduate students and even some of the professors would come and listen to the lectures. Now he was wonderful and inspiring. And then I also took a course from Doc Edgerton—the guy who invented the strobe light. He was the opposite of Guillemin in that he was very practical-minded, but he had a great sense of humor. And he was wonderful too. And so those are the two professors—well there was a physics professor, I remember, but basically those are the ones I remember.

Yost: Did you have any interaction with Robert Fano while you were a student there?

Norman: I think I met him. I had lunch one day with Claude Shannon, information theory Shannon, and I bumped into—like to say—I bumped into Norbert Wiener in the hall, and I kept waiting for the classic story that he had. The story he has was that he bumped into a student in the hall, and excused himself, and then, Norbert said, “Which direction was I going?”, and the student said, “You were going that way, Professor Wiener’, then he said, “Oh. Then I must’ve eaten lunch.” But, no. That didn’t happen to me.

Yost: Interesting [chuckling].

Norman: He taught differential equations, and all my friends would go to his class. But then after his class, they would go to another professor to learn the topic. There’s a million stories about him. But I graduated from EE, and I was offered a job, I think it was Raytheon, in Philadelphia, and I accepted it, and they said, “We’d love for you to get a master’s degree—

Yost: And that would’ve been in 1957?

Norman: Yes. "And we will pay for your master's degree and here's the application to the Moore School [of Electrical Engineering] at [University of] Pennsylvania." I didn't think I'd go on to graduate school. I was interested in building circuits. But, yes, I can see where a master's degree—they will pay for it—probably makes me better, so I applied. And then one day at MIT, I was still at MIT, I get this envelope from the Moore School, not only accepting me, but saying, we'd like to make you an assistant instructor and pay you $7,500 a year and all your tuition. In those days, that was a lot of money.

Yost: Yes.

Norman: I thought about it, and was like, "Oh, well, maybe I should do that and not go to Raytheon." So, I never went to Raytheon. Then I got my master's degree in electrical engineering at the Moore School, but I really wanted to do computers because I really loved making—I wanted to do artificial intelligence although I didn't know that word at that time. I'm not sure that word was yet invented.

Yost: So, John McCarthy came to MIT in '56, a year before you finished.

Norman: Right.

Yost: Did you have an opportunity to meet him?

Norman: No, but later on I became friends with John McCarthy and Marvin Minsky and Seymour Papert, and Allen Newell and Herb Simon and so those are all the key people.

Yost: Right.

Norman: But at the time, I didn't. But here's the story— Brown University, Andy van Dam.

Yost: Of course.

Norman: So, years later, I was going to a meeting in Woods Hole, and we flew into Providence. He was having a get-together at his home, and he and I started talking. And I don't know how we came across it but at one point he said that, “When I graduated high school, I was the first person in my high school class to go to college. And I said, “Oh. That’s interesting. Where did you go to high school?” He said, “Massachusetts,”

“Oh, where?”

“Woods Hole.”

“Well, where?”

“Falmouth.” And I said, “No. I was the first person to go to college.” Because it turns out he was like a year or two behind me. We didn’t know each other.

Yost: Ah, interesting.

Norman: Well, then we said, “Where did you get your PhD? I got my PhD at the Moore School of Electrical Engineering in Computer Science. And I said, “Oh. I was at the Moore school but in fact I wanted to do computer science. And they said, “Well we’re about to start one. If you can only wait a few years, you could be the first student.” But I didn’t feel like waiting a few years. Just then, a new chair of psychology came in. He was a physicist. And they had hired a mathematician, and I heard him give a talk, and so I went and said, “Well, it sounds like you’re doing just what I want to do. I want to build an intelligent system, and so you want to understand intelligent systems.” Anyway, he said, “You don’t know anything at all about psychology—wonderful.” And so, I went to psychology, and got a degree in what was then called mathematical psychology.

Yost: And that was a field really, less than a decade old, some pioneering work by Estes, and Bush and Mosteller.

Norman: Well, Bush was a physicist who came into Penn.

Yost: Ok.

Norman: And Mosteller was still a statistician at Harvard. And the professor that I was assigned to was [R.] Duncan Luce. He won the Presidential Medal of Honor and all that as a mathematical psychologist, and Duncan said I didn't know enough math, so he sent me off to the math department. Now, I had six years of engineering math—more math than any other student there, but that didn't matter. But there was a really pretty impressive group of people who in the late 1950s became mathematical psychologists. My version of this is that many of them decided that they had these powerful tools looking for problems, and it was far better to look for problems and work on the important problems and then use whatever tools were relevant which turned out less and less to be mathematics. So, there were many people like that. Dick Atkinson is an example. I'm an example. None of the other people in the computer field, in HCI, were a part of that, though. But over the years, my work has become less and less mathematical or technical, because I'm working on problems that—I would love to make them more formal, I don't know how. Anyway, so Andy van Dam shared that common background.

Yost: In 1962, you defended your dissertation entitled "Sensory Thresholds and Response Biases in Detection Experiments: A Theoretical and Experimental Analysis." Can you briefly describe your dissertation and the context of this work?

Norman: Yes. So, I was an engineer in psychology and I hated psychology because it was all memorization of experiments and stuff. There was no coherent theme and I was used to engineering where—or science—where you aren't supposed to memorize lots of things. For classical physics, if you knew the equation F equals MA, you can derive anything you needed. That was not how psychology was. But the closest to it was Sensory Psychology, which is a field called Psychophysics, trying to understand—well, the sensory systems and so on. I came across this interesting set of papers out of Harvard—called *The Quantum Theory of Hearing*, which basically said, look, the way the ear works, it's a beautiful mechanism—I won't describe it for this—but my engineering background was perfect to understand both the mechanics that were involved and also the mathematics of it. It actually didn't involve people; it was all the mechanics of how the eyes worked, how the ears worked, how touch sense worked and so on. And therefore, if a single neuro-fiber went, it should make a difference. And so, it should be a quantum. It shouldn't increase continually, it should have tiny, little jumps and you can tell this most if at the very beginning, lower levels. And there was a concept called 'Just noticeable difference'. It came from the German *differenzieren* or however you say that in German. And it means, how much must you adjust a sound, or anything for that matter, how much must you change it so somebody could just notice the difference between the two versions. So JND has become my symbol, it's my website, my license plates, it's my consulting company. The quantum theory of hearing was developed by people like S.S. Stevens, George Miller, and Georg von Békésy and Ulric Neisser. These are all major names in 20th century psychology. In fact, von Békésy got the Nobel Prize in Physiology. And Stevens had written the masterful *Handbook of Experimental Psychology*. George Miller was the guy famous for the magical number 7 +/- 2 and other works. Ulric Neisser coined the term 'cognitive psychology'. So, I was following in good footsteps.

Yost: Yes.

Norman: Using my mathematical psychology, and Duncan Luce had played a little with this—my advisor-- I realized that if people's decision criteria were noisy, because when you listened, it was really hard to hear the difference. When it was a little bit louder, you didn't hear it as louder, you heard it as different, because a tiny, little difference. And so I made a probabilistic model of the threshold where you say, "Yes I hear it" or "I don't", and what that did was predict this really weird function that had a discontinuity and a reversal, and I was actually able to find data that matched it. Actually, the paper I wrote on that was—every summer the mathematical psychologists used to go to Stanford, where we all got together. The most senior and the most junior people were all together, and that was really great—

great experience for everybody, I think. And then I met Dick Atkinson there. He was either a graduate student or a first-year assistant professor, and he was writing, compiling a book of papers, and he published my early paper. So, that was my first publication. (We are now sitting in Atkinson Hall because, although Dick as a psychologist, tried to hire me to go to Stanford, eventually he was head of NSF, and then we hired him here to be the head of UC-San Diego. And then he became the head of the entire UC system. And so now he's back here.) Anyway, that was my thesis.

Yost: Can you talk a little bit about the broader field of psychology and changes that were going on, and how you saw the evolution of the field in your early years in academe?

Norman: So, when I got my PhD, I was asked by Duncan, "So, where do you want to go?" And I said, "I don't know." We talked about it and said, maybe MIT, or maybe Harvard. And he said, "Ok. I'll have you visit those two places and you tell me which you prefer." This was a very different way of getting a job in those days. So I went to MIT and talked to a bunch of people and I went to Harvard and talked to people, and I came back. And I said, "I think MIT is more like what I'm used to, and Harvard is different, and I should go to Harvard." So, I went to Harvard as a post-doc under George Miller. And that was where I started to learn psychology because I discovered the English psychologists, especially Donald Broadbent, Anne Treisman and a few others. They were talking about real phenomena and trying to understand the mechanisms. Whereas American psychologists were governed by the behaviorists.

Yost: B.F. Skinner.

Norman: And they didn't believe in understanding the mechanisms. In fact, I became an instructor at Harvard after my post-doc. They believed in starting you as an instructor, then becoming an assistant professor because that way you could stay at Harvard longer. And Harvard's a good place to be from, but if your PhD is not from Harvard, you're not going to get a position. When I was introduced to the Department of Psychology, by Miller, Skinner stood up and denounced me, my field. But I didn't care. I was an engineer, and I was, understanding, doing mechanisms, and I started writing papers. I'd gotten into an argument with somebody about short-term memory, which they said was problematical and people didn't understand it, and I said, "What do you mean? It's got to be there." Every system requires a memory to buffer itself, et cetera. So, I wrote a paper—I made a mistake in calling it primary memory, not short-term, which was a word taken from William James, but it still was published in *Psychological Review*, which is the best journal in psychology. I was publishing madly, and actually my former professors at Penn wrote to me and said, "You're publishing too much." But they, mind you, were being published in the very best journals; there were papers that I've—I've always believed in trying to understand a phenomenon and trying to look at the whole story. That made me quite unique in psychology—not the only person, but the few of us, George Miller was one, sort of banded together. There was another person named David Green, who was at MIT, who was studying hearing, psychoacoustics, which was what my thesis was. And he too was trying to put together—he helped develop single detection theory in psychology, which was a very important analysis. And I also worked with Nancy Waugh, and Wayne Wickelgren. We wrote many, many papers together on memory, models of memory, with statistical fluctuation based on signal detection theory, and that was really [introducing] engineering into psychology. Then, in 1965 or so, I was a member of a society called Society of Experimenting Psychologists, which was a very elite, specialized East Coast Ivy-league sort of thing—it was supposed to be secret and you had to be invited. When you were at some age, 30-35, you got kicked out, you're too old, and so I was part of that. And David Green was there, and he told me he was thinking of going to this new campus that was just opening at La Jolla. And he said, "Why don't you come join me?" And I said, "Well, I don't know anything about it. Really they haven't asked me." He said, "Let's go together." And that was like a Saturday, and on Monday, I get a phone call inviting me to come visit. There were three critical people—the only three people in the Psychology Department in the first year-- George Mandler, Bill McGill and Norman Anderson. All three had the same focus on the philosophy of information processing. In fact, George was setting up something called the Center for Human Information Processing. He got a grant from the government. He even got money from the government to build a building for it, which is now called Mandler Hall. And Bill McGill was a mathematical psychologist already working on information theory and other aspects in psychology. He eventually became chancellor of this university, and then president of Columbia, and then came back. And so, the main psychology building is now McGill Hall.

Yost: Quite a group, and legacy. Moving back briefly, in the early 1960s, did you know of the early work of J.C.R. Licklider? Was it influential to you at all?

Norman: Oh, I knew Lick. Yes. Yes.

Yost: Ok. Can you talk about him?

Norman: He was psychophysicist. He had done work in psychoacoustics at Harvard, and then he went to ARPA. Not DARPA, but ARPA.

Yost: Right [ARPA renamed DARPA in 1970; founding ARPA IPTO Director in 1962].

Norman: And that actually helped build the whole philosophy of ARPA, was wonderful. It was, "Let's find good people and give them money to do work. I'm not asking what they're doing. If they have a bad year, we don't take their money away, we say, 'You had a bad year.'" In fact, the better you are, the more outrageous your work, and therefore important. The more likely you're to have a bad year or two. So, he helped develop that and was the first person to think of time-sharing computers, and I was part of that early group. I was doing my work at Harvard at Lincoln Labs, doing my work—what was their Lincoln Labs computer called? TX-0 or some such thing.

Yost: Right. TX-0, is that Harvard or MIT?

Norman: Lincoln Labs is MIT. But again, we—there was a group of us that met all the time.

Yost: Right.

Norman: Every week I think we had a meeting in someone's home. It was a group of people from MIT, from Harvard, Lincoln Labs. We didn't distinguish.

Yost: Was MULTICS of any—

Norman: No. MULTICS came later. This was before MULTICS. But this is the origin.

Yost: Rather, CTSS [Compatible Time-Sharing System]?

Norman: CTSS was the origin of MULTICS. Yes.

Yost: Yes.

Norman: And so, the Lincoln Labs computer became eventually that Digital Equipment Corporation PDP-1—that was the model for it. When I was at Harvard, George Miller thought we ought to be using computers to control experiments, and he bought a PDP-4. The first one, I think, ever used in psychology for experiments. I was the person in charge of it. So, I've actually used every Digital, Equipment Computer almost—PDP1, 4, 6, 7, 9, 10, 11, X. It's interesting. Yes, I knew most of those people, and Lick. I wasn't as close to Licklider as the others. He wrote a book about the library, and I wrote a paper in response, building on what he had done. I never followed up on that until actually about 20 years later at UCSD.

Yost: Back in 1959—you earlier mentioned the behaviorists and their growing dominance—Noam Chomsky wrote a very strong critique of the 1957 book *Verbal Behavior* by B.F. Skinner.

Norman: Well, let me tell you that story. At MIT, we had to take these humanities courses which we all hated. But my roommate convinced me that there was this new professor whose course we ought to take, because he's the new guy and he's kind of interesting. And it was Chomsky, who had just arrived at MIT. It was the first course he taught at MIT, it was called "Syntactic Structures," and it used his thesis which we didn't have, but part way through the course, his thesis arrived, and we all read it. I do remember the end of the course, Chomsky said "it is really weird, I taught this course at Penn, and everybody loved the philosophical debates and issues, and they couldn't follow the mathematics. And you guys, you hated the philosophy and couldn't follow it, thought it trivial, but you loved the mathematics." I did become somewhat friendly with Noam over the years because when I went to Harvard, Jerry Brunner and George Miller had started the Center for Cognitive Studies, and I didn't even know what the word 'cognitive' meant. But I remember when I first arrived, I'd gotten into fierce arguments with people on very first day about all sorts of topics. But arguments in the sense that Harvard loves arguments, and so the people that I was arguing with became my collaborators and co-authors. Harvard was a weird place. We'd have visitors come, and at every talk, Jerry Brunner would always be the first to stand up and ask a question, and it was a very wonderful question; phrased and lengthy and would go into the history. And he didn't face the lecturer, he faced the audience, and would ask this wonderful question. And I do remember one of my friends at one point—because all the assistant professors were trying to show how clever they were, he poked me and said, "I can't think of a good question. Can you think of a good one for me?" So, they just loved to rip apart your ideas. In fact, Harvard, Oxford, Cambridge, I've learned, if you give a talk at one of these places, and they don't rip it apart, you gave a really bad talk. So, it was a really invigorating atmosphere, and that's where I started developing. And as I said, I started publishing quite frequently in part because these arguments led me to ideas and we did a lot of experiments, and we were introducing a whole new field. I remember my co-author, Nancy Waugh, we wrote this paper on memory and I drew a block diagram of the memory system. She said, "I don't understand."

"What don't you understand?"

"You say that some of the items are forgotten. So where do they go?" And I said, "What do you mean? They just dissipate, just like if I make a sound, where does sound go when it's over with? It's dissipated into friction, in the air molecules, et cetera." And she just could not understand it, so I had to show an arrow going into a box, call it 'forgotten', because her background was as such she could not understand what information processing was about. My stay at Harvard was wonderful because basically anybody who was anybody would show up and give talks and I met them all—of course this was a time of revolution in psychology. It was starting to become more formalized, more scientific, more based on evidence and moreover, an attempt to understand the underlying mechanisms. So, when I got to UCSD, that was in the back of my mind and it was fascinating, you know, to see both McGill and Mandler. I knew about McGill. I didn't know anything about Mandler. They were already starting the Center for Human Machine Processing and they hired me as an associate professor with tenure. Later on, they told me that was a big mistake—that they had thought I was more senior than I was, and had thought that I wouldn't come unless I was given tenure. And I said, "Well, thank you for that mistake." So I never was an assistant professor, which is a miserable job because you are trying to set up your laboratory, trying to train graduate students, trying to bring in research money, trying to publish, and hoping you'll get promoted. It's the most stressful part of academia. I somehow bypassed that. UCSD was wonderful. We were pursuing models of human memory. I was studying attention and memory and, again, models of them. We hired Dave Rumelhart at one point, and Rumelhart and I became collaborators doing models of memory. Then Peter Lindsay and I decided we would teach an introductory text, an introductory course in psychology, that would be completely different than what anyone had ever taught. We would not give lots and lots of experiments that people would memorize, but that we would give fundamental principles from which you would derive everything. The book was called *Human Information Processing* [*: An Introduction to Psychology*]. It was a great critical success, not so much a financial success—our students loved it. Professors around the world bought the book or got a free copy. We thought—they loved it. But they thought it was too difficult for their students. They were wrong. It was just backwards. It was too difficult for the professors. It did go through two editions but—

Yost: And it was translated into multiple languages like many of your books.

Norman: Lots of languages. But Peter Lindsay, unfortunately—well, first of all, he left UCSD. He took the advance money and decided to go—I took the advance and bought a house. He took the money and d traveled around the world. And when he got to Bangladesh, suddenly it was a big famine and everything was a disaster so he actually took over helping to build the city, in charge of big ship loads that would come in. Anyway, he told me after all that he couldn't go back to being a normal psychologist. He went back to his home in Toronto and did good stuff at OISE [U of Toronto]. But then he had a brain aneurysm, so when it was time to do the second edition, he couldn't do it. I did it all by myself. But I never liked doing revisions. I've done three books, I think, with a second edition. But I would rather write a new book. Once I finish a topic, I'm kind of finished, so I'm ready to go onto the next. But, yes. That book was quite successful in many ways. In fact, I'm still getting royalties.

Yost: In 1974, you became a member of the steering committee of the Society of Laboratory Computers and Psychology. Can you describe that group and your work within it?

Norman: Well it was just like—again—there are a bunch of us younger folks who were much more involved with today's technology, in those days, and information processing and technology, both for our models and also to develop—to help control the experiments so we could do much better control. It was much easier, better than the mechanical stuff. Almost all the work in psychophysics had been done using sound because we had lots of technologies for controlling sound. In fact, I even worked in that area while I was at MIT, both in an audio store, selling high-quality audio equipment, and two summers working for the Woods Hole Oceanographic Institution, studying underwater sound transmissions. We went to Maine, found a quarry filled with water. We lowered explosives into the water, and sensors tracked the sound because it was refracted and showed a sound could be transmitted in the ocean for thousands of miles because it went through this narrow refraction period. It was like a—it was kind of like a fiber optic cable for sound. All those—I also spent a summer working at Remington Rand, Univac [Division], which was, of course a big vacuum tube computer. And I programmed the UNIVAC [Universal Automatic Computer] at Penn when I got there. It had a thousand words of memory, and that was considered a lot in those days. I like to tell people it had roughly a thousand vacuum tubes, and it actually had more. The failure was such that basically every hour some tube would go. And so, when you wanted to use it, it was this big machine that took up a huge room, and you checked it out. It was all your machine for the hour you checked it out. It was interesting.

Yost: At that time, did you think that digital computing would be really central to your research and your career?

Norman: I thought that information processing and computation would be central and it was.

Yost: Ok.

Norman: Still is. But I didn't understand—nobody knew Moore's Law yet or predicted the kind of mass changes, and we'll come back to that later.

Yost: You were chair of the Psychology Department from 1974 to 1978. Can you talk about that role at UCSD?

Norman: Well, the way the departments work, we take turns, and it was kind of my turn. But unfortunately, the Psychology Department, then, was filled with personality clashes. First of all, for reasons unknown to me, George Mandler decided to hire a behaviorist, George Reynolds, who then hired two more. And that split the department. They were nice people, by the way. The issues were not with the people, but it was just contrary. George later on decided that was a mistake. But he liked the people. But they were ripping the department apart, and I took over during all that. I thought I barely got through. When I came back to UCSD 21 years later—when I returned in 2014, Norman Anderson greeted me. He was one of the three founders, the other two had died,—and he gave a talk in which he welcomed me back, and said—I was the one who had joined the department together, cohesion. Boy that wasn't my memory. I thought I had failed. What was more important was not how psychology was developing but that I was getting dissatisfied with psychology because it had a very standard set of experimental methods. I was starting to meet with some people—we called it the Cognitive Social Scientists group. And it was George Mandler, and Jean Mandler and me, and Dave Rumelhart, Aaron Cicourel, Roy D'Andrade and Michael Cole.

Yost: What years we talking here? The mid-1970s?

Norman: Late 1970s.

Yost: In 1979, you, Roger Schank and Allan Collins formed the Cognitive Science Society.

Norman: Well, this is before that.

Yost: Ok.

Norman: So Aaron Cicourel was a very famous sociologist, but not liked by his Sociology Department even though he was the chair for a while. He was an ethnomethodologist, so-called. Roy D'Andrade was a cognitive anthropologist before we had that term. Jean Mandler was a child psychologist, George was a memory expert and emotion specialist. Dave Rumelhart was a wonderful, brilliant mathematical psychologist, cognitive psychologist. He and I worked together for many years. And Michael Cole was a cultural psychologist. He was actually in the department of communication here, but I had known his work before and he had done a lot of work in Africa, for example. All of us thought that the current psychology was wrong, was too narrow. And Aaron Cicourel explained about the white room phenomenon. We study our experiments in the white room where everything is held constant and pure, and we only vary one thing at a time, and we study undergraduates-- why do we think those results are meaningful? And he wanted us to be out in the real world. In fact, the other person who wasn't part of our group because he was a graduate student at the time was Ed Hutchins, who eventually wrote a book called *Cognition in the Wild*. Ed was a student of Roy and Aaron, and we later hired him in cognitive science. That reflects that whole philosophy: we should be studying people out there in the real world, which is very different than in the lab. That broadened my whole background, and caused me to think that psychology was too narrow, and we needed to expand it. I was friends with Roger Schank, and Alan Collins, and I already knew Simon and Newell. And Minsky and Papert. Papert was more influential to me than Minsky. Papert was a mathematician but he had worked with Piaget for many years and was very much interested in how people learn. He wasn't using normal psychology—it was all about motivation and making it a coherent system, et cetera. He invented a programing language called Logo. At some point the Sloan Foundation decided—I think it was because of George Miller, that they were going to support work in what they called 'cognitive science'. I think they called it cognitive science. And we got one of their early grants but it was because of this cognitive science, this group—as I'm concerned that it broadened my horizons and made me dissatisfied with psychology, saying I want to bring in neuroscience, and I want to bring in linguistics. I want to bring in computer science, especially AI. What today we call good old-fashioned AI. I was working more and more with people across all these different disciplines. I don't remember the exact details, but a one point with the Sloan money, we said, "Why don't we hold a conference here in San Diego?" That was the first—retroactively—we called it the first meeting of the Cognitive Science Society.

Yost: Yes. I think that was 1979. Were there fundamental debates or major questions that dominated those early meetings that you recall?

Norman: Well, there were fights and arguments all the time, especially with Roger who was a firm believer in whatever he was saying at the moment. But they were the positive kinds of arguments and fights. That is they made you rethink your ideas and change them, and I've always believed in changing my mind when I learn new things. People sometimes accuse me of changing my mind too often, and I say, "No. We're learning more. That's the whole point." When I give a lecture, and people tell me it's wonderful, well, that's nice to hear. But if someone says I'm wrong and they are intelligent about it, that's where I learn something. I learn one of two things: either that I didn't explain myself well, "That's good to know." Or that I was wrong. Well, that's also good to know. I'll change what I'm saying. So that's my only memory of that. But I think it was a pretty positive event.

Yost: At that time or before that time, had you paid much attention to human factors and ergonomics or did that come later?

Norman: Well, I'm not any good at dates, but at some point, the Three Mile Island accident occurred [meltdown March 28, 1979], so that was a nuclear power problem. I wrote a paper on human error and this is how I started the paper. I was with my friend, George Mandler, at his home. George told me he went to the cupboard to pour himself a drink, a bottle of scotch in a glass, and he poured the scotch into the glass and then he put the glass back into the cupboard and walked away with the bottle. And he said, "I guess I'm getting old." I said, "No. I think that's the sort of thing that happens to everybody." And so I decided I would study error—slips, where you intend to do one thing, and you do something different. But your intention was the correct one. You just did something different. And so what I did was I enlisted everybody I could find, and said, "I want you to tell me about any slip you make, any error you make, but I want you write it down immediately afterwards, and I want you to try to tell me, first of all what happened, second of all, how did you discover it?" I didn't care about what their theory was about why they made it, I just wanted to know what it was and how they discovered that it was wrong. And I soon had like a thousand of these slips. I don't ski. That's another long story but not relevant. But our graduate students loved to ski. So, I would go on trips with them and while they went off skiing I would take all these pieces of paper and spread them out on the floor to make coherent sense of it. I wrote a paper for *Psychological Review* on slips; slips of action, not slips of the tongue. Lots of people study slips of the tongue, mine are slips of action. Of course, Freud was in the background, who had done some of this work.

Yost: Right.

Norman: And I started off with that story about this, and I sent it off to the editor of *Psychological Review* and it came back the next day, and the review of it was, "Come on, Don." So, it was too informal for *Psychological Review*. So, I took out the story, tightened up the language and sent it back, and it got published without any revision, and in the first article, in one of the issues. I argued it's information processing, that a lot of slips are just a one-bit change. If you do the negative of what you wanted to do, it's often just one bit difference and I describe things like description errors that, which is what George had suffered from, I felt, is that the description was to move the object and the object had a rough description, but actually the bottle fit it—everything fit. And it's like when sometimes you pour—people often make mistakes pouring something into the wrong vessel and all sorts of mistakes that are description errors. Anyway, I was able to classify the causes of errors. And one that still stands, I think. It's been less valuable though, it's because I think a theoretical description, but if your job is to try to eliminate errors or minimize errors or whatever, it's less valuable. Although description ones are probably the most common. The other one is a capture error. Where you're doing something over and over again, like I go to work every day, and now Saturday arises, and I wake up and—Oh! I'm late!—and I rush off to the car and start driving to work. It's because you're captured and there are lots of those errors. I wrote a paper later on with one of my students, Abi Sellen, and she'll come back in HCI. Her stepfather was John Senders who actually was part of the Lincoln Labs group that I used to meet with. Famous human factors guy. We wrote a paper showing that errors of this sort, like capture errors, the more expert you are, the more likely you are to do it. Whereas with other kinds of errors, it was the less expert you were, the more likely you were to do it. So, there were differences in expertise, et cetera. Anyway, trying to figure out the mechanisms for all this, and then I got invited by the Nuclear Regulatory Commission to be on a study group to understand why at the Three Mile Island nuclear accident, the operators had made so many errors. It was a group of human factors people. Recently, somebody just wrote a book about human computer interaction and a little bit about the history, and I play a big role in that, and I'm very embarrassed by that book because a lot of it is wrong. As far as I was concerned, I was this junior member of this group, and I learned a tremendous amount by being with the group.

Yost: Are there certain individuals that stand out in that group that influenced you?

Norman: No. I've lost all their names, and I've lost the report. I've been trying desperately to try to find it again or remember who the people were because that was so many years ago and I didn't have much interaction with them afterwards. We decided that, and this is the important sentence that links my previous career with the HCI career, that the operators were very intelligent. They did very sensible things. They did what anybody should have done, given the information they had, but if you wanted to design a plant to cause errors, you could not have done a better job. I don't want to go through all the details, but it made me say, "Oh. My background in psychology and human behavior, and my background in technology seems perfect to try to work on these issues." I was not interested in the Human Factors Society. I think I was a member, but they tended to do small, little studies. And I thought they just weren't interesting. Most of science is that, by the way. I have a theory that in any area of science, there are only four or five good, deep thinkers and then the field splits and there's a new specialty. But most people just repeat previous experiments, try to see the flaws, try to do little things that in the end don't matter. That's why I gave up with neuro-quantum theory. It's really neat—kind of neat, but in the end, does it make any difference? No. It doesn't. That is when I started to work with NASA, NASA AMES, the one in Mountain View, on aviation safety. And there I met a whole other bunch of human factors experts, and I learned a tremendous amount. But I also brought in a slightly different viewpoint to them, which was now emerging as a cognitive viewpoint. In fact, I called what I was doing cognitive engineering. It was a term I invented earlier on, which is now a popular term, especially in Europe. And trying to understand the things that gave rise to errors in aviation. Aviation is one of the best studied because pilots now voluntarily report their errors, and that's such a valuable source of information. NASA pioneered in that and made it possible to report without being punished. In fact, it's a quasi-anonymous reporting. You report with your name, address, contact information. People there, ex-pilots, retired, read them. If they don't understand, they call you up and interrogate you until they understand it very well, and then they rip off your identification information and mail it back to you. And if the FAA ever discovers you taxied off the runway, which is an offense and you show them, "See? I reported it." and so on. Exempt. But if some court of law wants to get after you, then NASA would say, "We don't know who wrote it," because we destroyed all the identification information and sent it back. I'm trying to get that into medicine as well, and it's really hard. But that's when I did the switch. When I started using the early computers, we were using the UNIX operating system. I was making so many errors with it, even though I loved the system, it was just so incoherent, that I wrote this article, "The Trouble with UNIX" [1981] and I wrote it on PDP-11 or whatever we had in those days. We were one of the first few people on the ARPANET. I think UCLA or something was on the ARPANET, and we had a phone line down to us, so we were on it too. And I was doing work with my colleagues at MIT and all that. I don't know how but somebody found the manuscript and sent it off to people at Bell Labs over the internet, and I started receiving—we didn't call it email, I don't think, but messages.

Yost: Right

Norman: When I printed them out, there were 30 pages of nasty comments about how I didn't understand and how incompetent I was. One of my former students—one of my students got a job at Bell Labs, and he was trying to convince them I wasn't as bad as they thought. So, they invited me to come out and give a talk to them. They discovered I wasn't as bad as they thought. And a lot of the problems where I claim—programmers love to do the problem, so they hate to do the routine stuff like the argument, how you write—how you handle the arguments and procedure, and they hate that. And so, they would want everybody to do it quickly, and every time, they would do it differently. That's what led to the lack of coherence and also, they didn't understand people. So, for a while, when you used their text editor, you worked for hours, and then you went home saying, "I finally finished," and you came back the next day and it wasn't there. Well, you'd forgotten to save it. The way they solved that problem was they wrote in the manual: Many people make this error, but if you really want to keep it, you have to remember to save it. In other words, there's a joke, which is, if it's in the manual, it's not a bug, it's a feature. But they changed that eventually. That in my day, in my opinion was—I think it was the aviation safety work and the work I wrote, the paper I wrote on UNIX, which became famous, and working on the early computers, and I started to teach a course, which I called "Cognitive Engineering" at UCSD. My research group then—we got some money from the—not the Rand Corporation, but System Development Foundation.

Yost: Corporation or Foundation?

Norman: Foundation. The System Development Corporation was shut down and so they were required by their by-laws to turn the money into a foundation, and I got money from the foundation and we used the money—Dave Rumelhart and I split our interests. In the early days of the Sloan Foundation, we brought in a whole bunch of fellows, and one of the fellows was a guy named Geoff Hinton. I remember I was giving—this is my memory. I was giving a lecture about perceptrons in the early days of simple, one-layer neural network, explaining its problems and so on. And Geoff interrupted me and said I was all wrong. He went up to the blackboard-- we had blackboards in those days—and explained all the work going on in England using energy physics. That was interesting, so we ran a whole bunch of seminars and brought in people to look at what was going on. Then Hinton and a few other people started to develop what was called the ‘hidden layer’ in neural networks. It was a three-level network, and suddenly it was far more powerful, and the [Marvin] Minsky and [Seymour] Papert book, which showed perceptrons were fundamentally limited, was no longer true because this was the more expanded network. Rosenblatt who did the perceptron paper could never have done this because the computers were too weak. We could do it. Paul Smolensky, James McClelland, Dave Rumelhart—a few others, really pushed that. But I was no longer interested in the deep mathematics, I was more interested in practical, applied work. So, we sort of split. I did what became human computer interaction, and he did what became neural networks. Dave eventually became ill and had a brain tumor and died, but Geoff Hinton eventually ended up inventing ‘deep learning’. He now works for Google. His company was bought. He lives in Toronto. And when I asked him what was the breakthrough that allowed you to do deep learning, he said nothing. The breakthrough is that computers are a thousand times more powerful.

Yost: Were you at—

Norman: But all these are coming together.

Yost: Did you attend 1982 Gaithersburg, that meeting?

Norman: Yes, so I went to the Gaithersburg meeting. In fact, I gave a keynote.

Yost: Ok.

Norman: Didn’t I give an opening keynote? If it wasn’t Gaithersburg, it was the one after, which was—my memory is that it was not in Gaithersburg, it was in Boston.

Yost: Ok. Yes. Boston was the first meeting of SIGCHI. And, the year before, Gaithersburg, one evening, a small group conceived of SIGCHI.

Norman: Yes. I was a part of that group.

Yost: Ok.

Norman: And I thought it was a serious error to put it in ACM, because I said, "Look, this should not be in a computer science society because it was more behavioral science than computer science, maybe it should be in a cognitive science society, which combines the two. Or maybe its own society." I still hate the name ACM, Association for Computing Machinery, association not for the people but for the machines. But I lost. Ben Schneiderman pushed it through the other way. I've known Ben for many, many years but he's interesting to work with. Very bright, very good. And I don't know whether that was a mistake or not because ACM was far more powerful than any—there were no really decent design organizations, there still isn't—for academics in deep theoretical design, and the psychology organization wouldn't have understood it either, and cognitive science was just barely beginning, and ACM was powerful. In fact, one of things that happened was now human computer interaction courses have a set of standard curricula, and this plays a big role in their standard curriculum. It's one of the required courses. It isn't one of the options. But I still think it was wrong. I still wish it were different. As a quick aside, but relevant, I'm a member of the National Academy of Engineering. It has a bunch of different sections, Section Five is Computer Science, and Section Eight is, I don't know, Control Theory, or something else. But it's also where industry leaders get appointed, and that's where Human Factors is. So HCI is in Section Five, Human Factors in Section Eight. I was nominated in Section Eight several times, and never made it. And when they nominated me in Section Five, I made it immediately, and all my friends said, "What took you so long?" It was all secret, you don't know you're being nominated, and I was told afterwards. And the problem isn't in the Section Eight, isn't that the people in Section Eight in human factors didn't like me, it was that they are such a tiny minority, and as I tell people if you have a choice between Don Norman and Bill Gates, who would you vote for? Because there were CEOs of companies, then these little human factors people that were not doing mathematical stuff which is what engineering is all about. I still think that's a bad split. I've been trying to convince the National

Academy that we should have our own little division. So, anyway I lost that battle, but indeed, I did give one of the keynotes at the first SIGCHI conference which was in Boston.

Yost: What about the name? Was there debate on it—it creates a pronounceable term to have CHI, but it's putting computers before human. Was that discussed or debated?

Norman: Well I think—I don't know—we did talk about that in CHI. It seemed like a reasonable—in fact, calling it CHI, rather than HCI is actually—like all of design, there's a tradeoff here between ease of use versus, say, functionality and logical consistency. You could argue HCI is better; it puts people first. But CHI is more pronounceable.

Yost: Right.

Norman: So, CHI won, and I don't think that was—I do vaguely recall the discussion, but I don't recall any fights about it.

Yost: You've talked about a lot of people you collaborated with and early influences, I'm wondering—

Norman: Well, there's another story, let me—before I forget it—

Yost: Ok.

Norman: Then I'll come back to that. Because of the work I was doing at NASA, and then the impact I had on the trouble I had with the UNIX article and then the Apple 2 was starting to become popular, and I said, "Why would I ever use an Apple 2? It's a step far backwards from what I'm using." Then the first Macintosh came out. I invited the people from the Macintosh group to come down and give a talk to us. And to my great surprise, some of them told me they were my students. They had taken my Cognitive Engineering class. I wasn't aware of that. But one of my first PhDs was working at Apple. And had a job at Apple and a lot of people I knew were at Apple. So that is in part is what made me say fondly I'm going to leave academia, I'm getting old. I'm going to leave academia while I'm still young enough to do something different. That was in 1993. There was a new company starting up called Interval Research, and they offered me a job to sort of be second in command and help run it. And then my friend said, "Well, don't take the first job you're offered." So, I called up my friend David Nagel at Apple. He said, "Sure, we'll make you an Apple fellow; you can do anything you want; and it's equivalent to a VP." So, I went up and spent a couple days each with the companies. I decided Interval was just like running graduate students. They were all bright and fine people, but I would have to start training them all over again and Apple—when I walked into Apple, and I was a real world expert in psychoacoustics and one of the guys there told me everything I knew was wrong, which is what I like to hear, of course. But I learned from Georg von Békésy. He said, "Yeah, well von Békésy was wrong." But he gave the reason. He was trying to understand how the ear worked, and the only thing he could do was get corpses. And by the time he'd dissect them on go on in, it had hardened, and it was different results. Today we know how to get the same information differently, which was interesting. Because the inner ear is inside a lot of bone area, so it's really hard to get into. As opposed to the eye where you can get right at it. Anyway, so I went to Apple. There was something else that—oh, yes. But before I went to Apple, first of all, the group that I'd done with this System Development Foundation, we produced a book of our work, which we called UCSD: "User Centered Systems Design," which I think was the first use of the word 'user-centered'. After that, I had started the Cognitive Science Department. I took a sabbatical and I decided I wanted—I was chair...false. I was Chair of Psychology, and I was stepping down and I decided, I wanted to get far away from Psychology, from UCSD, not for any dislike but I knew if I was nearby, and there was a problem coming up, they would keep calling me. It would always be an emergency. So, I decided I would take a sabbatical but—there were two places I want to go to: the Applied Psychology Unit in England, Cambridge, and MCC, the new thing that was run by a former admiral that brought in companies from around the world.

Yost: Microelectronics and Computer Technology Corporation.

Norman: Yes, and so I decided I would go first to Cambridge, England to be further away, then to Austin, Texas. So, I went to Cambridge in the winter and Austin in the summer, which people thought it was bizarre. But it worked. I had no plans, but I was so frustrated. I was in the Applied Psychology Unit, which is one of world's best human factors units, and nobody could work anything. And the doors and light switches, and everything was crazy, and I complained! I was told, "Yeah, it's frustrating."

"It doesn't even follow your own principles!"

"Yeah, but, you know, we can't work with, you know, we have the staff. We'd upset the staff if we told them it wasn't right." Because in those days, academics did academic stuff and the British, they would never get their hands dirty and do anything. So, I ended up out of frustration writing The Design—*The Psychology of Everyday Things*.

Yost: Before we get to that, I want to ask you a question about the early CHI meetings. Jonathan Grudin wrote a book on the history of HCI and—

Norman: Where was CHI relevant to this? Was CHI already in existence? I was in England in 1980—

Yost: So the first meeting was '83 in Boston.

Norman: Ok, so I was in England in '86 or so.

Yost: Ok. And he put things in a framework of, that Licklider's, had with interaction and symbiosis and a future of ultra-intelligent machines but he made the point that at these early CHI meetings, the work of Licklider, Sutherland, Alan Kay, Douglas Engelbart, isn't cited. Did you—you obviously were aware of Licklider's work, but these other individuals, were you aware of their work, and did they have an influence on you, the graphics people and Douglas Engelbart at SRI?

Norman: Andy van Dam is in there too. Yes.

Yost: Yes, of course. Andy van Dam also.

Norman: Well, I wrote an early paper. In fact, I think it was for the journal that—those of us using computers—you named the society, I forgot it already. I wrote a paper which was heavily, which was really all about Alan Kay's work. That he gave me a picture of a Dynabook, which I put in there. But it wasn't practical—yet. In fact, he never built it. It was always a concept. Licklider's stuff—Licklider himself didn't really do any fundamental work. What he did was he helped establish it and pay for it and so on. He was—

Yost: Set an agenda for and supported others.

Norman: Yes. Who else did you mention?

Yost: Douglas Engelbart.

Norman: Doug Engelbart's work. Actually, I think the work that was more important than Doug Engelbart's was Ivan Sutherland's, where he did some of the very first graphics stuff at Lincoln Labs before he moved off to Utah. Engelbart was an anomaly. Those of us who knew about it, we weren't aware of the whole program, we were aware of the masterful demo that he gave.

Yost: The famous '68 demo.

Norman: Yes. When I took a different sabbatical, was a year I spent at Stanford where I wrote the book *Explorations in Cognition*. And I forget that was earlier then, before POET [*The Psychology of Everyday Things*] and so on. That was with Norman, Rumelhart—and the research group and all my graduate students, plus a few others. We had Bill Buxton in there. I discovered him at an early CHI conference. This crazy guy giving a talk—and wow! That's brilliant! So, brought him in. Steve Draper was a collaborator. He was part of a Sloan group. So this is the tail-end of the Sloan group. That was the UCSD work, the book. Yes. I guess they weren't a part of the published literature, most of them, trying to figure out why we didn't refer to them. They were very, very important, very influential. But there wasn't—cybernetics is today—actually, an interesting gap was we never used control theory, never talked about feedback loops and so on and that's—even though that was my training. And that's come back in now. That's becoming very, very important. And we didn't talk about cybernetics either. In some sense the current work is more influenced by the work of cybernetics, that Norbert Wiener, than by von Neumann, which was more pure theoretical, clean. But I think the von Neumann's approach was more dominant in those early days of CHI. And I think that Engelbart was kind of a—what did we call them? There was a wonderful paper written about—'Neats and Scruffies'. And the 'Neats' were the von Neumann-type and also the guy that did vision at MIT, David Marr. He was all about theory and making things clean and elegant, and the Scruffies were, "Well. This is the real world that isn't neat and elegant." But that was an interesting philosophical divide. I was kind of a scruffy.

Yost: Intriguing. What about the work at Xerox PARC in the 70s, the ALTO—

Norman: Right. I was going to comment, when I wrote the book *Explorations in Cognition*, I went to the Stanford Center for Advanced Study, but there I went to Xerox PARC almost every day. Actually, I had been a consultant at both Apple and working for Alan Kay and at Xerox working for Danny Bobrow. And Danny and I became friends, well, we were friends already. We had this weird discussion in which, "The first time I saw you, Danny, was when you visited my home a few years ago on your way to take a job at Xerox." Then one day I'm telling him, when I was at Harvard, I took this course in natural language processing—[Anthony] Oettinger was teaching it. It's a summer course and I took it. And Danny, said, "Oh. I gave one of the lectures, so we must've met then."

"No. No you didn't. I don't remember that." But then I found my old notes, and there was not only his lecture, but I scribbled comments all over it. So, Danny and I became good friends. We wrote many, many papers together, and in fact, we wrote them on the Bravo computer. And Alan Kay was there, but more importantly for this, Stu Card and Tom Moran and then periodically, Allan Newell would come and visit. So, we did work together there, starting that year but then continuing. I didn't like the work, though. To me, it was really good modeling, and I liked it and I enjoyed it, but it was too low-level. I felt it wasn't the level that I cared about. That it was making sure the plumbing details were correct, and I was more interested in the higher-level strategic issues. So, the GOMS [Goals, Operators, Methods, and Selection] model which they developed, which has been very popular—I don't really know of any place where it has been important. Now Judy Olson worked at Bell Labs, and she said she saved Bell Labs millions and millions of dollars by applying it. But that's operators who are doing the same tedious thing, and so if you shave a second off each operation, you have millions of them, and you save a lot of money. Aside from that—but I'm the only outcast of the GOMS group. They say they did wonderful work, and I say a lot of the stuff—Stu Card characterizes it as time and motions studies; it came from the early days of human factors. But that doesn't change the fact that—well, if I go to a CHI conference, and I hear that Stu Card is going to give a talk, I will cancel everything in order to go hear that talk. He's brilliant. He's wonderful and a good friend. He, Terry Winograd, Danny Bobrow, Card, Moran. Yeah, they were all influential. We all worked together.

Yost: I wrote a book on the history of IT services, including IT consulting, and I found it very interesting what you had to say about what consultants should do is solve a different question than what is asked of them. Did that come to you immediately in the start of your consulting career or was that something you learned over time?

Norman: It did come to me immediately, but I didn't know it. If we now follow a long history and trend, we get to make the big jump, and I don't even know what year this is when the design thinking craze occurred because David Kelley at IDEO and Stanford develops this design thinking notion, which my first thought was how brilliant because it allows you for the first—so we haven't even covered design yet—allows you to say design isn't just making something pretty, it's much more than, deeper than that. But then I also wrote a negative paper about it saying there is nothing in design thinking that isn't what great thinkers have always done. Doesn't matter if you're a writer, or a scientist or whatever. But I've since come back to saying, well, there is something special about what designers are doing, but it also helps, it's good PR. The word 'design thinking' had been around since the middle of the 1900s. IDEO simply popularized it and trivialized it, unfortunately. But starting with the work I did with the *User Centered System Design* book I moved more and more into design, and when I got to Apple, I actually for the first time worked with real designers. Then Danny and I became friends at the design group in general and I learned more about them. So, I was putting things together, and I finally decided there are four fundamental principles of human centered design. I can give them to you later but for the moment, one of them is solve the right problem. Don't solve the symptom. As I started laying out these four principles, I realized, "Oh. That's what I've always been doing. Don't solve the problem you've been given, figure out what the real issues are." But it didn't crystalize in my mind until 10 years later. Which is, by the way, the way I work. I just read a book on writing. The guy said that there is no correct way to write something. If you take a look at good, successful writers almost anything you can image they do, and they're all contradicting each other but there are kind of two classes. One of them is planners and the other is basically people who have no idea what they are doing but they just do it. The planners like to plan everything in great detail and a lot of them also like to write once, but they may take a long time to write each sentence. They try to make it correct. And the others like to write fast, and they often don't know what they are writing. In fact, I refuse to sign a book contract beforehand because—I tried that once, and it was a mistake—because that isn't what I wrote. And so, what I do is write my book and then give it to my agent and she shops it around. And when I wrote the *The Design of Everyday Things,* or *The Psychology of Everyday Things*, I wrote it in like two months. My term at Cambridge was ending and I wanted to get it out before I left and so I just sat at home and wrote it. Poof! I then spent two years re-writing it. And that's what happens; if you plan, you might spend two years planning it, then you can write it just once. But if you do it the other way, you might be able to write it in a few months, but then you have to rewrite it and rewrite it. But I also tell people when I say I write it in a month, that's false because I spent a couple of years thinking about the issues, and I can't write it in a month until it is in my mind, until I have it put together. But I have this philosophy that I never work on anything that I understand, and therefore most of the time I have no idea what I'm doing or why or where it's going. And over time, I'm able to then step back and put it into a coherent story and make sense of it and now I fully understand it. I often do that by giving lots of lectures along the way, and when I'm teaching classes, and then I write a book that clarifies it all and—oh! Ok. I finished it and understand it, now let me go to some other topic that I don't know anything about. So, when you asked me when I came up with this wonderful philosophy, well, it was years after I had been doing it and didn't realize it but I step back and think, "Oh, yeah." So, in my attempt to write down what I think are the four fundamental principles, it's what made me put together a lot of the work I've done. And I still believe in those four fundamental principles. Here's how you do design-- first, you have to understand the problem deeply, and go and study and do ethnography and then you had to do some ideation thinking, and then you try out, you do prototyping then you think about it, don't think it is a system, and do prototyping and testing, and then refine, et cetera. That's a very logical way of doing it, and now I think that's not right. It's not wrong, but I started realizing that many people, including me—the first thing I do is build something, which violates that sequence. But you build it not with the intention this is the answer as engineering builds it and thinks the answer, no, no.

Yost: As a learning process.

Norman: As a learning process. I throw it out into the world, and I learn so much more by the way people interact than by trying to study them beforehand. And so, then I can throw it away and rethink, kind of that's how I live my life.

Yost: When you set out to write POET, what became retitled—

Norman: DOET.

Yost: Yes, became *The Design of Everyday Things*. Well first, did that acronym, POET, come to you at that same time as the title, before, or after? But also—I'm wondering about who you were thinking of as your audience, it's used in courses, but were you thinking this will be more broadly influential?

Norman: Well it's interesting because I argue you should always understand your audience, and anybody who teaches writing says know who you are writing for and I fail at that. The acronym came quick because I remember I was reading some book on design—can't remember the name, which person it was—and it gave some story about materials that were being used and basically it was about graffiti and that people would deface the materials and so that you had to invent a material that you couldn't write on and so on. And the person said something like, "Maybe there ought to be a psychology of materials," And that's where it came to me. And, by the way, another important person was [James J.] Gibson. He used to come to San Diego for summers, and we used to fight a lot because he didn't believe that the mind was doing anything. He believed in information pick-up, it's just automatic. We fought and we met with each other a lot, and we drank a lot, and argued a lot. But, again, we loved to do that. I thought he was a brilliant observer of what was going on and not a theorist. And I was a theorist. But when I was writing *The Design of Everyday Things*, I started off thinking about how difficult it is to use so many things, and I looked at all the problems people have. But then I reversed it and said, "Wait a minute. We always go to a new place, and there's always new things, and mostly, we know how to work them. And so how is it that we know how to do and work and live in the environment when we are always encountering new things?" And that led me to realize that Gibson's work on affordances was highly relevant. I wrote it up and I refined what I've said, so I've been accused of changing my mind about affordances, and I don't care. Yeah. Because I use the word ‘affordances’, but Gibson didn’t distinguish between the real affordance and what was visible. And I didn’t either. Although I think I called them ‘visible affordances’. Later on, I decided, in fact, that the next revision in the work before that, to say, “No. The affordance is what’s possible. Whether you know about it or not is not relevant. But what makes it possible is perceptible about it. And I decided I would call that the signal, or borrow from, whatever the relevant field is—I would call it the signifier. The field of semiotics. But so, he played a big role too. He was fun. He once agreed with me! And his wife got mad at him and said, “What are you doing?!” But he used to be a Gestalt psychologist, and I think that’s what was happening, is he was revolting against the empty theorizing of the Gestalt psychologist, to say the phenomena they studied were very important.

Yost: You were vice chair of SIGCHI from 1985-87. Were there certain directions you wanted SIGCHI to go and how did it evolve in its first decade?

Norman: Yes, well, I didn’t like the direction it was moving, and a few of us tried to change it and so I made myself unpopular by forcing a change, actually. And the other change that I wanted to happen was to bring design into the story, and—can I pause and give you a different story for the moment and come back?

Yost: Sure.

Norman: I just was writing about this paper, which I think is going to be a very important paper with Michael Meyer who’s a designer here, one of my faculty, called “Why Design Education Must Change.” Because mainly it’s a bunch of crafts people who do beautiful, wonderful work but they—a lot of them don’t understand business, don’t understand the world. You go to design school. All you learn about is materials and drawing and mechanisms and stuff. Today a lot of designers like me—I don’t design beautiful things—I’m not any good at it. But I may design a new procedure for doing—admitting patients to a hospital, or even for how you actually do the work in the operating room. I have a project now on sounds in the operating room which are incredibly horrible and disturbing and annoying, and actually, they make patients worse, not better and so on. But those are what good designers do—so we need different kinds of designers with broader perspective, et cetera. If you take a look, though, at modern design, what was called 'interaction design', or for that matter, the field that's called HCI, where did it come from? It turns out it came from two different sources, neither of which knew the other was doing it. One of them came from psychologists, cognitive scientists, and computer scientists, banding together. And their goal was to make these systems more understandable and easier to use. That was the foundation of HCI, of CHI. Notice there are no designers in CHI. Well, there may have been one—in the early days, the first set. There is one graphic designer. His name I don't remember but I know him well! If I looked at the early members, I would say, "Yeah! That was our one designer." The other, though—there're two books that annoyed me, both written by friends of mine. One was a book, I forget what it was called, but it was written by—there are three people who started IDEO: David Kelley, and the second one is a British guy, and that's who I'm trying to think of the name. There's a book about interaction design; a big, thick book, and it's all about designers. They don't talk about HCI even though he was in Silicon Valley, the heart. He did talk about two people; the Nielsen Norman group, so I'm in there, but it's Bill…Bill [Moggridge]. Because the design community was called in. So Engelbart invented the mouse, Xerox PARC. One of them, I can't remember which one—one of the guys who was part of Engelbart's team who's now at PARC, and they were building the Bravo and so he built a mouse for the BRAVO. So, there are a lot of designers who got involved early on, and they think they invented interaction design, and we think we invented interaction design. And what they did, though, they brought a sense of aesthetics and beauty and style and fun and playfulness. I pointed out in the paper I just finished writing, that one of the critical books that was actually read by people on both sides, but they didn't realize that, was written by the Disney designers, about animation. It's a coffee table book but brilliant and wonderful. They showed how the principles they used in developing the early animation characters and that you, when you wanted to run, you spun your feet. When you back up, they went forward and when you went off the cliff, you didn't fall until you look down and realize you are off the cliff, then you fell off. And so, if you look at the early Macintosh, which copied a lot of that, when you opened a file, you could see it come open and you knew where it came from. There was a trash can that expanded. This wasn't part of the Xerox PARC stuff. This came out of the Apple stuff where they brought designers in. Where there are these two separate things, now today on one team, we're more and more together. One of the things I wanted with CHI, I wanted to bring in more design. And it was kind of a losing battle for a while. But today, there are more and more designers. But the problem is that CHI is still an academic organization, even though they claim they have lots of people from industry—no—because they don't have the people who build and ship the products, they have the people in research labs. And research lab people are more like university people than they are like the other people in their own company. Because I ran the Apple research labs, which used to be called the Advanced Technology Group, and let me tell you my people had no understanding of the business model at Apple.

Yost: One point that Susan Dray made when I interviewed her a couple weeks ago was that in the first couple years, there were a number of human factors people in SIGCHI, a lot of practitioners who were doing work in industry and for government taking part, but very quickly, with the drive for lower acceptance rate, it became far more academic. That this happened probably within the first several years, if not that, definitely by first-half decade, there weren't the practitioners anymore. But there weren't the people in product design.

Norman: Because when they write papers, they get rejected by the academics.

Yost: Yes. And obviously they get frustrated when that happens.

Norman: And the designers, the same way. And the designers don't even know how to write papers.

Yost: Yes.

Norman: You go to a design conference and they say, "See this slide? I built that. Isn't it wonderful? And I built this! And then I did this!" I did keep wanting to say, "I did this, and it was horrible! And here's what I learned." But they would never do that. So, there is that split. It's coming together and the only way to get good design work in is you have to have a referee by designers who say, "This is significant, not that it's beautiful, but that it's significant and we can build on it and learn from it." But the designers don't know how to do that. So, part of the reason I wrote these design education papers, I wanted to change how we educate designers—we're doing a special issue of a journal on this and IBM Design liked it. I said this will take a two or three-year process to rethink the whole curriculums of all design schools across the world, and IBM Design said, "We'll sponsor that." So, it's now co-sponsored by our Design Lab and IBM Design in Austin, and we're starting the process. We've just been putting together a team of people. IBM Design is a good example of what I would love to see in general, and I was just at Philips Design in Eindhoven, and they too—they're brilliant and wonderful, the best I've ever seen and it's what HCI—because should it be HCI or should it be design and, I don't know? Who cares? Because computers are everywhere so you can't avoid them, and Sony doesn't consider itself—is this design [pointing to the Sony digital audio recorder on the table]?

Yost: Reading your work made me think about these machines and how they are designed, certain elements intuitive in use, others not so much.

Norman: And yes, so I love the work that's going on at IBM because you have to know the politics of the company, have to be able to understand the business model, and it turns out the big barrier is middle management. I don't blame middle management; I blame the reward structure of the company. I was at HP, an executive, my job was to bring in new devices, new products, and I could not get started. People loved me and they treated me well, after a year, I quit in frustration. I couldn't get anything done. They wouldn't let me quit! But we all went to a meeting where the CEO said, "We need to do more new things. I want more innovation. You need to take more risks. Do you understand that? We have to get change. But I'm going to hold you to your bottom line." And he had a little answering system so he could ask the question, and we could all push a button. And he said, "Now do you understand me?" and we all pushed 'No'. Instead of trying to understand why we said 'No', he seemed to repeat what he said but louder, and so when he asked the question, we said 'Yes'. HP was divided up into these relatively small companies all around the United States. If we hold ourselves to our bottom line—basically it's the oldest statement, "I want you to take risks, but you know, some risks fail, and some succeed. I only want you to do the ones that succeed." It's nonsense but that's the world, and the people at HCI don't understand that. And the people in design tend not to. The ones that work at a company outside of the research labs, you're faced by these problems all the time. And you can't say, "My work is that this is the best design ever." You got to do it, but what if it's contradictory to another part of the company or what if it might reduce the sales in another part of the company or what if this or what if that? And middle management is just rewarded for profitability. They are not rewarded for taking risks, even though that's what the CEO says. This is a change that is coming in over all of education too. I think that if you take a look at schools of engineering, it started off that MIT was a craft school and then it changed radically in the Second World War when the physicists came in and said, "Yeah, you know, there's some science behind all this." They turned to science which therefore-- now engineering is applied mathematics. And it's the white room phenomenon all over again. And stuff doesn't work when you put it out in the world. And so, there's more and more interest now in engineering, teaching people to do real things and real products that make a difference in the world. It's even in healthcare. The joke is, if I do something that saves lives of a thousand patients, people think it's nice. But if I get a paper published in *Science*, I get a promotion. And so, HCI has to change. But it has a journal now that's called *Interactions*, and that journal has changed. So today, it's more design than the old standard classical HCI papers. But the problem with that, it's the design that I'm trying to change and get rid of. It's all the cute, clever little things we do in universities that have zero practical value. My goal is try to say how do we take the science background and the importance of using evidence to drive forward to have a theoretical structure so it isn't that I build this or build that. There's nothing. I don't learn from one to the other but that there's a background of knowledge we can build upon. But still keep the wonderful playfulness and beauty and ease of use and understanding and designing, so not only that when things work well, I can understand it, but when something goes wrong, I can understand what went wrong, and what I should do. So that we're really effective in the real world, but without forgetting our deep scientific basis. Now that's a good thing.

Yost: Were you at all involved with curriculum efforts of CHI and what did you think of curriculum agenda setting work that the organization did?

Norman: No. I think that when I switched back to industry, I was heavily involved with CHI, was a big supporter, and Apple was one of the big contributors in those days. When [Steve] Jobs came back [to Apple], though, he killed my group. And we stopped and he killed the usability group too, and it shows… and this is 1980s products. Killing the research group, I think, was required. It was a very important research group. In fact, I simply called up my friends who ran the research group at IBM, who ran the group at Microsoft, everybody had new jobs almost immediately. But Apple was failing, and couldn't afford research in those days. I'm sorry. I lost the track. What was the question?

Yost: Interesting thinking about these contexts…. Oh, I had asked about SIGCHI and curriculum work, whether you were involved much.

Norman: I wasn't—and then I started switching to design more and more. I followed a start-up to Chicago. It failed. And then I went to Northwestern, and again, helped start an original master's program in design at the Segal Design Institute and taught design in the business school. So, I wasn't part of the curriculum efforts. That sort of passed me by.

Yost: You've educated multiple dozens of doctoral students, and can you tell me a little bit about how you organize research and kind of your philosophy of graduate education?

Norman: Well, it's one of those things that I'm very successful at but it's hard to know why. I look for unusual students. We brought Geoffrey Hinton in as a post-doc, and he was kind of—we got letters of recommendation for him that said he was brilliant but failing. And that's the sort of people we love. And I took a graduate student who had flunked out of Berkeley. I had to work hard to convince people here to admit him. You know, he flunked out because he was bored. And I like to say, somebody who gets straight A's, I don't accept them. I want someone who tries, who thinks differently, who takes courses not because you can get an A in them, but because they are interesting. Actually, when I was at Harvard, we had these—we accepted graduate students, they required a couple of exams, what do you call the graduate exam to get in?

Yost: GRE.

Norman: And there was another one, Miller's Aptitude Test or something. And the philosophy—they used Miller's Aptitude Test, they said, "Anybody who gets a high score, we will not accept." I look for unusual people, and so a lot of them were–I've been very, very, very pleased with the graduate students. My very first ones, Jonathan [Grudin] was one of them, Steve Palmer was chair of Psychology at Berkeley, Marc Eisenstadt helped run the Open University on-line courses, Abi Sellen is now Associate Director of Research at Microsoft, and she's a member of the Royal Academy of Engineering. I just got her into the American National Academy of Engineering as a foreign member. I have some students in Israel who are doing really, really well. David Navon. Naomi Miyake in Japan. I like to think she was the best known—not the best known woman psychologist, the best known psychologist in Japan. She died [in 2015], unfortunately. She had cancer. She was brilliant. I'm probably missing others. I have this whole philosophy that I want to surround myself with people who are smarter than me, and people who do creative, wonderful stuff. You've already heard me say that when someone says "You're wrong," that's wonderful. That's how I learn. So, in the instruction, though… Peter Lindsay and David Rumelhart and I started our research group and called it the LNR research group: Lindsay, Norman, Rumelhart. We held a research meeting every week where we would present our new ideas, the students would present ideas, and we would argue with each other. That was just wonderful! And I can't recreate it here. I've been trying to, and I have not been able to recreate it. Those meetings were fantastic—they were so cohesive where we got to do so many brilliant things and we helped establish the field of human information processing. We established neural semantic networks. We didn't invent semantic networks. It came out of BBN with Allan Collins. And then neural networks which sort of overthrew all that old stuff, because that was all good old-fashioned AI, made of networks and so on. And then the neural networks overthrew that. And now we have deep learning, which has been so successful, but I personally feel deep learning is wonderful and powerful yet isn't the whole story. It may very well be similar to the way the brain does pattern recognition. But they're really savants and to go across disciplines, well, we come back to this old question: what is the role of consciousness, and so on. Consciousness is very interesting because it's just a shallow top and it's very limited in its processing but it's very powerful. I wrote a paper once with—[Tim Shallice]. Turned out to be one of the most popular papers I've written. He's a neuroscientist in England. We wrote it when I was in England. Well, no. He came here on sabbatical. It was a paper on will, we resurrected the concept of will. We said it's consciousness, and the role of consciousness is that all these neural networks underneath but how do you prevent yourself from doing something that you otherwise wish or the body wants to do? Or how do you force yourself to do something you don't want to do? How do you get out of bed—this was William James' example. He said, "The hardest thing in the morning," he was talking about will,

willpower, "is to get out of bed." Remember, they didn't have heat in those days, a cold, freezing morning. And I can't force myself to do it by thinking I should get out of bed. The only way to get out of bed in the morning was I have to think about the wonderful breakfast I'm going to eat. The more I think about the breakfast, the more I find myself out of bed getting dressed. So we decided that will was basically the consciousness system that could send inhibition down, preventing you from doing something that you shouldn't do or sending excitation down, causing you to do something you otherwise didn't want to do. But it couldn't force you to do it. It could simply bias you to do it. I kind of think, though, that's how I work too, as I bias my direction, and I bias my students, but I don't control the students. In fact, I discovered that if students have some good ideas, and I said, "Wow. That's exciting! I'd like to work on that with you." They would refuse to do it. They all wanted to demonstrate their own independence, and they didn't want to do anything with me, which was always amusing because after they graduated, then they would want me to work with them because they had already demonstrated they had a job. There are some people I know who have this very tight, well-defined research program, and when a student comes in, they are assigned their jobs to do. We never did that. We did have the rule, though, that you can work at anything you like, go in any direction, but it has to be that we, Dave Rumelhart or I, care about. If we don't care about it, you shouldn't be our student. But we're not telling you to do what we want you to do, we're not telling you to do the work that we do. It has to be something that we think is exciting. Dedre Gentner is another example. She's been working on metaphor her whole entire career—she's really pushed that in a really good way. Her husband, Ken Forbus, is a computer scientist who actually develops computer programs. He does what is called qualitative reasoning, which is perfect for studying metaphor. So, we just—yes. I've learned so much from the students this way. I guess I consider the students are how I learn.

Yost: Can you discuss Northwestern as a setting for your research and educating students? You were there from 2001 to 2010.

Norman: Yes, well what happened is that I followed a start-up to Chicago, and when the start-up failed, my friends said, “Oh! Why don’t you come and teach at Northwestern?” So I went and talked to the Dean. So, he said, “Ok.” But I said, “I still have my own company, Nielsen Norman Group, and I wanted to still do consulting and work with them,” so it was a half-time appointment in Computer Science. So, I started teaching computer science. I didn’t teach very much. I taught a course or two, I don’t even remember what they were, obviously HCI-related topics. But then the new Dean came in, Julio Ottino, who was incredibly ambitious and wanted to change everything and move it around, and he and I became good friends. He always got mad at the faculty. They weren’t ambitious enough. They weren’t creative enough. We had a program at Northwestern called MMM: Master of Manufacturing and Management that was joint with the business school, and you got a degree in Master of Management and you also—Master of Engineering Management, MEM. And you also got an MBA in two years. They were being taught supply chain and operations from the engineering school and the rest they got—and it was not doing well. It had two people who ran it: one from engineering, one from the business school. And the engineering person left. I was a friend with Ed Colgate who was a mechanical engineer but also interested in design, somewhat. So, Julio asked if Ed would run the program from the engineering side, and Ed refused. He said, “I don’t want to work with those management students, those MBA students. I hate them!” A lot of the engineers feel that way. I said, “I’ll work with them—I’ll do it.” So, I took over the program, and I decided to get rid of operations because I didn’t think it was needed-- in fact, that was the weakness of the program. There was no operations in the business school when this was started. But now there was a strong operations group in the business school and it was not needed in engineering, and moreover, there was only one faculty among all of them willing to teach the MBA students. So, I decided to introduce design. I taught the business students. I taught in the normal way I teach, which has actually gotten me teaching awards. People like my teaching. After the first semester, the MBA students rated me the worst teacher they ever had. I went and talked to the

Dean, Associate Dean of the Business School, (who was a very good teacher) about all this, and he laughed at me and said—he "was considered a great teacher"—he said, "It took me about five years to learn how to teach them. We don't normally do to you what we did—we don't normally do to our teachers. When we hire a new person, they're not allowed to teach the first year. We assign them good teachers, and they follow them, and they see how they teach in the class." Because, first of all, I didn't handout a syllabus. Second of all, I didn't tell them, I didn't have PowerPoint slides and give them copies of the slides before presenting them. Third of all, I didn't tell them what was going to be on the final exam. What I did on the first day is that I gave them a problem to solve and they came back a week later with brilliant solutions. And I would say, "How do you know that's true [the solutions]? Your job is to redesign the interior of a car to make it more useful for people who use it. So those are clever ideas but how do you know that's what people want or will use?" That's what I do on purpose. I deliberately give people problems they don't have the background in, because if you give them the background first, they think it's boring. They don't know why they are learning it. If you give theory first they have no idea what it's about. Give them a problem to solve that they are interested in, and they get stuck, and now the theory makes all sorts of sense. And there wasn't going to be a final exam or a final project. It was interesting because what was going to happen is that they would give me horrible teaching ratings and then a year or two later, they would tell me about this wonderful course they took from me a few years ago. They told me that now they are applying or, "I'm starting a new company, would I be an advisor to the company?" and so on. Because it was not the way they were used to learning, but it made sense. I wouldn't let them give slides. I made them just give talks. Or I had another rule, "If you have to use a slide, and you should, because if you want to show data or show a picture or something, an illustration, ok, but no words, except if it's a graph or something. Just enough words to label the axes, you know." And one of them came back to me and said he decided he would try that in one of his courses, and his professor got so angry when he started to give the talk, he almost refused to let him give it. After he had given his presentation, the professor came up to him and said, “That was one of the best presentations we’ve had all year.” That was the fight I was having. But I enjoyed it. And the students liked me. I loved the students, but they would fight. I remember once I had this wonderful class where the class got into an argument with each other, went back and forth, and I said “Wow! I succeeded!” And then they came in to see me the next day, said that was the worst class they’ve had and did I know how much money they had spent to go to MBA school and give up the high salary they had for two years and they didn’t come to hear the stupid opinions of their classmates. Well, their classmates are some of the most brilliant people in the country. They’re highly selected. Almost any company I wanted to talk about, somebody worked there. One of them was not just in the Blue Angels, but the leader of the Blue Angels, Marine Corp team. I mean, they were all highly accomplished. They knew as much as anyone else, and moreover in business, there are no correct answers. So, what’s my philosophy about teaching? Mainly, I think the main thing I offer to my students is enthusiasm. But you’d be amazed at how little there is in our teaching because if they find that I’m enthusiastic about the material and so on, they too are enthusiastic. The rule is don’t do something because you think you should, do something that you enjoy doing. You’ll be better at it. That was a long answer.

Yost: Fascinating and valuable to hear. You’ve become a fellow with some incredibly important organizations: ACM, the American Philosophical Association, the—

Norman: No, no, no. Psychological.

Yost: Psychology. I’m sorry. And HFES. Can you talk about what these honors have meant to you?

Norman: Cognitive Science Society, [American Academy of Arts & Sciences]. The only society that I belong to that I’m not a fellow of is the Industrial Design Society. I’m an honorary fellow of the Design Society, and actually I complained when they gave it to me. I said, “I don’t want to be honorary. I want to be real.” But this is a British society and they explained to me, patiently, that honorary fellow is a higher title than fellow. I thought it was a lesser title. Well, you just live long enough and know enough people and that's how it happens.

Yost: Before we conclude, are there any topics you want to bring up, any—

Norman: You've done your homework quite well, I've noticed. Actually, when I get good interviewers, and you are one of them, I'm always asked that question at the end. No, because the interview covers a wide range of things and gives me latitude to, as you saw, give long answers. So could I talk more? Of course, but do I feel anything of significance has been left out? Well, I'm sure we've left out important people who heavily influenced my work, some of the places I've worked in. For example, I became a good friend of Amos Tversky when I was at Harvard, and he was still a graduate student, Danny Kahneman, and Anne Treisman, who I think I knew before Danny did. Danny eventually married her. And they, of course, are the Nobel Laureates. And Herb Simon—so the three Nobel Laureates in behavioral economics are good friends. And the last one was Rich Thaler from University of Chicago, and I didn't know him at all but he was in Chicago and I was at Northwestern. One day he shows up at Northwestern saying he had actually read my book *Design of Everyday Things* and thought the work he was doing was really design. And that was his book on nudging.

Yost: Susan Dray told me a wonderful story about how she bought a box of *The Design of Everyday Things,* or rather the first edition POET, and sent them to the execs of American Express Financial Services in Minneapolis when she wanted to start a design lab, and then she would ride up and down the elevator in the morning, for a week or so, just after she distributed them, and see what they were saying about the book. They were all talking about it, and she felt it helped her get the green light and the money to start the user, her design lab at Amex.

Norman: Interesting. Well, let me tell you about The *Design of Everyday Things*, or if you like, *The Psychology of Everyday Things*, which is essentially the same book. The title change was clever. The publisher said we got the wrong title because people would place it in psychology section of the bookstore, people interested in design never look there. People interested in psychology, they want to know about why their mother hates them, or something and not about design—she was right. I did my research. I went off to bookstores to ask what they thought, and they thought yes, changing the title would be wonderful. However, it got reviewed in the *New York Times* book review section, and the review said, "This is a stupid book. Has zero importance. The author thinks he has a sense of humor, but he doesn't. I've talked to some of my friends in the design field and they say, 'There's nothing in that book that already isn't well-known. It's all nonsense in that book.'" That was my review in the *New York Times*. At this point, it has sold close to a million copies.

Yost: I found it a fascinating book to read. I enjoyed it thoroughly.

Norman: Out of curiosity, did you read—which edition, the first or the second?

Yost: The second. Also really enjoyed *The Invisible Computer*.

Norman: Oh.

Yost: And certainly there have been efforts to design digital technology into mobile and other devices and make things more seamless, but in some ways, there is—

Norman: Yes, they are horrible! Seamless means I have no idea why you did what you did.

Yost: Yes.

Norman: This morning, there on my calendar, there's two appointments, which is the airline schedule. One of them I thought was wrong because it has the time difference problem and where did the second one come from?! And I opened it up, and I can't even figure out how to remove it. It says it's provided by Siri. Siri's reading my mail and adding it.

Yost: Well, this has been wonderful! Thank you so much for taking the time this morning.

Norman: Well, I've enjoyed it.