University of Illinois Student Life and Culture Archives
Interviewee: David Eisenman (Part 3)
Interviewer: Katie Nichols
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Katie Nichols: Okay. This is Katie Nichols, and I am here, it's Tuesday, December 10, 2pm, and I'm interviewing David Eisenman. We are in the conference room at the Archives Research Center. This is part three, I believe, of our ongoing oral history interview. Okay, so today you'd like to talk about—

David Eisenman: Well, I thought we would put together a few threads that connected to the centennial year. Looking back on it, I hadn't really ever thought about this, but if we don't talk about Regent Gregory housing, we don't understand why I would be involved with the centennial year, and the centennial year, in turn, put me in touch with Carl Woese, whose archive here has been the big source for David Quammen's recent superb book, 'The Tangled Web,' or *The Tangled Tree*, I think he calls it, all about Carl's search for the origin of life on Earth and for some coherent way of looking at all living creatures on earth, namely a tree of life. So I thought maybe we ought to start with Regent Gregory housing. Now, I haven't, haven't talked about Regent Gregory housing before, I don't think.

KN: No, I think you just mentioned it briefly, but you haven't really gone into any detail.

DE: That's right.

KN: And I did print out those articles that you sent.

DE: Excellent, great. That's, that's a very useful archive, because, in a sense, that's the first project that took me away from just being a physics student, I guess you'd say. I started—I came here in 1965 in the spring, from Harvard, looking for where I would go, what I would do next. I wouldn't say where I was going to go on. My choices were to stay at Harvard, finish the bachelor's degree. I was majoring in physics, but I had switched to biology for a year because modern biology was coming together right before my eyes. It was an astonishing time to be interested in biology, because at Harvard you had Crick, of the Watson-Crick model, teaching part of a freshman course in biology. They had a brilliant idea. They had a course that was only one semester. Biology 2. And the idea was, we would assume you knew some biology. The school would assume you knew some biology, but they would give you four professors, each of whom had done something pretty extraordinary, and that person would just talk about his or her own work. So it happened that Crick was on sabbatical that year, so for the part of the course that he normally would—what'd I say? Crick? I mean Watson. James Dewey Watson, of the Watson-

Crick model. Crick was—Watson was on sabbatical, so Matthew Meselson, who had been a physicist, but switched over to biology, took Watson's part of the course.

And Meselson was the person who proved that, indeed, the DNA helix came apart and each side of the helix was able to make a copy of the other side, allowing, of course, the genetic material to make a copy of itself, which is obviously what must happen in cell division. But Meselson proved it by putting reproducing, dividing cells into a nutrient medium that had either radioactive or isotopic nutrients, so that the second, the half of the helix being created as the original helix pulled apart would be heavier or radioactive compared to its previous original copy. And then in a centrifuge, you could centrifuge second generation cells, and you would find that the DNA would stratify into the original DNA, the DNA that's half heavier, okay, and then after a few more generations, you might even get DNA that is all radioactive or all heavier in any way. He proved that it has to be doing just what Watson and Crick suggested that it would do. So his lecture showed how you could bring techniques from physics in those days into biology and prove a point in biology.

The other teachers were equally interesting. Kenneth Thimann, T-H-I-M-A-N-N, had discovered plant hormones, auxins in particular, if we all know that if you snip off the terminal bud of a tree, for example, nice Christmas tree shape, if you snip off the very top of the tree, you will get side branches coming out. Sometimes you'll see in the woods a Christmas tree shape. The tree comes up to a certain point and then suddenly get two little Christmas trees on top of the original Christmas tree. Well, Thimann discovered that plants send signals out from the terminal bud that keep lateral buds from sprouting out. But if lightning strikes or an animal bites off the top of the plant, the plant regenerates by the lack of that hormone frees up lateral buds, who then grow out, and the tree sprouts and grows again. So he told how he did that work, and that was fun.

Don Griffin was the fellow who discovered that bats find insects by sonar. They put out extremely high-pitched squeaks that bounce off the insects come back, and the great big ears of the bat tell the bat where the insects are. Let's see, who is—oh and the fourth was the most exciting of all, in many ways. Keith Porter had, had been a another physicist who came into biology and discovered that if you perfused biological material with a monomer and then polymerized it, you could come up with a way of fixing tissue that would allow you to cut it exceedingly thin, and then an electron microscope could be used to use the quantum mechanical fact that particles have wavelengths, and you could now, you could extend the magnification possible when you look through a so-called electron microscope, so high that you could start looking at details within cells. And he was finding what later became known as the organelles, the mitochondria, endoplasmic reticulum, et cetera. Now, that was absolutely essential, because the DNA model of Watson and Crick gave us a sense of how genetic material might be passed on from cell to cell, but we didn't understand having an instruction tape required a machine that would translate the instructions into the building blocks of cells, and they were literally looking at and naming micro structures within cells right when I was in college. They were giving the names to things like mitochondria, endoplasmic reticulum, things that now every kid learns in high school.

So here are the people actually doing the work. And at that point, nobody had cracked the DNA code. They realized that all proteins are made out of a certain number of amino acids, and there must be some kind of code that three base letters in a DNA sequence—they thought it was probably three—would suggest a particular amino acid. So if you're assembling a protein, the DNA sequence would tell you how to make that protein. [Coughs.] But as I said, the tape of DNA would somehow or other have to be—some information from that tape would have to go out into some structure in the cell that would translate it into the sequence and actually build the protein that that cell was engaged in building. And nobody knew exactly how that was going to work, but Porter was making these extraordinary electron micrographs, which were now showing—you could look with your eye and see how thick the cell wall was, how thick the nuclear wall was, you could see the size of these little dots, the endoplasmic reticulum, which turned out to be the place where proteins are actually made.

This work was going on right at the time, and he was at the forefront of knowledge of the world, and he would bring to class and hand out actual photographs of electron micrographs that he had made the day before in the lab. Well, it was just too tempting, so I switched to biology in my junior year and took a full year course from Porter. That was just awfully exciting. He would say, 'Everybody's wondered what the Golgi body are. We've been able, we've been able to see them for 100 years. We can stain nervous tissue, and you will see these interesting cells, but nobody knows what they do.' And then the next Monday, he says, 'Yesterday or last weekend in the lab, you know, we figured it out.' You were right there watching modern biology come together. And Marshall Nirenberg came and gave an even lecture—evening lecture. He had been working some other place, but he was coming through. Gave a lecture. He was the first person to crack the DNA code. He made poly uracil by, by making artificial DNA and showing that, that if you just did U, U, U, U, U, U, it always coded for the same amino acid. So he, he came up with the first cracking of the code. We all said, 'He'll get the Nobel Prize.' Of course, he did. He got the Nobel Prize. We could see all the people who were going to get the Nobel Prizes in the next generation, you know. So it was exciting. So although I was majoring in physics, I couldn't resist biology. And I went to, I took biology courses. I went to night lectures in biology. I loved it.

Then I came here. Well, I came here to look at the place in April. And so I came to the physics department and the head of the department, Alney [ph] had me talk to Charlie Slichter, who has just died within the last year. Charlie was then, perhaps, well, must have been 40, 42, because he died at 93, something like that. And we are talking, oh, well, actually, we're talking almost 55 years ago now. Anyway, Charlie and I had a chat. Charlie later ended up on the corporation of Harvard, which is the five men who sort of own Harvard University. They literally hold title to it.

If they wanted to, they could sell it and take the money for themselves. They're not just trustees. They are the owners. Well, Charlie became one of them some years after this, his father had been a famous Harvard economist, Sumner selector, so Charlie grew up around Harvard, and I said, 'You know, my choice is come here and continue in physics, although I'm not completely convinced that I'm set to be a physicist, I don't know that I want to add to the top of the heap of knowledge, but I want to root around in it a little longer. But my interests are a little more vague than they should be, perhaps. But one choice would be to stay at Harvard and do a Master's of Science degree.'

I'd been studying the history of science, history of physics, in particular, with with Gerald Holton, who, again, to my astonishment, is still alive. Imagine a man who spoke to us when we were freshmen in 1961 is, in 2019, still going to work at age 96 in the same office where he and I had a seminar in 1965, you know, 55 years ago. And Holton and I have exchanged an email within the last year or two, which is, you know, to my amazement. In any case, I was becoming more interested in history and philosophy of physics than in actually doing physics. But as I said, I wanted to study some more of it. And Charlie Slichter, very smartly, said, 'Get the hell out of Cambridge. It's a disease. People find Cambridge so attractive that you can sort of think that's the only place to live.' But he said, 'There's a real world out here.'

So that's how I came to the University of Illinois, and immediately they gave me a teaching assistantship, and I started teaching freshmen, sophomores in physics in the basic courses. What I found was pretty bright kids, at least as bright as the people I'd been around as an undergraduate, but I also found that the brightest kids in the class were kind of lonely. They wanted to come up and talk after a session, and I got the distinct impression that, unlike the Harvard houses, there was nothing in the way people lived at the University of Illinois that would encourage, what shall we say, gregarious scholarship.

We were encouraged to spend an hour if we wished over breakfast, lunch, and dinner. The dining halls were large. They were dark and comfortable. They were just the opposite of a McDonald's. It was not fast food. You had, the entire house could have been in the dining room at the same time almost, you know, there was really room for us all. So right from the beginning, we spent a lot of time with each other. We undergraduates, and, and the chat wouldn't just be with people in your own major. You made friends in the freshman year, and then you broke up into 10 different living units called the houses, nine in my day, but they added one after I left, and roughly four to six hundred people in each one of those houses. So the houses are for sophomores, juniors, and seniors. The freshmen all live together pretty much in Harvard Yard.

So as a freshman, you, you meet 30 to 50 other people from all over the world, certainly all over the United States, and you get to know them, and they major in lots of different things, then that group breaks up over the nine houses, but the tendency is to keep in touch with them at least a little. And they're making new friends, and you're making new friends, and so it kind of created a natural network in which, if you had just read a book and you mentioned it to a friend, they'd say, 'Oh, I know a guy who's also interested in that book.' And so you didn't have to meet people. But the—it's not a surprise to me that Facebook came out of Harvard. Because there was a conscious effort on the part of Harvard to make the undergraduate experience one in which we taught each other. And, and you don't—it turns out David Riesman, a Harvard sociologist in those days, wrote a book called 'The American College' [*The Perpetual Dream: Reform and Experiment in the American College*], in which one of the questions he asked alums was, did your undergraduate experience change you? And the people who said that they, the people who strongly said yes, tended to—a very high percentage of people said a strong yes to that question at something like eight institutions.

KN: All right, I'm gonna—okay, just pick right up where we left off.

DE: Okay, well, we were—I was going on, possibly at too much length anyway, about what it had been like to be a Harvard undergraduate. But I was convinced, I was convinced that the students here were really no different from my classmates at Harvard, at least potentially, but that the social structure of life outside of the classroom at the University of Illinois was drastically different from what I had experienced. And I thought it would be fun to see if David Riesman was right, and whether the way you live on a campus could play a big role in how much you learn from each other. John Henry Cardinal Newman wrote a book called *The Idea of a University* in 1870 or '80 perhaps, and in there, he made the startling observation that if he had to give up every unit of a university, every aspect of university, except one, the thing he would keep would be the student body. He thought young people learned more from each other than they did from the teachers and the books. In a way, every student acts as a sort of a, what we say in physics is an impedance match between his friends and the whole rest of the world. If you know somebody, and you know how they think and what they're interested in, and then you come across something that excites you, you know with whom to share it. And it is in the process of sharing the struggle with understanding the Greeks or understanding quantum physics with someone else that we learn.

It's an old commonplace that the best way to learn something is to teach it. Well, when you learn together, when you ask each other questions and grill each other or try to tell somebody what you think you understood from a new thing you've just learned, you're sharpening your own skill in thinking because the other person says, 'I don't know what you're talking about,' and until you do get it clear to him or her, you haven't got it clear to yourself. So these are old observations. It's nothing new, but it's odd how, how rarely a university plans its structure.

And by the way, Harvard doesn't get a whole lot of credit for the house system. One man by the name of Harkness, who had tons of money during the depression of the 1920s and '30s, had been a Yale graduate as an undergraduate, but I think a Harvard Law graduate, if I remember this correctly. And he traveled to England and saw Oxford and Cambridge, and he was impressed with the so-called colleges at Oxford and Cambridge, which are indeed buildings that hold four to six hundred people and in which you eat, have comfortable quarters shared with several other students, and young men tutors, who are themselves working on PhDs or research projects, are scattered around within these dwellings and meet with the undergraduates regularly and fill in, as it were, the gaps in their learning that come between courses. Harvard had tutors in my day, so there was actually a copy of Oxford and Cambridge, which had been around, of course, at that point for a thousand years.

And Harkness came back and said to Yale, 'You know, I really, I've got the money, I would build you these buildings, I would build you colleges.' And Yale said, 'Thank you, but we like it the way we are.' Then he went to Harvard and said the same thing, and Harvard said, 'You know, we think that's a good idea.' So he did. He built the houses at Harvard or converted luxury housing for rich Harvard undergraduates into these more intentional structures. And then when Yale saw what he had done for Harvard, they said, 'Oh, we've had second thoughts.' And so he did the same thing for Yale [laughs]. So one man by the name of Harkness gets the credit for the houses at Harvard, and I believe they call them the, I think they call them the colleges at Yale. In any case, it works, and Amherst and a few other small liberal arts colleges that also have on-campus housing that happens to be roughly in that size and has taken advantage of it to make it attractive, so students stay on campus all four years. These are the places where the grads say, 'My life was really changed by that place.' So it's a, it's impressive, and I guess I'm an experimenter by instinct, and so I thought it'd be fun to try something like that at the University of Illinois.

So without saying too much more about the project, I'll just say we did it over massive opposition from the staff of the Dean of Students Office. The University of Illinois has a housing division that isn't really staffed by academics at all. It's by people who run hotels, basically, and I was describing the University of Illinois, by my second year as a hotel—a high school with a hotel attached. I felt that we were not trusting our students to really learn the—at Harvard, I could ignore a course for months. If I was bogged down writing a paper in some other course, I could do the minimum, say in physics, barely get by. But if, on the final exam, I knew the material, nobody held against me, the fact that I'd done very badly on our exam earlier. I mean, who cares when you learn it? If by the end of the semester, you've learned it. I could get extensions on papers. I'd go and say, look, here's 30 pages of draft, but I'm not ready to give you a final paper yet, because I've changed my idea in the course of writing this paper about what's really going on in this matter in history, or whatever the paper was on and by showing my work to my TAs or professors, I was always able to persuade them to give me whatever time I needed to get my work done in a way that satisfied me and would be interesting to them.

I remember writing a paper for Steve Thernstrom, who, again, is still alive, approaching 90. I owe him a letter because he made a big difference to me. He was a social science professor who was collecting real data about social mobility in New England. Anyway, Steve assigned us a paper in, I believe it was called Social Sciences 2, and I was going right along applying a theory of history to a chunk of history. And I was making the theory work by selecting my data. And you reach page eight of the paper, and you turn it and say, I no longer believe anything I said in the previous seven pages. And then I spent the second half of the paper, refuting the first half. And he loved it. He absolutely loved it. And I did too. I thought it was a not bad paper. But that was the kind of thing we were allowed to do and trusted to do. And what I found was that we had a rigorous grading system in physics that by God, if you didn't turn your homework in on time, and if you didn't do well on our exams, you definitely weren't going to get an A in a course, even if you aced the final. And I thought, well, this regimentation sounds like high school, like we don't trust these kids to decide when to learn and how to learn. They have to do it on a rigid, nice treadmill that we have set up.

So I had the feeling we were, we were like high school teachers cracking the whip, and assuming that everybody was just grudgingly doing the work. And I just saw those lack of, that lack of interaction. I started asking the housing division, would it be okay if faculty came to lunch with students and got a free lunch in the residence hall. So they said, 'Well, who's going to pay for it?' Things like that. Our faculty—we could invite faculty members to come to the houses for lunch, and you could have lunch with Nobel laureates or people who had fought with Joseph McCarthy during the McCarthy era. Anybody who wanted to bring lunch, they were always welcome. So, Charlie agreed with me that there were aspects of Harvard that shouldn't be unique to Harvard that would work perfectly well here. And with, with his support, and that of a few other senior faculty members, we finally got permission to do this experiment.

I don't want to go into it at any greater length, except to say that I've only lately remembered, or maybe realized for the first time seriously, that it was that project that probably motivated David Pines to come to me in 1967, the same year that it was launched, and say, 'I need somebody to work on the centennial year next year. We have an ambitious program of visiting lecturers, and you seem to show an interest in non-classroom learning. And this will be just that. John Kenneth Galbraith, the economist. In fact, Francis Crick, Watson's partner in the DNA discovery is going to come. These people will give evening lectures, and students might very much benefit from attending them, and why don't you take charge of the advertising, the arrangements, become the centennial coordinator of centennial events.'

There was a budget of a million and a half dollars, which in today's money, would be $10 million, so we had cash and, and we had a mandate to celebrate the 100th year of the University of Illinois. So that then brings me to Carl Woese. Because Carl Woese, who was in our biology department at that time and hadn't been here long, was going to be the host to Francis Crick.

Now, my job is to go and work with design students to produce posters that would be large and were—I had dedicated bulletin boards around campus. I also had—*The Daily Illini* was trading its space in Illini Hall for a certain amount of advertising space in the paper. The University wasn't using all of the space that they were entitled to, so I had a lot of free advertising space in *The Daily Illini*, but I needed to write ads. I also wanted to write for *The Daily Illini* and give a preview of what a person might be talking about, a particular lecturer. So it was a kind of— and then I would even meet these people at the airport and drive them to their residence, wherever they were going to be staying. But I also wanted to make sure that every campus visitor didn't just hobnob with his friends on the faculty, but that they were, besides giving the lecture in the evening, which was the typical pattern, they would also visit at least one class and maybe have lunch with students. So why not? If Francis Crick, who by then had his Nobel Prize, was on campus, why shouldn't some students meet him who were in biology? That kind of thing.

So I became the centennial coordinator. My job, as I say, included finding out enough about what somebody's lecture was going to be that I could, in turn, go and work with a graphic designer to come up with a good poster. So I went to see Carl Woese because he was Crick's host. So it was the first time we ever met. And somewhere in the course of saying, 'What's Crick going to be talking about, I see the title, but I don't know what he means by it.' Woese—I said, 'Well, okay, what do you do, you know, why are you Francis Crick's host?' And he said, 'You know, I have tenure, and I'm, I'm not a kid anymore, but I'm in the middle of my career, we hope.' He said, 'you could, in a position—I could do what most people do, which is make little additional contributions to my field, do an experiment, prove something, add a little bit of knowledge.' He said, 'I'm going after the biggest question of all, which is, how did life begin? At least life on earth.' And he said, 'I may never—it may not be a solvable problem. It may not be a question we can answer now, or possibly even ever, but why not go for the big one? That's the really—that one's the one that turns me on.' And I heard this, I took it in. But, you know, other than finding that little interesting, I didn't think about it.

So now we jump forward a mere 10 years. Yes, it's late '70s, and I'm back at the University of Illinois after some other adventures with the governor's office and with DeKalb research, where I was science advisor to the CEO, but I'm back on campus. And I looked up Judy Willis, whom I'd gotten to know very well during the centennial year. She was a Harvard PhD in biology, working on insect proteins, insect cuticular proteins. And I said, 'Judy, you know, I didn't enjoy my biology labs as much as I might have as an undergraduate, because the people around me were so much better that they would race through the experiments. My lab partners would always be in a hurry to get finished, and I would rather poke around in labs, and I haven't done enough of it.' And she said, 'Well, as it happens, my lab tech is leaving, and I need a lab tech. You would be the most overqualified lab tech in history. But if you're interested, sure, come and we'll, we'll get you some things to do.' In fact, I can't remember, maybe I was already volunteering to do tissue culture of cuticular cells when her lab tech quit. So maybe I was just volunteering over there. I'll have to ask Judy—she's still around, too—how that got started.

But anyway, I ended up being her lab tech, so one day in 1977—so this is okay, 10 years after the centennial year—it happens that Carl Woese's lab was down a long corridor and around the corner from where Judy's lab was, and one day, at about 3:30 or four in the afternoon, when sometimes we had tea, Carl walked in and said, 'You know, I think I found a new branch of life that we've been unaware of before, unrelated, in any way, very indirectly, related to the other things on earth.' And and he told us about it. So I'm one of the six, the first six people, maybe, not in Carl's lab to have heard about the Archaea, which are microorganisms that are, that had been thought to be exotic bacteria, but which Carl proved to be even more distant from the bacteria than they are from animals, and—what we now call animals and protists and plants.

So there we were hearing about the Archaea, and about a week later, there was a public announcement, and then Carl gave a department seminar and became quite famous for what he first called the archaebacteria. Later they were called the Archaea, because he hated himself for having been talked into slapping on the name bacteria, since they really aren't that closely related to the bacteria. Anyway, Carl became famous temporarily, the Wash—[laughs] he got a call from the White House saying, 'This new form of life—do we have anything to fear from it?' Well, [laughs] it wasn't a new form of life. It's probably the oldest thing on earth, but, but it—newly realized to be different from everything else, was what it was all about. But it turns out, the technique Carl was using for proving the relationship among creatures was to look at those micro-organelles that I talked about a little earlier, that we may have lost, and trace their changes over time by looking at how similar organelles in the cells of different branches of life differ from each other. The more they differ, the further apart you would assume evolution has pushed them over time, so you can sort of make yourself a tree of life by simply relating chemical characteristics of the RNA and DNA in micro components of cells.

It was a very interesting line of research, no question about it, and it's been enormously fruitful. And David Quammen's recent book shows how Carl's work right here at the University of Illinois changed the world of biology, especially microbiology. So this is just—all I'm doing here is four moments with Carl Woese over a period of 40 years. So moment number one, he announces his goal to figure out the origin of life. And he talked about that again on that day, when he came to our lab, he said, what I really want to know is, how did it all get going? There must have been a primordial soup in which chemicals are bouncing around, and somehow or other, a class of compounds, finds a way to make copies of itself, and that's what the double helix does. It makes a copy of itself by peeling apart, each side has a complementary base on the other side. So it's going to match up with that base if they're floating around in the nutrient medium. So somewhere in the primitive ocean cell—we don't have cells, yet—compounds started making copies of themselves, and they probably did it badly and made mistakes. And so slowly, somehow, over billions of years, we get some level of stability, and we now believe that the, that the mitochondria in our cells—the little powerhouses—are probably originated with bacteria that actually got inside other cells and began living with the other cells, a kind of weird symbiosis. People are calling it endosymbiosis. Anyway, it was brilliant work, brilliant work, only now really being fully appreciated, I think.

So, there's Carl in '77 still talking about that primordial soup and guessing that actually life began, probably many times, if you mean simply molecules capable of reproducing themselves, but that in the end, one strain worked well enough that it took over the whole world, and all the others have disappeared. We may have had thousands, millions of origins of life, but they didn't lead to anything stable enough to take over and win. But it's only in my lifetime that we've realized that every living creature on earth is related to every other living creature. It looks like we all come from a single surviving, at least, origin, which is kind of an exciting notion. And I've been arguing for 50 years that it is quite possible that the only life in the entire universe, vast as it is, unimaginably vast, could be the life we have.

And Enrico Fermi, a physicist, made the argument I think, that we have to take seriously, which is, we know that our particular solar system is a fairly recent one. Many parts of the universe are far older than we are. If life emerges frequently, it has to have emerged billions of years earlier than it did on Earth in other parts of the universe. And if indeed, something like us, sentient creatures capable of civilization and a great deal of control over nature, if they are at all common, they have come and gone, or have come and expanded elsewhere in the universe. And the astonishing thing is, we've never seen any signs of them. If they had a way of communicating with us or an urge to do so, God knows, they should, by now, and by now, really, billions of years ago, have come up with any technology that we'll ever think of that would allow them to let us know they're out there. And the fact that we haven't seen them is a strong argument that we shouldn't even think about them because we're never going to see them. We would have seen them by now if we're ever going to. So we better face it, as far as we know and as far as we'll ever know, we are it, and if we allow life to die, die out, if there's any way we can keep ourselves going, it's kind of up to us.

I've argued that—I wrote a lecture years ago called one equals zero and two equals infinity. And I think there are many examples in which you see something once and you dismiss it because it's a fluke, but if you see it twice, you begin to think there could be a lot of this out there. I think our minds are inclined to dismiss unique events. But the second—as soon as I see any kind of life that isn't related to all of us on Earth, I'm going to argue for billions of origins of life throughout the universe. But right at the moment, my hypothesis is we're it, and we better take it that way.

Okay, so next moment with Carl. In '84 or '85, the MacArthur Genius people gave Carl his Genius Award. So he got, in those days, I think maybe not—today, it's a half a million bucks.

Maybe then it was 250,000. Anyway, I went to his lab. Now, I hadn't seen him in eight years. He'd become famous, now in *The New York Times*, but I was doing other things. In fact, by then, I was working on HIV/AIDS in the blood, risk of getting AIDS from a blood transfusion, that was my main concern. But anyway, when he— when they announced that he was going to get this award, I said, 'Oh, yes.' So I went to Carl's lab, and I said, 'Carl, I'm here on behalf of the David P. Eisenman foundation.' I said, 'Now we have a policy, Carl, that if anybody in Champaign-Urbana wins a MacArthur Genius Award, we match it, I said, and we match it at the rate of $1 of ours for every $10,000 of The MacArthur money. So here's my check for $25.' And Carl didn't smile, didn't laugh. He said, 'Oh no, David, you don't have to do that.' He was completely serious. And I thought, oh my god, he doesn't see that this is meant to be funny. I said, 'No, Carl, I do have to do it. It's policy. You know, when you establish a policy at a foundation, you've got to carry it out. I don't, I don't see any choice here. I've got the check, and you've got to take it.' And he said, 'Well, okay, thanks.' And then I said, 'Now I just thought that since I was one of a handful of people outside of your lab who first heard about the Archaebacteria,' which I think we were still calling them in those days, 'I thought some of your lab techs might be amused to see the mimeograph sheet that went around the lab when you gave your lecture to the lab,' and they all were very happy to see it. And it's in my house somewhere, but I don't know if it is, it may even emerge. I remember bringing it back and posting it up on my wall in my study, but I've looked there recently, and it isn't there. But if you could see my study, there are 10,000 pieces of paper there. It may emerge. And I've looked recently for his cashed check, which I think would be hilarious, and I saved everything, everything. And I have my cash checks back to 1987, but I can't find, I think, through the round, but I haven't put my hand on '84, '85, '86 yet. I may not have gotten over to his lab right away. It might have been a six month or nine month delay between the time he got his his award from the MacArthur people.

So now we want to jump forward to 2002 and I'll wrap up David Eisenman and Carl Woese. But these are just funny stories. I think it was in 2002 that there was a—oh no, I guess there's another moment. In 1999, my boss, my my old boss at the governor's Bureau of the Budget, back from the Ogilvie years, John McCarter, who went on to DeKalb research and became their president, and I worked for him as a science advisor in the in the '70s. By 1999, John McCarter was head of the Field Museum in Chicago, and he called me up and said, 'David, as the millennium ends and as the century ends, we have a question,' which was, 'what was the most important scientific advance of the 20th century?' I said, 'You're asking me?' I said, 'Well, I don't know. I mean, I could think of things that I find exciting, but I'll tell you what, why don't I ask Carl Woese? Carl is really a clever guy and very perspicacious.' So I called Carl Woese. I said, 'Okay, Carl, here's this question. I'm not the guy, but you could be. What do you think?' And he said, 'Well, I think Norman Pace's discovery that we know almost nothing about microbiology—we thought we knew a lot, and we know almost nothing.' He said, 'I think that is the most extraordinary discovery of the 20th century.'

By the way, you know what people thought was the most extraordinary scientific advance of the 19th century? Now remember, the 19th century is the steam engine and the telephone and the phonograph record. And you could go on and on. The match, strike a match, get fire. Prior to the match, people struggled all through history to stay warm. If your fire went out, you know, you're rubbing two sticks together, trying to get, trying to get, yeah, as you're hitting your steel on flint and hoping to get sparks and get your wet paper to light, but the match, that was, was voted close to the end of the 19th century as the greatest invention of the 19th century.

Well, this wasn't invention. This scientific advance, and sometimes finding out what you don't know could be the biggest advance. Well, Carl went on to say, here was what Norman Pace did. He went out into his backyard, dug up a little tiny bit of soil, washed it with water, and got out, presumably, all the living microorganisms in it. Then used techniques to smash them, get out their DNA, multiply the DNA. We had something called polymerase chain reaction that lets you make thousands of copies of DNA fragments, so many that you can then feed them to a computer, well, feed them into a machine, which automatically sequenced the tape of the DNA. Then by looking at overlapping sequences at the ends of fragments, you reassembled the genome of each organism that was in your little sample of soil. When they did that, they discovered that they could recognize fewer than 3% of the sequences of genes that they had managed to come up with, meaning that for hundreds of years, people, well, 100, at least 100 years, people had been cultivating microorganisms. You know how it goes, they stick a swab down your throat and then put it in a test tube. It goes off, and a guy in a microbiology lab gets out this swab touches the tops of petri dishes and sees what grows. Some petri dishes have just sugar in the agar. Others have fetal calf serum. Something has blood. You see you have different nutrient mediums, but you want to see which bacteria or molds will grow out if they have something to eat.

What we just discovered, what Norman Pace showed, was that we aren't cultivating anything like 100% of the germs that are there because we don't know what to feed them. And what you have to realize is that what 90, what 98 of them are excreting—I was going to say shitting—is what the others eat. They're all there together. And if you don't have them all there together, they can't survive. I mean, talk about a micro ecosystem. And all we were studying for the last 100 years were the handful that happened to be able to live just on sugar or just on blood or on something complex like fetal calf serum. So we thought we were studying a representative sample of the micro world, and we were just studying very special creatures that happened to be able to grow fairly easily on things that humans think of throwing into agar. Wow. And then it turns out, he went to the ocean and did the same thing, took a drop of seawater and found exactly the same thing, that we thought we knew what was in the ocean, and we actually had cultivated and learned about only a handful of the creatures that are out there.

Now, again, it's funny how in a life, you start putting things together. One of our projects back at DeKalb, when I was in mergers and acquisitions, was to work with fancy chemists to come up with safer food colorings. Everybody knows, or maybe we don't, that the colorings that we use to make say, red velvet cake, you know, red number FDA, FDC, red number 6. These are all coal tar dyes that are absolutely dangerous, at least according to the Ames test. If you, if you throw them into cultures, they mutate microorganisms. And things that mutate microorganisms usually turn out also to be carcinogens. So we're eating stuff that, in theory, ought to give us cancer. But because nobody has ever been able to prove that you got cancer from eating red velvet cake, these things are known as, generally recognized as safe—GRAS—and they're kind of exempted from other rules. Well, the smart idea somebody had was, what if you could take the existing colors that are already approved by the FDA and leash them to a polymer backbone into a molecule so big that it can't even get through the wall of your colon. Shouldn't be able to interact with your body. You would eat them, and they would go right through you, and you poop them out, and they never get near your cells. Okay. This should be safer. It could be done chemically fairly easily. We could charge many times for these polymerized colors what people were paying for the commodity colors that anybody can make from coal, basically. And so this would be a profitable business, and it would slightly improve our confidence in the safety of our food. That was the idea.

So we actually did invest in that particular project. And one way to prove that you had polymerized colors was you used it to color Jell-O. So we had our polymerized colors in a layer of red Jell-O, and then a layer of clear Jell-O, a layer of blue Jell-O. If you put that in the refrigerator and you wait for weeks, if you did that with regular Jell-O, with the ordinary colors in it, the colors bleed. You can see the red Jell-O bleeding down into the white Jell-O in the middle, and the blue Jell-O bleeding up into—the colors are migrating. You can tell they're small molecules. They actually, just by bumping around, even in a refrigerator, they move/ But the polymerized colors didn't move. We had these absolutely perfect lines right between the layers of Jell-O, but when we served, when we, when we fed this Jell-O to animals, when we looked in the poop to find the colors that had gone through without being changed by the animal, the animal had somehow or other digested them. And we had tried stomach acid. We had tried known human splitting molecules. What I want to say enzymes, and we did not know how humans could possibly be digesting these things. Well, you see, we didn't know about all the microorganisms in the gut of the human, and we're just now talking about gut organisms and how important they actually are. The myth I was told as a biology student in the '60s was that cows could eat grass because they had microorganisms in their seven stomachs that can split the long chain sugars that we call cellulose and turn them into something that the animal can digest, whereas humans can't. Well, maybe we can't, but there are things going on in our gut that we are yet unaware of that have to do with at least the health of the the ability of our gut to keep bad bacteria from infecting us. And increasingly, as we take antibiotics that are stronger and stronger to kill bad germs, we're screwing up our guts. And so now we have a whole field of restoring intestinal flora. But it is a riot how little aspects of your life can come together later and explain each other.

Anyway, that's what I told McCarter in 1999. I don't think I'd realized that the DYNAPOL project had failed for a reason that Carl had just put his finger on. But in 2002, just a couple of years later, it was the 25th anniversary of the discovery of the Archaea, and there was a celebration here on campus in the form of a seminar at the, again, at my age, you just struggle for names. Okay, the the little museum to the east of the Krannert Center for the Performing Arts. That museum over there.

KN: Spurlock?

DE: Thank you, the Spurlock Museum. So it was in the Spurlock auditorium, which, by the way, is where Quammen spoke a year ago about his book on Carl Woese. So there was Carl Woese in that very room celebrating the 25th anniversary of the discovery of the Archaea. And who was in the room except Norman Pace, the very guy that Carl had mentioned in '99 as having made this discovery that we don't really know much about what's out there. And I just couldn't resist, at that meeting, standing up saying, because I really hadn't been in the loop of this particular part of the world. I said, 'Carl, you called them the archaebacteria back when you and I first knew each other, and you had just discovered them, when did they became known as the Archaea and why?' And he just burst out with a, 'Damn it! Worst mistake I ever made my life, letting those things be called the archaebacteria,' and and it turns out it was a real sore point with him, but, but the room just erupted. Everybody had a wonderful time after that, and it kind of loosened up the whole discussion.

So that was the last time I saw Carl. So here's a man I spent less than an hour with, well, maybe two hours in my whole life, but it's fun. I was there when I heard what his project was. I was there at the moment of his biggest discovery. I made him an award, and I asked him the right question. Actually, I'm good at asking questions, I think, because saying, 'What do you do, you know, what's your interest? Why-why do you know, how do you know Crick?' Sometimes, sometimes the right question gets you an adventure and, and so that's that's all I have to say about Carl Woese, okay. But I would never have had those adventures if it hadn't been for Regent Gregory housing, which got David Pines and Charlie Slichter, who were not just physicists, but educators, and thought a lot about, how is it that people actually learn what turns us on, what makes a university a place where people change and discover who they are. I never would have gotten to do most of what I've done in my life if they hadn't been for that one project.

And it's a—I say to young people, you know, if you think you should plan your life out at 16, decide what you're going to be and how you're going to do it, all that, you have to recognize that a 16-year-old's an idiot. The vast majority of 16-year-olds don't know anywhere near enough about life to know what would make for a satisfying life. There may be a handful of people who know exactly what they want to do, and I'm glad for them, but my strategy has been to never be too busy to drop whatever I'm doing if there's something much more exciting that I want to throw myself into that is ready now. And I tell people, I think of myself as having a giant stove with many things simmering on different burners. But if the world isn't ready for them, and I don't have the right partners to work, they just have to sit there. And there may be things you'd love to do or things you'd love to work on, but you don't know quite how to attack them yet. So in the meantime, it's perfectly okay to read randomly, be available to your friends, do useful work, but never be so busy that you can't drop what you're doing in an emergency and pick up the fire hose or leap aboard an opportunity and run it with it somewhere so.

Well, I wanted to cover Carl Woese today, but that, in turn, led to other topics which maybe we should hit quickly. Some of the interesting people on campus back in the '60s and '70s. George C. McVittie was in our astronomy department, but he was one of the few people in the world who took general relativity seriously. Einstein—it was said in my era that, you know, five people on earth might possibly understand Einstein's general relativity theory. I think that's nonsense at the—I believe I can explain it in six minutes, enough so that a person's whole view of the universe would change, which maybe is really all that most of us will ever get out of it, but McVittie was an excellent mathematician, and he took Einstein's field equations, which are a complex math that very few physicists would bother to learn unless you cared about general relativity. McVittie was teaching a course in general relativity. Nobody in the physics department—well, not nobody, but very few people in physics would have bothered in those days with general relativity. There wasn't a lot you could do with it. You can't perform experiments on the universe. There are a few things you can do on the face of the earth that actually can, can test the general relativity, the general theory of relativity.

Special relativity we all know about, that's the one where we just discovered that there is no single clock ticking for the entire universe, that the only clocks that are synchronized are ones that are, that are all moving at the same speed through space together. So we are all here on Earth. We are all not moving in a straight line, but we're going around our sun and our clocks, if we synchronize them, pretty much stay synchronized. But it is—Einstein was able to prove in 1905 that if you were identical twins, and if your twin got aboard a spaceship and accelerated slowly up to something close to the speed of light and went off on a venture to another planet and came back 40 years later, meaning he left at age 20, and now he's 60, that when he comes back, you would be long dead, and in fact, a couple of 100 years would have gone by on Earth. By leaving our inertial frame and accelerating and accelerating a lot, and then slowing down, stopping, turning around, coming back, his his body would have lived 40 years, but we back here would have gone through hundreds of years. We don't have the same clock ticking for all of us, and if we start going at high speed, we can prove that. Now there, there are we—this was easily proved. You can go to the top of a mountain and measure how many particles from outer space of a certain sort per square inch are coming through per second. And a detector at the top of a mountain, you know the speed they're coming at. You can measure that too. You also know how fast they decay. So you can predict how many should disappear between the time you measure at the top of the mountain and the time you measure in a valley below the mountain. When you go down and measure in the valley, you don't get the answer that you would predict if those particles who are going very, very fast were experiencing clocks that tick at the laboratory speed. We can have those same particles in a laboratory. They live a different length of time on the average than they do when they're going fast compared to us. Their clocks aren't ticking the same way our clock is. We can do earth bound experiments that prove special relativity. General relativity, on the other hand, is a much wilder proposition, and I could lecture on the subject, but I think I won't. My only point being that a broad enough education if you're curious about things, just because they're interesting. You, you lose some of your your more focused friends. Nobody could see why I would go over and take general relativity from McVittie, but I did.

Now, McVittie was a massive—he taught a very simple course. Actually, he didn't expect us to master the math. He just showed it to us, tried to make sure we got the concepts of general relativity down, and you could pass his final exam. He would give you six questions. He said, 'If you can answer any four of them satisfactorily, you'll get an A. But,' he said, 'it's a two-hour exam, and do your best.' So he said, 'On the other hand, I want to tell you something.' He said, 'I once had a student who got up after'—the final exam was three hours, maybe, I think that's right, three hours standard final exam. He said, 'I had a student once, I explained this to them, if they could answer four questions satisfactorily, they would get an A.' He said, 'This fellow got up after one hour and put the paper on my desk.' And I said, 'Why are you doing that?' And he said, 'Well, I've answered four and I think I've answered them satisfactorily. And you know, you said that would get me an A, so I'm leaving.' He said, 'Well, he had answered four of them satisfactorily, of course. He said I flunked him, of course.' [Laughs.] Now, he was British, and his notion was, if it's a three-hour exam, you should write everything you can in three hours. Well, just because you're smart enough to meet my minimum standard in one hour doesn't mean you have the right to get up and leave.

So McVittie was crotchety, shall we say. Well, McVittie also, I think at that point, had an appointment in the Center for Advanced Study. And I was still David Pines' assistant in the Center for Advanced Study. This must have been back during the riotous years of '68-'69, '69-'70, when I was working on Project 500, SELP, and raising money for the Martin Luther King Fund and eventually the SEAL project, which we've talked about. But I was hanging out at the Center for Advanced Study, doing these other things which had nothing to do with the Center for Advanced Study, but that's how life is. Anyway, here was McVittie, and I'm seeing him now, not as a student, but as you know, a person hanging around the Center for Advanced Study.

And Luitpold Wallach, PhD, PhD, was also in the Center for Advanced Study. One day he came down the stairway having visited with David Pines, complaining about something, saying, 'Outrageous, outrageous!' Oh yes, he wanted his own carrel in the library, his own personal carrel that he wouldn't have to clean up every day. Could leave his work materials. And he thought he should get that if he was a professor in the Center for Advanced Study. And I said something, you know, vaguely disrespectful about his expectations. And he said, 'Why don't we have lunch?' So we did, and I got to know Luitpold Wallach, PhD, PhD. [Nichols laughs.] He one day I asked him, 'Where did you get your education, Professor Wallach?' He was in classics. So he studied Latin and Greek. He said, 'In Gottingen and Dachau.' Ooh, concentration camp. He had been on the way to being a rabbi and lost all faith in God from what happened to him at the time of the Nazis.

He was an extraordinary guy, but he and he and McVittie and various other old European faculty who were of the real old school and believed in law and order and doing things the right way, all sat at lunch at the same table in the ballroom of the Illini Union. And every once in a while, I would join them. I was the only person under the age of 70, I think, at the table. And of course, I was at that point, my later 20s, middle 20s, and I was quite involved in doing things with the people on campus who were breaking the windows and rioting and complaining about the war and so forth, and these people were appalled at all of that. So I was sort of their contact into that world. And I think I became a hero of that group the time I threw the bottle through the window of *The Daily Illini*. They invited me and wanted to know all about it. [Nichols laughs.] So it was an interesting period, but there were some interesting people on this campus in those days.

John Bardeen was in physics. He had been a junior fellow at Harvard, which meant that they, they give you money, and they say, do anything you want. You are a bright young person. You're not tied down to any particular department. You don't have any teaching responsibilities. Use this place. We've got libraries, we've got people, come and do things. Bardeen was a junior fellow at Harvard, ended up, as you know, one of the inventors of the transistor, which utterly changed the world. Radios that had to have big, hot tubes in order to work, turned into little battery operated devices like this recorder that you're recording me on, that use far less energy. And John was famous for whispering, you could barely hear him, and for being very thoughtful. I tell my students that I've been in rooms over the years with Nobel Prize winners in physics, and they're not the first person to leap up with a remark if somebody says something or asks a question. Often they are quite quiet, but when they finally do say something, it actually has depth, instead of being the superficial, quick answer, which might satisfy people, but often is just being, quote, smart.

Anyway, I've always been pleased with the fact that John Bardeen got to know me and always greeted me on the street, which I felt was awfully kind. I don't think of myself as a person of any importance at all. But after he got his second Nobel Prize—and I think he remains the only person to get two Nobel Prizes in the same field—but the second one was for work that he and others did on superconductivity, the fact that there a super cooled liquid can actually have an electric current run in it, and have no resistance. The current can run almost forever. Bardeen came back—well, it was awarded, it was maybe 1972, something like that. Might have been a little later. In any case, I hadn't seen him since the Nobel Prize was announced, which is always in September, October. December had come. I think the Nobel Prizes were given out today, as a matter of fact, it's always December 10, which is Susie Pines' birthday, by the way. She's dead now, but now we will always know her birthday because it was Nobel day. But anyway, I hadn't seen John, and then I was in February, maybe of the next year, I was at a concert in the Krannert, and he and his wife came out into the lobby, and I was standing there, and we were looking at each other, and he smiled and said hello. And I thought, ‘Oh, God, what do you say to the guy who just got his second Nobel Prize?’ If you say what everybody in the world says, which is, congratulations on your damn prize, you know he's heard this at this point 30,000 times, and it's going to be not what you would do. On the other hand, you haven't seen him since he got it. And I blurted out, 'Has everything settled down?' You know, showing a little sympathy for the outrageous amount of publicity that you get. And his wife suddenly said, 'Oh God.' And she went on at great length about how annoying the whole thing had been, including one particular, extremely persistent reporter who called everybody that John Bardeen had ever known and pumped them for personal information. She said, ‘He even called your stepmother and asked if you've always twitched.’ And, and she said, 'John, you didn't twitch when I first met you.' And he did have, he did have a tick, his his head would jerk a little bit. And he laughed and said, 'No, I didn't tick in those days.' [Laughs.] I thought, she has had this built up in her for months, and all I had to ask was, has it settled down? [Laughs.]

But I'll tell you something about being an interviewer. I had a—I was on cable television for a couple of years here in Champaign, also in the 1980s. I would invite people to come and tell me about some project I heard about they were working on it. Very often they were people I had known years earlier but hadn't seen for a while, and I made a point of not rehearsing at all. We were just going to meet and have a conversation. So typically, we would, we would talk in the studio, just on a camera, talking heads, but then we would go and visit a project of some sort, do some field stuff, and then we talk again. We'd view some of that that had been cut by our producer, a really talented guy by the name of Greg Smith. We'd sit back in the studio and do voiceover on some of our field discussions or work. Anyway, that really taught me a lesson. When I saw the finished programs, I realized time and time again I would miss a cue from the person I was interviewing. I should have followed up, or there was a place I could have gone. But when we're talking with people, we're always half listening to them and half thinking about the next thing that we want to say, and we don't listen as well as we should. I'm now convinced that if you're serious about a conversation, I know, a man and a wife talking things out that are serious, or two people trying to understand the subject, struggling with it.

I think there should be some ground rules, and I would strongly recommend that both parties have a pencil and paper and make an agreement not to interrupt each other. I let you talk until you feel you've exhausted whatever your current topic is. If I have a question, I write the question down. I don't interrupt you. If I have a comment, I write down a word or two that would remind me what my comment was. I think in the absence of paper, we're all afraid we're going to forget some question or some point and we end up interrupting and destroying the flow of thought that we should be allowed to pursue. And I really very strongly recommend this. My experience is, a few times I've asked the right question, and it's wonderful what happens when you ask it and you just shut up, listen to the answer. But I also can think of many painful conversations in my life that would have been better. And I really do this when things are tense, either because we're passionate about getting something done, or we're angry at each other, or whatever it may be. I've gotten people to agree to the paper and pad and non-interrupt. It's just a simple intervention. But boy, the difference it makes.

All right, other people I wanted to mention. At a party at David Pines' house during the centennial year, who is in the group, except Niels Bohr's son. Here's the most famous physicist, in many ways, of the 20th century, Niels Bohr. Well, I mean, Einstein's the most famous, but Niels Bohr probably touched more people and made more things happen than anybody else. But he's—the Bohr atom was the beginning of quantum physics in many ways, and Niels Bohr was still around up till the time I was a graduate student, I think. Anyway, his son, Aage, also, was a Nobel Laureate in physics, a friend of David Pines, and I remember a party at David Pines house in which Niels Bohr's son Aage came up and said, 'Oh yes, now, your name is what, and what is it that you do?' I'm being asked about myself, a lowly graduate student in physics working on the centennial year by, you know, a Nobel Laureate. And I said, 'I am totally unimportant.'

KN: Oh.

DE: [Laughs.] But, but on the other hand, having expected that kind of people, I mean, John Cage would be there at David's house, there were famous people from all over the world constantly at parties at David's house. So then, the Pines's invited me to Christmas dinner 1967, and when I showed up, I was the only guest. And I think that one act of kindness was kind of a vote that has kept me going ever since. I thought, 'Gee, I'm interesting enough that I could be, you know, the only guest in this house where we've had Nobel laureates and famous composers and God knows who else.' These were the people who were around—Susie Pines explained that in those days, faculty—there were so few good restaurants, in fact, there may have been none in Champaign in the '50s and '60s, that one of the tests of a new faculty wife was, could you put on dinners for members of your department? And it was a very major fact, being a faculty wife, [laughs] and even when I came here, there were really, no restaurants.

KN: Really?

DE: It was appalling. I mean, the Illini Union was where we ate. Students—there were greasy spoons, a handful of vaguely acceptable places. But it was pretty, it was pretty primitive. Greasy spoons in those days. And, you know, nobody, nobody today would imagine what the place was. I'm sounding like an old person. People used to say, 'I walked through the snow two miles to go to school.' [Nichols laughs.' I'm feeling that I'm sounding like that.

But I think we've done what I wanted to do today, which was talk about what intellectual life was like on this campus when I first came here. That—I will say this, that in those tumultuous years, after—again, one thing led to another—after the centennial year, Carl Kaysen, an economist who was heading up the Institute for Advanced Study at Princeton, was visiting David Pines one day, and Carl had been tapped by Harvard to look into riots at Harvard, and they hired me to be an investigator. I spent the summer of '68 commuting back and forth between Cambridge and Champaign. I would work two weeks, three weeks here, three weeks there. And what I discovered was that at both campuses, as I interviewed faculty and talked about the relationship between faculty and students and the relationships between faculty and each other on campus, what I was finding was that specialization in wealth were changing the social structure on campus. We built a faculty center. It's across the street from the Krannert. It has never been used by the faculty in the way it was intended. Harvard had a faculty club, and it was thought that maybe the University of Illinois would benefit from having one. We got money donated to us. I forget the name of the thing, but the donor's name is on it. We tried to make that place work. It had a dining hall. The faculty were invited to go there, but nobody went. The Harvard faculty club was beginning to be less and less used. The—I mentioned the ballroom of the Illini Union. That's where faculty did meet and have lunch with each other.

But by 1967-8, faculty were being paid more than before. It had been the case at Harvard up through the Second World War that a Harvard faculty member wasn't really paid all that much. Mostly, Harvard faculty were people who were rich and were being faculty members because they could afford to be. Physicists funded their own research. Biologists paid for their own supplies. Science was a rich man's hobby. The atom bomb changed all that. Government money and industrial money started flowing into science because, for the first time, science was leading engineering. Engineering had been blasting forward, empirically, learning how to do things, and physicists would come along afterwards and do the analysis. The reason that bridge didn't fall down was... But first the guy built the bridge, then physicists figured out why didn't it fall down. But with the atom bomb, we suddenly realized basic physics could predict technology. You could guess, if you understood some fundamental principle of physics, how the world could evolve. In fact—I mean, how you could make a product that would be something really useful to humans that nobody had thought of before. But this was kind of new. And, and then the other thing was, of course, after Sputnik in 1957 Americans became aware of the fact that the Soviets were investing in science and technology education. We call it stem now, what science, technology, something in math, electronics, maybe I never get the acronym right, but suddenly became very important for many more people to go to college.

And as I think I mentioned in the previous hour, the social structure of college has changed. With so many more people going to college, places like Harvard, Princeton, Yale, didn't expand. And therefore when the wealthy wanted to send all of their kids to college, they swarmed in on places like the University of Illinois. I've already discussed the rapid increase in average family income on this campus between the late '50s and the time I arrived in '65 and now when we are really a pretty elitist campus from a socioeconomic point of view. But my analytical mind told me that race wasn't as important a factor if you cared about investment in human capital as was family background, meaning the educational level of parents and their income levels. And I think the economists are coming around and seeing the same thing. But it's hard to throw our minds back to a time when the faculty were, for the first time, being paid like real professionals, earning as much as doctors and lawyers, and becoming more and more sub-specialized. So within English, somebody studying Shakespeare no longer spoke to somebody studying James Joyce, because the techniques they were using to study Shakespeare, you know, you could feed all of Shakespeare to a computer, have it simply count the number of times he uses each word. And by doing that, you can identify chunks of Shakespeare that were probably written by somebody else. There are signatures to a person's vocabulary. Certain people use certain words more than other people use. Certain people use words that almost nobody ever uses except those few people.

So there were techniques coming into even old subjects like English and literature that were separating people from each other, and they were no longer cross fertilizing. Campuses were becoming less collegial and more specialized, and it was easier and easier for people to use long distance to talk to a buddy at some distant institution who happened to be closer to his or her own particular interests. So trying to find something that bonds us and creates a social community, it gets harder and harder. The faculty are more and more remote from each other and from their students. They come and they do their work and they go home, but they don't take a lot of responsibility for the intellectual growth of their, of their younger colleagues. So these issues were already emerging, and the people at Harvard were keenly aware of how their culture was changing, which, of course, made me think even more about how places like Harvard and the University of Illinois, were evolving, how they were different, and how the struggle to make this campus a little bit more of a home for people who are curious, genuinely curious, not just interested in memorizing a lot of stuff and having a degree slapped on their back, but who who honestly wanted to know things. And I still think that, I'm afraid fewer than 3% of even college students are genuinely curious and self-critical. I feel that a very large number of people go to college and feel that because nobody disproved any of the things they believed when they were in eighth grade, that they've just now had all of their eighth-grade prejudices ratified by a degree. So my feeling is you need to fight against that.

But this really isn't about me. It's supposed to be about the history of the University of Illinois. But I felt that I myself am seeing how my interests came together in this period through a series of coincidences, or a path in which each project, in a sense, led to the next one, because somebody saw what I was doing, had enough sympathy for it that they wanted to make sure that I had enough support to keep doing what I was doing. So Regent Gregory leads to centennial year, leads to Carl, and matches up with a previous interest in biology, which leads me to Judy's lab, which leads me back to Carl, which leads me up to the present. I'm reading a book about Carl aloud with a couple of students and catching up on modern biology at age 77.

So that's probably enough for today. We filled in some background, and what are the big ones that are still hanging over us? I might want to return to a bit of our original topic, which was student radicalism in the '60s, and what has happened to those people. How many people ended up coming by and doing oral history with you? Did you get anybody to do it?

KN: Um, I think six or seven, yeah.

DE: Okay, good, good, good. And so you know what they're doing now. I mean, they told you what's happened since? Excellent, very, good. Well, that would be fun. If you gave me a list of the people that you got to talk, I could see if I knew any of them, and if I can bring anything to bear on what they did. But I think this is good.

KN: Okay.

DE: And I'm feeling good about it.

KN: Good!