CHEMICAL HERITAGE FOUNDATION

MAURICE J. KERNAN

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

William Van Benschoten

at

State University of New York, Stony Brook
Stony Brook, New York

on

8, 11, and 18 November 2002

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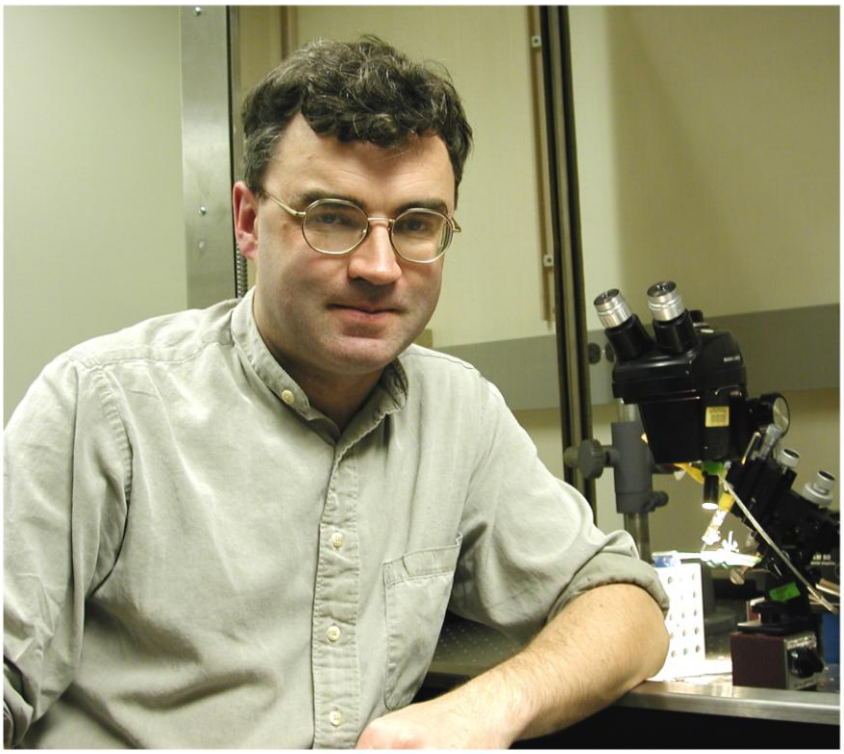

Maurice J. Kernan

ACKNOWLEDGEMENT

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UNIVERSITY OF CALIFORNIA, LOS ANGELES

Oral History Interview Agreement No. R120902B

This Interview Agreement is made and entered into this 9 day December of _____, 2002 by and between THE REGENTS OF THE UNIVERSITY OF CALIFORNIA, a California corporation, on behalf of the Oral History Program at the UCLA campus, hereinafter called "University," and MAURICE J. KERNAN, having an address at Neurobiology and Behavior, State University of New York at Stony Brook, Stony Brook, New York 11794-5230, hereinafter called "Interviewee."

Interviewee agrees to participate in a series of University-conducted tape-recorded interviews, commencing on or about November 8, 2002, and tentatively entitled "Interview with Maurice J. Kernan. This Agreement relates to any and all materials originating from the interviews, namely the tape recordings of the interviews and a written manuscript prepared from the tapes, hereinafter collectively called "the Work."

In consideration of the mutual covenants, conditions, and terms set forth below, the parties hereto hereby agree as follows:

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5. To insure against substantive error or misquotation, Interviewee will have the right to review the manuscript before it is put into final form. University therefore will send Interviewee a copy of the edited transcript for review and comment. Interviewee will return transcript and comments to University within 30 days of receipt of the transcript. In the event that Interviewee does not respond within 30 days, University will assume that Interviewee has given full approval of the transcript.

6. All notices and other official correspondence concerning this Agreement will be sent to the following:

If to University:

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If to Interviewee:

Maurice J. Kernan
Neurobiology and Behavior
State University of New York
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University and Interviewee have executed this Agreement on the date first written above.

INTERVIEWEE	THE REGENTS OF THE UNIVERSITY OF CALIFORNIA
Signed release form is on file at the Science History Institute	Signed release form is on file at the Science History Institute
(Signature)	(Signature)
Maurice J. Kernan	Janice L. Reiff
(Typed Name)	(Typed Name)
Neurobiology and Behavior	Interim Director, Oral History Program
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Date 11/8/2002	Date 9 Dec 2002

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MK
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✓ Interviewee's Name

14 July 2022
Date

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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MAURICE J. KERNAN

1962 Born in Dublin, Ireland, on 7 May

Education

1984 B.A., Genetics, University of Dublin, Trinity College
1990 Ph.D., Genetics, University of Wisconsin-Madison

Professional Experience

University of Wisconsin, Madison
1985-1990 Research Fellow or Graduate Research Assistant

University of California, San Diego
1990-1993 Research Associate, Howard Hughes Medical Institute
1993-1994 Visiting Research Biologist, Department of Biology

State University of New York, Stony Brook
1995-2001 Assistant Professor
2001-present Associate Professor, Department of Neurobiology and Behavior, Center for Developmental Genetics

Honors

1984 Wisconsin Alumni Research Foundation Fellowship
1985 UW-Madison: College of Agriculture and Life Sciences Fellowship
1989 University of Wisconsin-Madison: Lubrizol Industrial Fellowship
1991 Genetics Society of America: Sandler Memorial Award for thesis research in *Drosophila*
1997-2001 Pew Scholars Program in the Biomedical Sciences Grant

Selected Publications

M. J. Kernan, M. I. Kuroda, R. Kreber, B. S. Baker and B. Ganetzky (1991). *nap^{ts}*, a mutation affecting sodium channel activity in *Drosophila*, is an allele of *mle*, a regulator of X chromosome transcription. *Cell* 66 949-959

M. I. Kuroda, M. J. Kernan, R. Kreber, B. Ganetzky, and B. S. Baker (1991). The *maleless* protein associates with the X chromosome to regulate dosage compensation in

Drosophila. Cell 66 935-947.
M. Kernan, D. Cowan and C. Zuker (1994). Genetic dissection of mechanotransduction: mechanoreception-defective mutations of *Drosophila. Neuron* 12 1195-1206.
M. Kernan and C. Zuker (1995). Genetic approaches to mechanosensory transduction. *Curr. Opin. Neurobiology* 5 443-448.
M. Kernan. (1997). The molecular basis of the mechanical senses: one mechanism or many? *Journal of NIH Research* 9, 32-36.
D. F. Eberl, R.W. Hardy & M. J. Kernan (2000). Genetically related transduction mechanisms for hearing and touch in *Drosophila. Journal of Neuroscience* 20 (16) 5981-5988.
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R. Dubruille, A. Laurençon, C. Vandaele, E. Shishido, M. Coulon-Bublex, P. Swoboda, P. Couble, M. Kernan, & B. Durand (2002) *Drosophila* regulatory factor X is necessary for ciliated neuron differentiation. *Development,* 129 (23): 5487-5498.
Y-G. Han, H. Kwok, & M. J. Kernan (2003). Intraflagellar transport is required to differentiate sensory cilia but not sperm in *Drosophila. Current Biology,* 13, 1679 – 1686.
J. Kim, Y. D. Chung, D. Park, S. K. Choi, D. W. Shin, H. Soh, H. W. Lee, W. Son, J. Yim, C-S. Park, M. J. Kernan, & C. Kim (2003). A TRPV family ion channel required for hearing in *Drosophila. Nature* 424, 8 1-4.
T.J. Watnick, Y. Jin, E. Matunis, M.J. Kernan, C. Montell (2003). A flagellar polycystin-2 homolog required for male fertility in *Drosophila. Current Biology,* 13 2179-2184.
M. Martinez-Campos, R. Basto, J. D. Baker, M. Kernan and J. W. Raff (2004). The *Drosophila* pericentrin-like protein (D-PLP) recruits several proteins to centrosomes and is essential for the formation of functional cilia and flagella. *J.Cell Biology,* 165, 673-683.
J. D. Baker, S. Adhikarakunnathu, and M. J. Kernan (2004). Mechanosensorydefective, male-sterile *unc* mutants identify a novel coiled-coil protein required for ciliogenesis in *Drosophila. Development,* 131, 3411-3422.

ABSTRACT

Maurice J. Kernan was born and raised in Dublin, Ireland, the eldest of four siblings. His father worked for an insurance company; his mother was a homemaker. A love of the outdoors and interest in nature was nurtured at a nearby area of salt marsh and sand dunes, North Bull Island in Dublin Bay, where he explored and watched birds; his science projects in school were nature-based and carried out there. An avid reader, his formal education began in the local public school, but from the age of eight he attended a Jesuit day school, Belvedere College.

He matriculated at Trinity College, Dublin to study biology and developed an interest in genetics, studying in a department with ties to genetic research in the United States. During the summer after his third year of college, he traveled to the US and trained with a Trinity alumnus, Mittur Jagadish, in the Boyce Thompson Institute for Plant Research at Cornell University. While at Cornell, he also heard a lecture from Allan C. Spradling, who, with Gerald M. Rubin, had just figured out how to make transgenic *Drosophila* using transposable P elements. After earning his degree, he moved to the United States for graduate research in genetics at the University of Wisconsin-Madison, joining Barry Ganetzky's *Drosophila* laboratory; his doctoral research on a mutation affecting nerve cell activity led to a pair of *Cell* papers in the early 1990s. Kernan undertook postdoctoral work in *Drosophila* with Charles S. Zuker at the University of California, San Diego, where he began a genetic analysis of the sense of touch; from there he accepted a faculty position at SUNY Stony Brook where this work continued.

At the end of the interview, Kernan discusses setting up his laboratory and research program and learning to be a laboratory manager. He also discusses funding, teaching, balancing family life with his career, competition and collaboration, the nation's scientific agenda, and the Pew Scholars Program in the Biomedical Sciences.

UCLA INTERVIEW HISTORY

INTERVIEWER:

William Van Benschoten, Interviewer, UCLA Oral History Program. B.A., History, University of California, Riverside; M.A., History, University of California, Riverside; C. Phil., History, UCLA

TIME AND SETTING OF INTERVIEW:

Place: Kernan's office, SUNY Stony Brook University.

Dates, length of sessions: November 8, 2002; November 11, 2002; and November 18, 2002.

Total number of recorded hours: 6.22

Persons present during interview: Kernan and Van Benschoten.

CONDUCT OF INTERVIEW:

This interview is one in a series with Pew Scholars in the Biomedical Sciences conducted by the UCLA Oral History Program in conjunction with the Pew Charitable Trusts's Pew Scholars in the Biomedical Sciences Oral History and Archives Project. The project has been designed to document the backgrounds, education, and research of biomedical scientists awarded four-year Pew scholarships since 1988.

To provide an overall framework for project interviews, the director of the UCLA Oral History Program and three UCLA faculty project consultants developed a topic outline. In preparing for this interview, Van Benschoten held a telephone preinterview conversation with Kernan to obtain written background information (curriculum vitae, copies of published articles, etc.) and agree on an interviewing schedule. He also reviewed prior Pew scholars' interviews and the documentation in Kernan's file at the Pew Scholars Program office in San Francisco, including his proposal application, letters of recommendation, and reviews by Pew Scholars Program national advisory committee members.

ORIGINAL EDITING:

Carol Squires edited the interview. She checked the verbatim transcript of the interview against the original tape recordings, edited for punctuation, paragraphing, and spelling, and verified proper names. Words and phrases inserted by the editor have been bracketed.

Maurice Kernan reviewed the transcript. He verified proper names and made minor corrections and additions.

Carol Squires prepared the table of contents. William Van Benschoten assembled the biographical summary and interview history. Squires compiled the index.

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INTERVIEWEE: **Maurice J. Kernan**

INTERVIEWER: **William Van Benschoten**

LOCATION: **State University of New York, Stony Brook**
Stony Brook, New York

DATE: **8 November 2002**

VAN BENSCHOTEN: Today I am with Maurice Kernan. This is November 8th, 2002. This is tape one, side A of the Pew [Scholars Program in the Biomedical Sciences] oral history interview that we're conducting here at SUNY [State University of New York] Stony Brook.

Let's start with something fairly basic. What is your full name?

KERNAN: Maurice Kernan, pronounced "Morris," though it's spelled Maurice when you're from the other side of the Atlantic.

VAN BENSCHOTEN: Okay. Maurice. Where and when were you born?

KERNAN: I was born in May, 1962, in Dublin, in Ireland.

VAN BENSCHOTEN: Did you spend most of your childhood in Dublin?

KERNAN: Yes. We lived in Dublin all my childhood, even right through when I went to college. I got my primary degree in Dublin. I lived at home with my family while I— It's more usual, I think, in Ireland than here.

VAN BENSCHOTEN: What I know of Dublin comes from James Joyce, *Dubliners*, *The Autobiography of William Butler Yeats*, and probably a few other places like that. That was about a hundred years ago in Dublin. What was Dublin like when you were growing up?

KERNAN: Well, there are some points of intersection. I went to the same schools that James Joyce did. So I guess you could certainly find places that haven't changed. But Dublin was changing a lot while I was growing up, and has continued, and perhaps changed even more in the last five years in terms of how rich people have gotten. It's quite a wealthy country now. I

probably couldn't afford to move back there.

VAN BENSCHOTEN: Oh, really.

KERNAN: I think I had a very middle-class upbringing in Dublin. Never felt deprived in any way. Started off at the usual National School—public school that people go to—but then switched to a Jesuit-run fee-paying school when I was about eight, and stayed at that right through the equivalent of high school graduation. Most of my family still live there. My brother [Colm Kernan] and sisters [Niamh and Margaret Kernan], my parents [Thomas Kernan and Veronica Perry Kernan] are still alive.

VAN BENSCHOTEN: How often do you get back there?

KERNAN: About every two years or so. I used to manage to do it about every year and a half, alternating summer and winter visits, but since I've had children myself, that's slowed down a little. More often, people come over to visit us here.

VAN BENSCHOTEN: And what part of Dublin?

KERNAN: In Clontarf, north side of Dublin, which, depending on where— There may be a slight difference, depending on whether you come from the north side or south side, in your attitude to life in general, but we came from the suburb, a particular area called Clontarf. It's hard to put it into equivalent terms here, but it was a good place to grow up.

VAN BENSCHOTEN: I'm doing a little research in New York. I'm sort of new to New York City and exploring it and understanding it a little bit better, but there is definitely a sense of place in New York. You know, if you belong let's say on the Upper East Side—

KERNAN: Right.

VAN BENSCHOTEN: —you definitely don't connect yourself with, say, people in Brooklyn or Queens or whatnot. I mean, is there that sort of loyalty or connection to place in Dublin, in the various parts of Dublin, as you would find maybe here in New York?

KERNAN: Oh, it depends on how far you want to go back. I mean, a child is connected to their

immediate surroundings, yes, you grow up with— I think, a lot more of your time as a child is spent, or was spent, on the street, just being outdoors, out of your parents' hair, than would be the case here. So right from the start you've got a sense of place by being outdoors most of the time, despite the weather.

I think childhood was a process of exploration and gradually expanding the boundaries of what I could explore. That's sort of in some way analogous to the way I do science. One thing that became important early on for me was a thought of getting as quickly as possible to places which were natural, where you could do natural history. I remember the first bicycle I got; you know, that's an expansion of your range for a child. I did a lot of cycling all the time I was in Ireland. Never actually drove a car, even though I was graduated from college, until I was near the end of my Ph.D.

VAN BENSCHOTEN: That's amazing for an Angeleno to hear.

KERNAN: I was trying to think about how far back you wanted to go. In Dublin, one of the— Where I grew up is slightly more urbanized than a typical suburb here, because houses are closer together. It's more built up, so, somewhere between a suburb and a city, particularly the area [Clontarf] we lived in, but they're close enough by the area— You could get to natural areas. One of the closest that was really important for me was a big area of salt marsh, sand dune islands called [North] Bull Island. I started getting into bird-watching and doing a lot of exploring there. Things like high school science projects were usually natural history-based, down in that area.

VAN BENSCHOTEN: Let's talk a little bit about your family. Let's start with your grandparents. Could you tell us maybe a little bit about them, what they did and where they lived, and your relationship with them?

KERNAN: I only knew two of my grandparents. My father's parents both died before I was born. One, his father, my paternal grandfather [Thomas George Kernan], died when he was very young, so he never knew his father, really. He was the youngest of a family of seven— No, six brothers, I think. He was reared by his mother [Mary Theresa Kernan] and by his older brothers. So I know nothing at all of him other than maybe an old photograph or two. His mother I didn't know at all but except from the odd story, and not too much of those.

My mother's [Veronica Perry Kernan] parents I knew much better. They owned— Both families, both my mother and father's family, lived in Dublin also, and their parents back for about five generations, which is sort of even unusual. They were proud of that. It's sort of even unusual in Dublin because that's a city to which many other people have come and settled. But they lived in neighboring areas.

My maternal grandfather [William F. Perry] owned a store, general store, hardware store. He died when I was probably about early teens, I think, maybe twelve, maybe thirteen. I remember [my maternal grandfather] as somebody fairly jovial who we would make fun of a lot, but he was set in his ways. His history included— Afterwards, things you'd wished you had found out more about were times he'd either fought or helped with the Irish independence in 1920, '21, had been interned by the British in Wales very briefly, had some medals that we've dug up out of a closet that came from that era. I think he didn't like to talk about it much, in part because I think part of his family and his wife's family, my grandmother's family, were on opposite sides in the civil war that followed immediately after, and obviously they didn't like to dig it up again. So we never heard too much about it.

Let's see. My maternal grandmother [Emily Colley Perry] was the person I knew best, because she survived longest, right up until— I was in college when she died. She was small, very amazingly vigorous. She lived into her nineties and was bright and sharp, obviously conservative and set in her ways, but up to— One of my memories of her is bringing us to the zoo once and her climbing up on the fence to get a better look at the penguins, which was something you don't expect your ninety-yearold grandmother to be doing. She died eventually of melanoma, cancer, and I think that was— Sort of lived with us during the last stages of that. Seeing that was probably the closest I'd come to death and seeing somebody die. Otherwise my life has been relatively untouched by death.

VAN BENSCHOTEN: And what did she do?

KERNAN: She was a wife of a retired storekeeper. She didn't have a paying job herself. She was retired, living on a pension. And probably—this is the way of grandchildren—she existed, from my point of view, in order to serve me and my siblings.

VAN BENSCHOTEN: [laughs] Exactly.

KERNAN: Then again, yes, I didn't talk enough to her about herself. Probably you'll find a general— Something I'll come up against is I felt I never asked people enough about themselves.

[Tape recorder off.]

VAN BENSCHOTEN: Okay. We're talking about your maternal grandmother. Did you want to add anything else to that description?

KERNAN: No. I find it sort of frustrating. I could summon up her image and something she would do or say instantly, but as with my grandfather, it's just sort of frustrating to know how little of that is conveyed through description, at least insofar as I may be able to give it.

VAN BENSCHOTEN: My grandmother told stories, and there were always people who sort of appeared in these stories again and again and again, and I don't know who they were or where they lived, what they did. But there they are in these stories. I wish that she were alive to explain that.

Let's talk a little bit about your parents. How about your father [Thomas Kernan]?

KERNAN: He is someone I find myself resembling more and more every day. [mutual laughter] And it's remarked on more and more, every time I ask people to turn out the lights. "What're all these lights doing on in the house?" I think of myself; that's my dad.

He's somebody who's very gentle in manner, people like a lot, worked, finished high school to the stage that most people did, which in Ireland was called the intermediate certificate, which meant you left school at about sixteen or so and worked. Got a job in an insurance company, and worked basically for that same company or derivatives of it for his working life. He's now retired, is somebody who is, I think, fairly shy. He's a mixture of somebody who's quite shy but can be very socially adept once he's familiar with people, something probably also I've again observed in myself. I keep observing things in similarity, ways in which I'm similar to him. At one point, again around the time I was in college, he struggled somewhat with depression, which is— Probably there is a tendency to run in that side of the family. In fact, I think we've even been part of a genetic study that people have done in Ireland on that. Some other of his brothers, one or two of his brothers have the same sort of related problems.

I think my earlier memories of him have always been as sort of the source of stability in the family. He tends to be one who calms things down, doesn't like— Likes things to be more emotionally on a more even keel. He's very happy about what I do, but not all that terribly interested in it. I think when I was deciding to go into science, he was more worried about whether it was a workable job. I think some cousins were enlisted to call me up and say, "Well, how about biomedical engineering?"

VAN BENSCHOTEN: [laughs] A little bit more money.

KERNAN: Most of my many male cousins have all been engineers. It's much more the family trait, rather than scientist.

VAN BENSCHOTEN: What was your relationship with your father, growing up?

KERNAN: Almost completely unexamined. [laughs] Dependent on him in some ways, but not in— He was very much not somebody who'd be controlling or dominating. The times I felt closest to him, again, were doing things that he did and would have done more of in his youth, but did less of when he was— You know, again things like going hiking in the hills south of Dublin. [pauses]

I don't know. What's occurring to me is only descriptions of my negatives, you know, not this, not that, which is neither of us had much of an interest in sports, which is sometimes a father-son thing. Though a very early memory is being taken to see a soccer game, which is something he used to do, again, in his youth, local soccer teams.

VAN BENSCHOTEN: What was his name?

KERNAN: Thomas Kernan. Tom, Tommy. My son's named Thomas [Thomas Piers Kernan] after him.

VAN BENSCHOTEN: I'm sorry, I interrupted you, though. What were you going to say?

KERNAN: I think our relationship was quite distant, not because we weren't on good terms with each other; just because neither of us was being particularly outgoing until, in some ways, after I went away. Then, obviously, you think more about what you've left behind. So visits back are often more— You use the time to catch up more than you would have and talked about more in the few days I was back, rather than you might have in months or while I was living there.

VAN BENSCHOTEN: Is he retired now?

KERNAN: Yes.

VAN BENSCHOTEN: And he still lives in Dublin.

KERNAN: Still lives in Dublin, enjoys retirement. Took up golf, along with several of his brothers and many of their sons. So there's a whole golfing scene there that I'm, again, not— Wouldn't know how to participate in when I go back. My parents started traveling a lot more lately. Every time I call up they seem to be either just off to Greece or back from Paris.

VAN BENSCHOTEN: Really. That's nice.

KERNAN: Which is great.

VAN BENSCHOTEN: They've come here, right? They visited you.

KERNAN: They visited me. I was here. They visited me. When I was in Wisconsin, my dad came out to visit. I don't think my mother visited me there. I was married when I was in San Diego [California]. They came out then. They've come out here. They were at Long Island [New York] once or twice.

VAN BENSCHOTEN: What is their view of the United States, of Americans? Out of curiosity.

KERNAN: [laughs] Oh, I would say a standard Irish view, which is great affection mixed with a lot of disdain for American foreign policy. You know, none of my father's family, none of his contemporaries came over to the U.S. [United States], which is probably unusual for a family that size. My mother's brother [Desmond F. Perry] did and has lived here, and some cousins did, and that's another important family connection for me. So she's been in contact, you know, she's visited Chicago, which is where they live, a couple of times, or Trenton, which is where her cousins live.

VAN BENSCHOTEN: Why don't we turn to your mother. If you could give us her full name.

KERNAN: Veronica Kernan. Veronica Perry was her maiden name. Usually Vera.

VAN BENSCHOTEN: And describe her a little bit.

KERNAN: Probably the dominant influence on me. No, certainly the dominant influence on me, as probably for any Irish son, particularly on the Irish eldest son. Very, very lively, intelligent. Probably the one who would have tended to be pushing me to do more, live up to my capabilities, to which I quite frequently pushed back. More, I think, with her I would share very much the same sort of sense of humor, which can be sort of ironic, sarcastic, pretty dry. [pauses] Let's see. I'm just trying to get a grip on some specifics to make it easier. Describing your mother is—

VAN BENSCHOTEN: It's a hard thing.

KERNAN: It's a hard thing.

VAN BENSCHOTEN: It is. People who influence us deeply, I think. What kind of education did she have?

KERNAN: Educated in a convent school, I think, in Dublin, in the city center. Did not go to college. Went further in high school. Went on, I think, as far as the leaving certificate, which is now the standard exam you take—about seventeen or eighteen— that people will go through. So, went further than my dad in high school, but didn't go to college. Not many people would have, of her generation then. But her brother [Desmond F. Perry] did, though. He was older by two years.

VAN BENSCHOTEN: So was it a small family, then, just her and her brother?

KERNAN: Yes. On that side it was just her and her brother.

VAN BENSCHOTEN: And how about hobbies? What did she like to do, more or less? What did she do?

KERNAN: Again, she existed to— In our point of view, she was the center of the world. [pauses] I'm not sure that she did all of the things that she would have liked to do when we were growing up, and that's probably some of what she's getting to do now. She'd done some traveling with her family before she was married, quite a bit in Europe, and is now getting to do that again.

I had a younger sister who was born—I've got three siblings—I've got a younger sister [Niamh Kernan] who was born when I was fourteen, sort of the long-lost child. Not the afterthought, but unexpectedly late child. So it meant that their childrearing years were extended quite a bit more than perhaps they would have expected.

VAN BENSCHOTEN: So are then you the oldest child?

KERNAN: I'm the oldest. I'm the oldest. I've got a sister [Margaret Kernan] who's two years younger than me, a brother [Colm Kernan] three years younger than that, and then a sister who's fourteen years younger than I.

I think what she likes doing best is being sociable with a group of friends who are friends from her youth, who are so closely knit that we refer to them as Aunt Breda [Hilliard], Aunt Margaret [Halliden], Auntie Breda, who would have been, you know—although they weren't aunts at all, not related at all—just her close-knit friends, and still are. That and traveling, some walking, just still—

[Tape recorder off.]

Yes, sort of strange hopping back between the lab world and then going way back to childhood.

VAN BENSCHOTEN: It's kind of wrenching. But you said that your mother, for instance, had four children, and now she enjoys traveling, probably something she wanted to do earlier.

KERNAN: Yes. I think the overall impression, I think something that struck me when I came to the U.S. was how very, very stable my childhood had been in comparison to most of the people I was at graduate school with, when we got to sharing family stories and family backgrounds. And that was something for which I felt extremely fortunate, you know, and appreciate only after I came here, perhaps, appreciated the commitment to stability and to making a good environment for a family that my parents had made.

VAN BENSCHOTEN: So you would ascribe that, then, to— You had already mentioned that your dad, for instance, was a very stable figure, too.

KERNAN: Right.

VAN BENSCHOTEN: I assume then that the marriage worked—

KERNAN: Yes.

VAN BENSCHOTEN: At least well enough so that—

KERNAN: Did and does. They're quite different personalities, so they're complementary in

some ways. So sometimes there is some tension, but it was, again, at the time, we never or very rarely, if ever, were aware that they would have been fighting at any time, although I'm sure they were, at times.

VAN BENSCHOTEN: Would you say now that your mother came from sort of middle-class origins?

KERNAN: Yes. Maybe upper middle class, whereas my father may be coming from lower middle class. There was a slight difference there, a store owner's daughter versus somebody whose father, when he was alive, I think had been a carpenter or joiner.

VAN BENSCHOTEN: Skilled craftsman, artisan.

KERNAN: Yes. As I said, who died when my father was so young that he was pretty much supported by other brothers.

VAN BENSCHOTEN: And when did your parents marry?

KERNAN: In 1961.

VAN BENSCHOTEN: How did they meet? Do you remember? You weren't there, obviously, but did they talk about that?

KERNAN: [laughs] I think through friends. And, yes, it's something I would like to find out more about. But I think they were introduced by friends. The areas where they lived in the city are not too far apart, not too distant from each other.

VAN BENSCHOTEN: Let's talk a little bit about your siblings. You said you had three. What about your second oldest? What is his name?

KERNAN: The person who's oldest next to me, that's Margaret, my older of my two sisters.

VAN BENSCHOTEN: And what does she do?

KERNAN: Right now she's gone back to do a Ph.D. on early child psychology and education. In between times, she's worked in early education, and throughout, sometimes as teaching, sometimes helping to run a school in London, sometimes helping write educational policy for government in Ireland. Let's see. Actually, she'll be visiting here next week and is going to visit child care centers where we have our children, too, as part of her research.

I think, when she was just going to college, she tried science for a few weeks, but decided that it wasn't for her. [laughs] Sort of following the older brother's lead, but she was able to—She's very decided, and was able to make up her mind right away—something I would not have been able to do—it's not for her. She wanted to do something else.

VAN BENSCHOTEN: Who comes next then, after that?

KERNAN: Then my brother, my younger brother, Colm. He's an engineer like many of the other Kernan cousins. I think, though, he started as a mechanical and structural engineer, and then decided to go into something, I think it's more like cost analysis on construction. Married [Lisa Staines], recently had a child.

My older sister—just to go back to her for a second—is married to a scientist [Filipe Freire] who is a theoretical physicist, lived for a while in Heidelberg and in London, and eventually came back. My brother, Colm, also lived in England for a while and was working. He came back to Dublin. I'm the only holdout so far.

VAN BENSCHOTEN: And then the youngest?

KERNAN: Youngest sister is Niamh, who's a medical doctor, who graduated, went through medical school in Dublin at Trinity [College], and has probably just decided to go into general practice in Ireland. Every now and then I make some sort of half-attempt to give her a taste of research, but she's resisted so far. She's probably the smartest one in the family.

VAN BENSCHOTEN: That's saying something.

KERNAN: Do you know anything about the Irish system of how and whether you get into college?

VAN BENSCHOTEN: No.

KERNAN: It's highly competitive, much more so than here. An awful lot depends on the exams you take when you finish the equivalent of high school, the leaving certificate exams. Basically, those were probably the hardest exams I ever took in terms of both the amount of material you have to deal with and the stakes. You do exams in seven subjects or so, and depending on the level you take them at, your grade, you get a certain number of points. And those points added up to a number, and you buy your way into colleges or a course in a particular university with those points.

So whether your courses be in medical school or dental school or something like that, that's in high demand, you've got to do really well on your high school exams in order to become a doctor, which is not necessarily the way you want to choose a doctor.

Luckily for me, science was not in as high demand as medical schools, which I probably wouldn't have got into there, but she [Niamh] aced her leaving cert[ificate exams] and has since continued to do really well on board exams and things like that.

VAN BENSCHOTEN: So of the three siblings, who were you closest to, growing up?

KERNAN: For most of the time when I was in Ireland, I was closest to Margaret. We're closest in age. Between me and my brother, we're probably temperamentally different enough that we weren't all that— And also different enough in age, five years apart, that— He was the annoying little brother when he was younger, and by the time he was older, we were different enough in temperament that we got on well, but not particularly close. I think my parents are telling me that the reason Margaret and I got on so well was that neither of us were particularly emotionally demanding of the other. It was more like a—

VAN BENSCHOTEN: Hands-off.

KERNAN: Hands-off. In some ways I've been closest since to my youngest sister, to Niamh, and Niamh is spelled N-I-A-M-H.

VAN BENSCHOTEN: Oh, good. Thank you. I would have mangled that.

KERNAN: The thing I regret most about coming to the U.S. when I did was that I missed an awful lot of her growing up. That's the only major loss, the price that was paid for coming over here. But in part because she's still living with my parents at home, I get to talk to her every time I call them, and I call my parents and her more often than I would call my siblings. Every week or so.

VAN BENSCHOTEN: Is she married, by any chance?

KERNAN: No.

VAN BENSCHOTEN: I should have asked this when we were talking about Dublin. Maybe a little bit more in *Dubliners.* Are you a believer in the validity of national types? In other words, is there sort of a quintessential Dubliner?

KERNAN: Probably like many Irish people, I'm a user of national types when it suits me, whether or not I believe in them. [mutual laughter]

VAN BENSCHOTEN: Exactly.

KERNAN: So you'd be glad to play up the stereotype for the sake of the story, but I'll probably react against it in many ways. I don't know that there is. It matters where you come from, a lot. In some ways, when I made— Maybe there was a difference to me when I specified the difference between North and South Dublin, the way that North— South Dublin is typically seen as more privileged, and, you know, on average that's where richer people live and more people will have graduated from college live there. South Dublin, particularly, is seen as less deprived.

In Ireland, it used to be status did not equate with wealth, and there was a certain inverse— There was quite a lot of inverse snobbery. That's probably changed now, maybe not for the better. Being poor is no longer an honorable profession there. There's been some problems over having a split economy. But then, with that said, on the north of Dublin where we were living— And we were kind of the richest part of the north of Dublin, so it was being on the other side in several different ways. Ever see the film, *The Commitments?*

VAN BENSCHOTEN: Parts of it. I haven't seen the whole thing. Why?

KERNAN: That's in North Dublin, and the distinction between North and South is really important.

VAN BENSCHOTEN: Okay.

KERNAN: I think it was that North Dubliners were the— I forgot the—

VAN BENSCHOTEN: Now, when you're growing up, were you cognizant of that distinction, of your place in sort of the social world and hierarchy? I guess I'm trying to ask how class-conscious—

KERNAN: Yeah, Ireland is probably— There was much less of a class-consciousness, I think, than there would be here now. The wealth gradient really wasn't anything like the wealth gradients I see now where I'm living, right here. And status didn't equate with wealth. There was some family feelings. Like some of my mother's family, not just her immediate family, but her cousins, her father's, her mother's sisters, were probably sort of in a— You know, lived in a bigger house and had more pretensions, or which we regarded as pretensions. So it probably did come out in that way. [Unclear]

Mimicry is very much also a feature of Irish— A lot of the way you exaggerate or mimic something is— Probably there is awareness of class-consciousness there.

VAN BENSCHOTEN: All right. We have just enough tape on this to maybe get into this topic about your youth. Describe yourself as a young child, maybe between four and eight, maybe just before you get into the Jesuit school. What kind of child were you?

KERNAN: Scared. [laughs] I think. I don't know, maybe it's just those are the memories that persist longest. I think it's having just been through Halloween. I think I remember being deathly afraid of anything around Halloween time, any masks, anything like that.

Curious. I think I always liked exploring things. I liked to explore anything. I liked making maps of things. Not necessarily very gregarious. Probably a tendency to being a loner all the way along. Very bookish. Read. By reputation, I'm told, I started reading at two and didn't put a book down, haven't put a book down yet. And I'm being punished for it now because my daughter [Ciara Emily Kernan] is turning out exactly the same way. So I had always that retreat available to me, and something I made use of quite a lot.

VAN BENSCHOTEN: What kind of books would you tend to gravitate towards?

KERNAN: Everything. Everything involving ink on paper. Stories. Early on, nonfiction as well as stories early on, and I think books about animals. I think I remember one. Whether or not it's real or it's just memory shorthand, I connect interest in natural history coming out of one particular big picture book of animals.

VAN BENSCHOTEN: And where did you get most of your reading material? Was it something that was laying around the house, or did you go to libraries?

KERNAN: Again, as soon as my bicycle range expanded to include the local library, I was hooked.

VAN BENSCHOTEN: You mentioned the natural history book. Were there other books that were important? And we can go beyond the four-to-eight range. Were there books that were very important in forming you?

KERNAN: Let's see. In fiction, some of the older adventure stories. You know, any of the Arthur Ransome stories, John Buchan later on. Things sort of with a hunting, shooting, fishing, sailing background, which is odd because— Mythology books of all sorts. The whole family's led by my daughter's reading, has read the entire *Harry Potter* series, and they're fun, but I keep thinking how much better, you know— maybe it's parental snobbery—the equivalent books based on Celtic mythologies were; a couple of series that I read when I was younger, which I'd like to find again. I still read in sort of the same areas. I don't know. I'm trying to get my daughter to write down titles of books that she reads starting now, because that's something I wish I had done.

VAN BENSCHOTEN: We're near the end of this tape. I'm going to flip it over.

[END OF TAPE 1, SIDE 1]

VAN BENSCHOTEN: This is tape one, side B now.

We were talking about books, how important they were. You said that you were a curious child. I imagine some of that curiosity, obviously, led you to books, too, but what else were you curious about, I guess? Where did curiosity lead you?

KERNAN: I think just being absorbed in things, without necessarily analyzing them. Curiosity led me to explore, you know, what's around the corner.

VAN BENSCHOTEN: This is where the bike was so important, too.

KERNAN: Mobility. And I liked the feeling of being in a situation where I was secure enough to go where I wanted, bike where I wanted, and yet not know what was around the corner, even exploring. Exploring a built-up suburb sounds odd, but actually it really was— I really did get that sense of discovery, seeing any place or any thing that I hadn't seen before, and wondering how it worked, and wondering how it— Figuring out even things, like on plane trips I love to look out the window.

Just even things like geology, geomorphology. Loved reading John McPhee's books on geology before flying across the U.S. [United States], before driving across it. You get that sense of figuring out not just what's there, but why is it there. The Bull Island that I spent a lot of time on was actually a recent—in Irish terms—arrival. The island formed after they built a harbor wall out in Dublin. The wall was surveyed by Captain Bligh, the same guy whose mutiny [unclear] on the *Bounty.* About a five-mile-long sand island formed behind it, and it still was changing, so one of my projects was on how that had happened and how much changing was still going on.

VAN BENSCHOTEN: And when did you do this project?

KERNAN: That would have been high school, late high school project. Not real research, just more book research, history research.

VAN BENSCHOTEN: This is a point of connection that we have, because I remember quite clearly the day I got my bike, my first bike. And it was precisely— I mean the excitement was there precisely because it did allow me to get away from home, in a sense, and explore this strange new world. And I would do it with my friends as well, and that was also fun.

Was there any great adventure that you would go on with your bike? How far, in other words, did you leave the homestead and head out?

KERNAN: Oh, I think the longest rides I've done were about a hundred-mile circuit from the house in a day.

VAN BENSCHOTEN: That's pretty far.

KERNAN: That would have been off into the Wicklow. That would have been sort of the biggest endurance test, you know. But for quite often I'd done fifty-, sixty-mile circuits in a day. That was through the later teens when I was able to do that, but that takes you down into the really wild country in [County] Wicklow and in south of Dublin. That, again, was the next step

in exploration, getting to places that were not urbanized at all, but looked— That's the way they really could have looked, looked exactly as they would have in Joyce's time and for a thousand years before then.

VAN BENSCHOTEN: Were these bike trips that you went on alone, or did you go with others?

KERNAN: Quite often, more often alone would be usual. But there was a group of people at the school [Belvedere College] that I went to which I eventually nucleated around. There was a science teacher who also liked biking, and kids who tended not to like the team sports, but were still physically active, and biking like that— Bike rides were a good noncompetitive activity. And a sense of camaraderie that I never got, really, from team sports—probably because I was never any good at them—was a part. You could get on group rides like that, and then once we got— A couple of friends and I would go. There are all these youth hostels around Ireland, so you can go on several-day trips pretty cheap.

VAN BENSCHOTEN: I don't know enough about Irish society and Ireland overall, but how popular is biking among the populace at large?

KERNAN: Probably less so [now], it would be. And I'm not sure I would do the same trips because the traffic has got so much heavier, but it was the standard means of transportation. I mean, it's very much so. Irish society, really, if you go back to the late nineteenth century, it was really changed by the invention of the bicycle for precisely that reason. You could court the girl in the next— You aren't just limited to this village and the ones around it, but you can range quite a distance. So I think everybody at that time was dependent on bikes. Ever read Flann O'Brien?

VAN BENSCHOTEN: Right.

KERNAN: There's this whole comic series he has on— I don't know if it was a policeman. There were a lot of policemen, but it was somebody who was so wedded to his bike, had spent so much time on it, and the roads were so bumpy that eventually he melded with the bike and bike molecules became part of him, so he took on the personality of a bicycle and would stand on the curbs with one leg up like this, leaning over slightly. [mutual laughter] So the bike's a definite motif in Ireland. It's kind of a pity now that it's probably more dangerous than it used to be.

VAN BENSCHOTEN: Yes. That's happening, unfortunately, in a lot of cities, too. L.A. is

becoming very difficult to bike in as well. I try it, but I fear.

You talked about making maps as well. What did you make maps of?

KERNAN: Our surroundings. I think I remember a vacation where I went, down in Wexford in the south coast of Ireland, which had this sort of interesting looking beach which had a lot of rock outcrops on it. Me and my sister [Margaret Kernan] and another family, who we coincided with vacations for a couple of years running, got to be— Had one of those vacation friendships which were very close for that, you know, once each summer. I remember first making maps of all that beach, and naming all the rock outcrops and all the features out, and that was our— It was one of those "This is our private land," sorts of things.

But I've always liked maps, always liked just reading them. Like them as the way they convey information and look good, and going from making the transposition between maps and landforms, that's always appealed.

VAN BENSCHOTEN: And now, I mean, you're a geneticist. You're mapping genes. [laughs]

KERNAN: Yes. And that's still something I like to do probably too much. I like to draw my maps over and over again. I probably spend too much time making them look pretty and that craft aspect of things.

VAN BENSCHOTEN: Exactly. So there's something aesthetic, too, about these maps, obviously.

KERNAN: Absolutely. Very.

VAN BENSCHOTEN: They're more than just simply tools to navigate an area.

KERNAN: How much of the aesthetics of the natural world you can transform into a map, and how much you can't. That's always been strange to me, both in science and data, that when you transform anything in nature back into data, you're throwing away almost everything, and in some ways the data becomes the more believable the more you throw away, but actually can— Especially [when you] try to quantitate things, you're throwing away so much information in order to make— But the more you throw away, the more believable it becomes, the less you know.

VAN BENSCHOTEN: Do you believe that the more you throw away, therefore the more beautiful it is?

KERNAN: No.

VAN BENSCHOTEN: No. Okay.

KERNAN: No. And in some ways, that's sort of something, a problem that's stuck with me. When I was doing high school science projects down on the Bull Island in the salt marshes, I could just sit and absorb things and not analyze them, and for way too long I wouldn't get anything down, wouldn't get anything recorded. I can just sit and watch a fly behave for way too long. [laughs] But that said, I do really like elegance in presentation. There's two books there, Edward Tufte's books on visual presentation of data. Those are among the bibles on the shelf.

VAN BENSCHOTEN: That's interesting, because I was going through and I was reading some of the letters of recommendation that were sent to the Pew [Scholars Program in the Biomedical Sciences] when you were applying for them, the scholarship, the grant, rather. I forget now who wrote this, but one of the things that they pointed out was it wasn't simply this article, these experiments that you had done and the findings that were remarkable, but it was also the way, the method that I guess you used, how you got to these results as well, and how they were presented. So it's interesting now that you emphasize presentation as well.

And when you say beautiful, you know, a map is beautiful, you talked about elegance, but what else goes into what makes a map beautiful?

KERNAN: Accuracy.

VAN BENSCHOTEN: Accuracy. Okay.

KERNAN: The fact that you can trust it. Or if you can't, it's not a good map. The fact that you can— Or sometimes what's very important, and in some ways what I like about drawing genetic maps is how you can clearly show what you don't know, as well as what you do, and make the distinction between the two clear. That's very important when you're, say, doing positional cloning, trying to run down the location of a gene; you know, not clearly showing that you don't know— We have these genes in the interval, but you don't know what the order is, and you won't know until you do this experiment. Almost always, yes, I have to draw things out. Yes, you can pick out many of the figures. Here is something that I found useful. This is a— I'm

pulling out maps, just the sequence of a protein that we worked on.

VAN BENSCHOTEN: So [that] people listening to this tape know what you're doing, you have a green notebook and you've opened it up. And these are what, sequences?

KERNAN: This is just four versions of the sequence of the same protein, just as it might be printed out in a paper as a primary sequence, long list of letters, each one standing for an amino acid. But what I did was sort of printed out everything in a light gray, and then just highlight certain amino acids in colors, and just from the patterns that the colored letters made particularly—say cystines in bright pink against everything else in a dark gray—enabled us to identify some motifs that we couldn't have otherwise. Eventually the computers were able to do it all, but we beat the computer to it in that instance.

VAN BENSCHOTEN: So you can see at a glance—

KERNAN: So you can see what I was able to pull out.

VAN BENSCHOTEN: Yes. Interesting.

KERNAN: And picking patterns out of text like that is something I like doing. One of the accomplishments I'm proudest of, again, was something very like that. It was on a protein that I worked on for my thesis work. Initially, it was a longish sequence, didn't look like anything. The computer programs at the time weren't able to pull out small, dispersed motifs, but something about one of them struck me and I was able to identify it as similar to something I'd seen someplace else, and eventually work out pretty much on paper by hand the fact that it belonged to this particular family of proteins RNA helicases. Then later we could show the same thing by computer once we knew the answer.

VAN BENSCHOTEN: Before we move on, hiking. You said this was important. When does hiking become important? Do you sort of combine it with your biking?

KERNAN: Yes. Quite often I do combine hike and bike. So the ideal day for me, it's almost an aesthetics thing of having a circuit that would involve biking out from the city, finding a route to hike along that would involve a ridge with a horseshoe shape, so you could leave your— Stash your bike someplace, hike along a ridge so you get good views all the way along, come back to the bike, and bike home by a different route, and finding, discovering routes like that to involve sort of map-reading, as well as getting out, being sometimes by yourself, sometimes not. More

often, by myself.

VAN BENSCHOTEN: And how much hiking and bicycling do you do now? Is it still an important part of your life, or have you had to leave that behind?

KERNAN: Much more, I'm afraid, in the intention than execution. It's hard. The most frustrating— Long Island's [New York] a great place to live except for that fact. The highest point in the island is, I think, a landfill. [mutual laughter]

VAN BENSCHOTEN: How unfortunate.

KERNAN: Even though, you know, I like getting out into complete wilderness, semi-urbanized or even junked-up areas have always appealed to me, too. So the salt marsh in Dublin was kind of close in to the city and it was regarded by some people as almost a dump, but you find great wildlife there, too. There's an image that stuck with me of, I think, finding, coming on a sparrow hawk sitting on top of some garbage that I came upon once. It's one of those—

VAN BENSCHOTEN: Memorable.

KERNAN: Memorable.

VAN BENSCHOTEN: You've talked a lot about hobbies that you've had, interests that you've had as well. Were there any others, though, that we haven't brought up yet that were important and should be mentioned?

KERNAN: Sailing. It's not something I ever expected to be able to do, but my parents [Thomas Kernan and Veronica Perry Kernan] surprised me with a gift of some sailing lessons at a place called Malahide, a bit north of Dublin there. Oh, I liked that so much, and then I picked it up again when I was in graduate school in [University of Wisconsin] Madison, because the campus there is beside a lake, and they've got a little great sailing club.

VAN BENSCHOTEN: How about when you were in [University of California,] San Diego? Did you do any sailing there?

KERNAN: No. Oddly enough, no. I don't know whether it was just— Again, the location

wasn't very— In Madison you could be in the lab at four or five, decide, "I'd really like to get out on the lake," and do it within fifteen minutes. I never got it together as a postdoc[toral fellow]. By then work was already becoming overwhelming, though, yes, I regret it now. Or even more, sea kayaking is what I should have done there. So that's still an ambition for the next year or two, is to get back in the water. It's easier here.

VAN BENSCHOTEN: Yes, I'd think so.

KERNAN: Good canoeing, good kayaking.

VAN BENSCHOTEN: And how about music? Was music important at all?

KERNAN: No. Which is odd because I married somebody [Karen Kernan, née Kwik] for whom it is quite, very important, and my daughter [Ciara Emily Kernan] has now started cello lessons this summer, and it's really great to have music in the house.

We were all exposed to music. I don't know why my parents didn't push me harder at it. Maybe I was very resistant to it, but I like listening to it. I had made, you know, a couple of high school attempts at, or grade school attempts at recorder lessons, things like that. Nothing really stuck.

VAN BENSCHOTEN: Your parents give you these sailing lessons. How did that work? Did your parents tend to suggest things to you, or did you suggest things to your parents when you were a boy growing up, or both, about, you know, if you wanted to pursue some interests or whatever? What was their role in that?

KERNAN: For the most part, I decided what I wanted. I was hard to push, and when I wanted something, I went for it. I think sailing— I forget exactly. There was a slight connection there because my dad [Thomas Kernan] had worked mostly in marine insurance, and there was always stories about boats and ships. He had a sort of an interest that for him was left kind of unfulfilled in— I remember being taken to boat shows and having him sort of look longingly at boats which he didn't feel like he could afford then. I think that one of his brothers did buy a little dinghy boat and did some sailing on it then.

VAN BENSCHOTEN: Let's talk a little bit about natural history and how important that is. How can I put this? I guess I'm interested in when your first interest in science arises. How scientific, I guess, was your interest in natural history? [mutual laughter] That's one way to put it. I don't know.

KERNAN: Well, as I was saying, in some ways it wasn't scientific. It was as much aesthetic as scientific right from the start, and in some ways it still is. Last month I was— To put it kindly, we manage our property for biodiversity rather than taking care of our yard. We've got lots of mushrooms and things, so I was sitting out in the back, picking all these weird-looking mushrooms, and my daughter saying, "What are you doing?" I just liked how they looked and wanted to find out what it was that was growing. So it's very much sort of that curiosity was inseparable—I think for me— from science. It's all part of the same thing, and I couldn't point to a moment when I'd say, "Now I'm a scientist."

VAN BENSCHOTEN: Ta-da! [laughs]

KERNAN: It always seemed to be part of the same thing, and still is.

VAN BENSCHOTEN: Let's talk a little bit about school. What are your earliest memories of schooling, your schooling?

KERNAN: Being scared of Halloween. [laughs] It goes way back.

Schooling. I can remember, I think, isolated images probably from when I started school, and would have started at four, age four or five. Vague memories about kindergarten before then. From four till eight I was in the local school, and have one or two isolated memories of that, but more with the perils and pleasures of recess. Having to wear glasses from an early age, being short-sighted from quite early on, I think almost the whole time I was in school, from age four or five. Probably put something of a slight crimp in, you know— Wearing glasses early on changes the way other kids interact with you, and the image they have of you, as well as your own image. It affects you in turn. So even though I had a natural bent towards being bookish, that was probably reinforced by other people. So, these four eyes.

So a big change in schooling came for me then when I switched to— I think in part this was from my parents. I think in part because they felt that the standards at the local school weren't high enough, and that even though I had been jumped a grade very early on, so I was put into a grade with kids older than me because they thought I was academically able for it, but I think I wasn't up to— You know, that's a huge difference when you're four or five or six years. It's a huge amount.

VAN BENSCHOTEN: It is, yes.

KERNAN: But I think they wanted an environment where I could be in a class that was appropriate to both my age and my abilities, so I switched to Belvedere, which is this long-established Jesuit-run school in the city center, where James Joyce went for [unclear].

VAN BENSCHOTEN: Exactly. When you mention these names like Malahide, I mean they're always popping up in Joyce.

KERNAN: Oh, yes. Dollymount Strand.

VAN BENSCHOTEN: Right.

KERNAN: That's the front side of the Bull Island, the sand island that I did all— I hung out in the back side where all the salt marshes and the birds were.

VAN BENSCHOTEN: You mentioned Sandymount, or am I getting that mixed with— Sandymount Strand?

KERNAN: Sandymount Strand, yes. That was the strand near my grandparents' house. That's where they lived.

VAN BENSCHOTEN: And there was a tower, the martello tower.

KERNAN: There are several martello towers along the coast. Yes. We knew all of them. They're all still there.

VAN BENSCHOTEN: So I take it, then, that you appear to have read Joyce as well.

KERNAN: Oh, yes. It was required.

VAN BENSCHOTEN: Was it?

KERNAN: Actually, it wasn't required in school. At that school, he'd left enough of a reputation behind that it certainly wasn't required that you read Joyce, and he wasn't mentioned.

You know, you'd see suspiciously "James Joyce was here" carved into the Formica under the desks. [mutual laughter]

VAN BENSCHOTEN: A little humor, like our "George Washington was here."

KERNAN: Of course, I think they make more of it now that he's become distant enough to be respectable, even though he was probably visiting the brothels while he was at school.

VAN BENSCHOTEN: Exactly. And *Ulysses* was banned.

KERNAN: You know, the school is quite near— It was in the middle of the city, which was north side of the city, too. It's again, you go to the north-south. The north side of Dublin used to be, way back, early, mid-1700s, was the rich part of the city. Then they built the— I think the lieutenant governor at that time moved south. Everyone followed him. The north side fell into disrepair and became the slum area and the red-light district earlier on. But Belvedere, the school, the main building was located in one of the big old houses, and got its name from the earl or whoever it was that lived there before.

So, it was an odd situation. It was a fee-paying school with quite a rich—by Irish standards—student population, set down in the middle of what are still pretty deprived areas.

VAN BENSCHOTEN: I'm a complete Joyce fanatic, so I'm going to throw out one more word and then we'll move on. Clongowes.

KERNAN: Clongowes Wood College, yes, it's another Jesuit school. It's further out from the city. I've been there a couple of times, I think when they were playing rugby matches.

VAN BENSCHOTEN: And was Joyce something that you had to read, and so read, or was Joyce something that you read avidly?

KERNAN: I wouldn't say I read Joyce avidly. I never found him all that easy, nor natural, until I read it, heard it, again, sort of thing— I've probably read much more Joyce since coming to the U.S. [unclear].

There are many things that happen to the Irish when they come to the U.S. The main thing is, they become more Irish. I remember in San Diego, for instance, they had a reading of— I did part of a reading of *Ulysses* on Bloomsday.

VAN BENSCHOTEN: That's great.

KERNAN: So, I enjoyed it. When you start reading it aloud, then it gets so much better.

VAN BENSCHOTEN: That's what I've heard. I've heard, too, that people who have trouble with *Finnegans Wake,* if they were simply to read it aloud to themselves, or have it read to them, it doesn't necessarily become instantly clear, but it does make more sense, and it is more fun. It's more rhythmic and beautiful.

KERNAN: And if you can do the accents right, and get them right and hear them right. I'm not sure it makes any more sense, but it's—

VAN BENSCHOTEN: It's musical. It's more musical.

KERNAN: Yes.

VAN BENSCHOTEN: Talk about the transition, then, from the public school to the Jesuit school. How was that transition for you?

KERNAN: On the whole, it was great. It was exactly the right move to an environment where the things I was good at were a little more valued, though team sports was never— Team sports was also valued there even more, and I was never good at that. I could always run fast enough, which was just as well. [mutual laughter] On the rugby field.

VAN BENSCHOTEN: That's always a useful skill to have when you're going through school.

KERNAN: I think, yes, it was clear, when we all hit puberty and everybody else got bigger than I did, it was time to drop rugby as soon as possible, as survival. I played a bit of squash, never well enough to get good at it.

VAN BENSCHOTEN: How about academically? When you get to Belvedere, what are your interests at this point? And you can take it right through, if you like, you know, middle school or high school.

KERNAN: So it's primary school till you're twelve, and then secondary school, six more years. Primary school would have been, yes, books, books, books, and more books.

VAN BENSCHOTEN: And again, sort of polymathic, moving in all directions?

KERNAN: Stories. Stories about magic, stories about that childhood sense of— Which I can see in my daughter that she's soaking up then. The reason *Harry Potter* things are so popular is because you get that sense of imagination and empowerment, of being able to feel you could do anything or travel anywhere.

What else? Yes, some natural history, looking for stones, looking for fossils.

VAN BENSCHOTEN: Did you collect insects?

KERNAN: No, oddly enough. Insects were— I don't know. I think the insects are something that's much more abundant and much more flamboyant here than in Ireland.

VAN BENSCHOTEN: But you had collections, though?

KERNAN: Fishing. Fishing was big. Sorry, you asked about academic interests. We sort of wandered out of the classroom again.

VAN BENSCHOTEN: Well, that's good, too, though. Were there teachers, for instance, or mentors that you were meeting who were important in shaping you at this point?

KERNAN: Yes. Well, let's see. One thing that was always difficult, and still is difficult, is expressing myself. And I always found writing to be an incredible labor, rather than drawing. So I liked art. One of the things I liked best—I felt we never did enough of, because we were tracked, if you're academically able, onto a more academic track—was metalwork, working the shop, working to make stuff with my hands. I loved that, and still find that the most satisfying. Even designing something. Making, having something like that made. [pointing to apparatus] I didn't make that one; the people in the shop—

VAN BENSCHOTEN: What is this again, for people who won't be able to see this?

KERNAN: It's machined pieces of Perspex, assembled and put together, and what it makes up is a fly motel, mating chambers for fruit flies.

VAN BENSCHOTEN: That's great. It's sort of a rectangular block, right, that has these sort of—

KERNAN: With twelve little chambers. The idea is, you can open and put flies into each one individually without disturbing the others.

VAN BENSCHOTEN: That's great.

KERNAN: You can then use it for timed matings of fruit flies.

VAN BENSCHOTEN: And you designed this yourself.

KERNAN: Yes. Making, just crafting little, not very technical bits of— Nothing involving calculation or, you know— But just hand work is something I enjoy a lot. So I felt like I could have done a lot more of that and gotten good at it, but never did at school.

Science became emphasized as a subject once we got into secondary school. That's staying within Belvedere. I had kind of the last half of primary school, [age] eight to twelve, and then secondary school, twelve through eighteen. So you changed buildings, but stayed within the same school. I think it's another aspect of stability that seems to be different also between Ireland and the U.S. You tended to stay in the same class, same school, same school building, all the way from [age] twelve, or in my case eight, up until you left school.

VAN BENSCHOTEN: So, same cohort.

KERNAN: Same cohort.

VAN BENSCHOTEN: Mixing with the same people.

KERNAN: For better or worse. And friendships that were then stable, also because other

people didn't move around as much. People's fathers or families didn't leave to go find work elsewhere. They tended to also be stable that way.

Let's see. Where were we? Science. Yes. A couple of great science teachers. One, a guy called Michael Grehan, who taught us the basic science classes, but also organized those bike rides that we went on a lot. I don't think, it conveyed anything particular about any particular science topic, but just the value of it and the worth of doing it, and the feeling that this interest that I had was worth doing.

A chemistry teacher, Benji [Benjamin] Fenton, who was a real character, had a great sort of gruff way of speaking, but a real sense of style and panache. I remember being crouched, hunched over a desk, you know, crouching down trying to get my eyes level with a graduated cylinder. He told me, "Oh, you don't do it like that. You do it like this," and swept up his arms and poured something into it. So we got a sense of the importance of elegance in the way you do something, as well as just getting it right.

I think, though, still, even through school science, my interest in the projects that I picked up and entries that I did in science fairs were still self-driven, based more in natural history, which is not something I really got from the science classes in school, and pretty much a lot of the stuff was stuff I would bring into school rather than get from school. So, a lot of it was self-motivated.

VAN BENSCHOTEN: Well, if you would, talk a little bit about those projects. You already mentioned one. You talked about Sand Island.

KERNAN: I think I did two that were— One was on looking at just feeding habits of birds at Dollymount, on the Bull Island. It's home to a lot of wintering birds, thousands and thousands of wintering waders and ducks, and lots of different species.

A great place to— Because it's sort of semi-urban, you can bike along one side and there was this busy, heavily trafficked road, and with your back to the cars you were out watching this pristine-looking, beautiful—to some eyes—salt marsh with all these birds on it. So it was a great way of observing a whole lot of this huge area all at once, from a bicycle.

So it was ideal. And you could count, see, because it's open ground, and birds, ducks and waders are pretty easily identifiable. I'm not that great a bird-watcher. I can't do the little brown bird in a tree [at] a hundred paces, or whatever it is. I like my birds labeled clearly. But you could observe them all, and what I tried to do is to figure out whether the way they moved from one part of the island to another and to feed at different times was tied to the tides, or to the time of day. I never really got— What a real ornithologist would do by focusing on one species and doing it properly at all times, getting all the data points at all times of the day or night— I was trying to cover everything at once, and so it was a very overall description. So, lots of time was spent just looking through binoculars instead of collecting real data, but it was fun.

And then another was, same place, same island, just looking at basically just a transect of the island and trying to go from the parts that had been formed first to the parts that had grown up more recently, and doing things like soil analysis and species counts.

VAN BENSCHOTEN: You've mentioned already John McPhee. This sounds like something an early, budding John McPhee would have done. [laughs] That's interesting.

KERNAN: Maybe. Except he would have written about it much more eloquently than I was able to.

VAN BENSCHOTEN: Okay. Any other projects that you want to talk about?

KERNAN: Yes. So, I think a big shift in scientific viewpoint, I think, come from that because obviously I was going— That was high school, late high school. [Unclear] was sponsored by the National Ireland Young Scientists Competition and I won a prize in that, I think for both of those projects, the equivalent of the Intel [Science Talent Search Award], Westinghouse science competitions here.

That was towards the end of high school, so I finished up that with obvious interest in ecology, natural history, field biology. Then went in [to college] with that interest, got enough of the points in the leaving cert[ification] to get into biology in Trinity College in Dublin. And then—because you specialize early there—before I had to decide whether to stick with that interest in ecology of which there was a department there, or then found myself— You know, I don't think this [ecology] really works as a science, or at least, I don't know whether it was a sense that the error bars were just too enormous, that you couldn't— This sort of precision and accuracy and certainty and knowledge of what you know and don't know, that I could get from maps but wasn't to be found there, but was to be found in something like genetics. So making that switch was actually— That was sort of a shift in viewpoint that I remember.

VAN BENSCHOTEN: I'm sorry. How soon again did you have to make that shift?

KERNAN: That would have been probably midway through the second year of college. Sophomore, in U.S. terms.

VAN BENSCHOTEN: Before we get you to Trinity, though, I wanted to ask a little bit about friendships. You already had talked about how your cohorts, you have a cohort, basically. You're with them for a long period of time. How important were— Did you develop strong

friendships?

KERNAN: The first cohort was the kids who lived right in the neighboring houses. This was a street of semi-detached houses which is your typical Irish suburb, which means that you've got houses that are not row houses but separated, two pairs of houses joined by a common wall and then separated from the next house by a couple of garages, with tiny little walled-in yards. Lots of streets of those. So within twenty yards up and fifty yards down the row, there is a group of three or four of us that we'd seen each other every day since the time we were three or four. You didn't choose your friends. They were just the people— Of course they were your friends, because they lived beside you. Once I went to Belvedere and started commuting into the city center rather than going to school with them, that connection broke. Some of them, though, two of them eventually also came later on into the same school.

VAN BENSCHOTEN: How easy was it to keep up these friendships, sort of the geographical friendships you're talking about, after sort of the tracking sets in and people are going to the city center or going to Belvedere or whatever? Or is that tracking at all? Am I misusing that term, sort of?

KERNAN: Not with tracking, but once the circumstances changed. They're still friends. One guy died early when he was about thirty, but the rest of the other cohort: one's a salesman; one's a policeman. I'll still make sure to make contact with them, with one of them at least, to which one was closest, every time I go back. Once we've done a couple of hours catching up, there's not a lot much more to— Apart from talking about kids, which is inexhaustible.

I'm conscious of how disorganized a speaker I am. I'm trying to do this in an extended way.

VAN BENSCHOTEN: Well, it's hard to pull it together.

KERNAN: You were asking about how moving to the different school changed the friendships. Yes, it broke it up quite a bit. I ended with a new circle of friends, but still, you know, weekends, evenings, I'd be back from school at about 3:30 in the afternoon, at least in the summer. Summer days are really long in Dublin, so there was plenty of time left for—

VAN BENSCHOTEN: Are you still in touch with some of this cohort that you're talking about?

KERNAN: Very early cohort, from Belvedere, actually, no, oddly enough. Or I'm not sure it's if it's oddly enough. I think the cohort that I might have kept up with the most would have been people in a drama club I was in. There were some people there. But I think, no.

It would be really interesting to go back to a reunion—I've missed the last major reunion they had—and find out they're— I think the guy that was the equivalent of a valedictorian made one comment, I think, in one of his— I don't know if it was in the equivalent of the address that he would have given, but, "I hope that for as many of us as possible, we'll never say that these were the best days of our lives." [mutual laughter] You know, they shouldn't be. It'd say your life's pretty sorry if high school was the best day of your life.

VAN BENSCHOTEN: It's funny. I was talking to a friend about the movie *Stand By Me*. Have you seen that film?

KERNAN: Yes.

VAN BENSCHOTEN: It's about a small group of children who grow up. I guess they're about maybe eight, or between ten or eleven maybe in the film, ten and twelve. But anyway, at the end of the film one of the characters says, "Well, now I know that these were the best days of our lives." And when I hear the end of that film, I feel so sad. [mutual laughter]

KERNAN: Right.

VAN BENSCHOTEN: "Don't say that."

Let me flip over the tape.

[END OF TAPE 1, SIDE 2]

VAN BENSCHOTEN: All right, this is tape two, side A.

We were talking about schooling, cohorts. I wanted to ask were there any other critical events. I think you've covered quite a few of these, but were there any other critical events that I'm not picking up by talking about schooling and talking about cohorts and other things, events that were important in developing you? These could be, you know, sicknesses, accidents. They could be parties. They could be anything at all. I'm just sort of throwing out a net, I guess.

KERNAN: Let's see. Nothing comes to mind. So in one sense, you know, I'm wondering now if my memory, I find, is so much at fault; but it's associative, so there may well be things that occur later on as they arise. But, overall, my impression is that no— Not really precipitating events. I feel like, all along, I was this sort of— What got me to do science was something that was there all along, that it was just a matter of gradual development, that I always had that sort of goal.

The way determining events— Maybe just because they're more recent, I have memories of things or decisions that I've made more recently, but not early on. In fact, almost always I often find that when I've—you know, faced some critical decision—actually cleared the decision, I have the impression that, actually, the decision had already been made a long time previously, that it was more a matter of coming to express it.

And the process of making a decision is something— Even in terms of neurobiology, that's one of the questions, one of the deep questions, that I've always found interesting, and I doubt that I'll ever get to approach it experimentally: what actually happens when you make a decision, and what does that mean?

VAN BENSCHOTEN: It's interesting that you find that interesting, too, because I often think of it as tumblers that sort of fall mysteriously into place. We don't quite know what the combination is, but there they are. One day they fall into place, the lock opens, as it were. We come to our decision. That's not a very, I know, scientific explanation, but it's the best I can do.

How about religion? I mean, Ireland, obviously—

KERNAN: I was kind of wondering when we'd get to religion. [mutual laughter] For interviewing somebody Irish, you've held off for a long time.

VAN BENSCHOTEN: Exactly. So I've got to ask. What was the impact of religion on you and your family?

KERNAN: Less constraining than you might expect by reputation, by stories of Irish jokes, and the fact that I was educated in a religious-run school [Belvedere]. In fact, pretty much all Irish schools, whether they're public or not, are, in fact, religious-run. They used to be. The local parish priest was the school manager, even in a public school. The separation of church and state has not caught on, really, there. It's happening now, but not— For other reasons.

We were reared Catholic. My mother [Veronica Perry Kernan] and father [Thomas Kernan] were and are—they would call themselves—devout Catholics, probably. I'm not sure they would use the word *devout,* because it would smack of a little too much pretentiousness for an Irish person to express it of themselves. That said, they let us know there was enough of Irish

in them—and Irish Catholicism has always had this strain of contrariness and skepticism—that, yes, we're devout Catholics; yes, we obey the Pope; but you don't really want to go along with everything he says. [mutual laughter]

VAN BENSCHOTEN: That's interesting.

KERNAN: Who was it said the ability to hold two opposed ideas was either the sign of first-rate mind or the sign of an Irishman? [mutual laughter]

VAN BENSCHOTEN: That's interesting.

KERNAN: So the Irish have always— It's a strength, I think, usually a strength, that you can hold two— Frequently required that—if you regard yourself a Catholic individual and yet function in the world—you'll hold opposed ideas in your mind at once.

I no longer consider myself Catholic, but I certainly did all the way through high school and even sort of got in— There was a what was called the Charismatic movement in the Catholic Church, which was sort of an emotional, sort of touchyfeely approach to faith that was, in part, a reaction against the rigid, punitive strain of Catholicism, and I got involved in that for a while. In some ways, I sort of went as far as I could to answering emotional needs that weren't otherwise being met, around the time I was leaving school. But it couldn't— As far as any time it would come into conflict with that sort of sense of curiosity, need to explore, need to figure out how things really worked, i.e., science, science won, hands down.

But I'm not sure, again, partly because I'm Irish and can be contrary, it never really came to a conflict. I did not have any dark nights of the soul wrestling: it's this or that. When I came to the U.S. [United States], I think I was still going to mass, at decreasing intervals, but almost as much for a sense of identity for the— Replicate the rituals, and even still, I still do that. And I still think that I wouldn't regard myself as Catholic. I would still want to be and want my children to be exposed enough to religion so that it doesn't seem something that's alien, even though the Catholic Church does seem to be pretty alienated or alienating these days.

VAN BENSCHOTEN: You have a daughter.

KERNAN: I have a daughter [Ciara Emily Kernan] and a son [Thomas Piers Kernan].

VAN BENSCHOTEN: Okay.

KERNAN: My daughter's seven, my son's four.

VAN BENSCHOTEN: And is she being brought up Catholic?

KERNAN: No.

VAN BENSCHOTEN: Okay.

KERNAN: No. She was baptized, almost because of grandparental pressure, and it's a big topic now. For us, the conflict arises there, you know, because that's when you really have to decide, make a conscious decision, "What are we going to do?" So far, we're saying no. When it comes down to it, I just can't tell my daughter something I don't believe in. That goes against the grain. Yes, family tensions have arisen out of decisions we—

VAN BENSCHOTEN: But not between you and your wife [Karen Kernan, née Kwik], but between you and your parents?

KERNAN: No, between me and my parents.

VAN BENSCHOTEN: Is your wife, by the way, Catholic?

KERNAN: No. She has described herself sometimes, or I describe her, as the least spiritual person I've ever met. Maybe that's why I married her, but she's the daughter of a Presbyterian minister, or somebody who used to be a Presbyterian minister. And her parents are still quite very religious and practice, and they are involved very much in church work, though he is now a combination of a Presbyterian minister and a nuclear physicist, so that you don't want to mess with the spiritual and temporal powers.

VAN BENSCHOTEN: When you return to Dublin and you're with your parents, and Sunday morning rolls around, do you go with them to church or do they go to church still?

KERNAN: I might go, but probably not. I might go just to, you know, to be in the same church again, just to see faces. I would not—say, if it came down to am I going to go out, you know, plan a trip, a hiking trip, or something that I might try and fit into a trip there—not do it [the

trip] because it was Sunday or anything like that.

I've wondered, you know, whether they've become less— They were never dogmatic, and, like I said, they always had a grain of skepticism and allowed us— We had plenty of arguments, but usually we just let them lie. Religion was never a constraint or a burden. It was just another way of looking at the world, which eventually I found to be incompatible with the scientific worldview.

VAN BENSCHOTEN: Do you feel, when you were growing up, that your parents had expectations for you in any way?

KERNAN: Yes.

VAN BENSCHOTEN: In what way?

KERNAN: I was obviously smart and bookish and able to think, and they really did, and do, believe that you should use your talents and that I should make use of them. Of course, you only realize how frustrating and annoying you must have been to your parents when you see the same thing happening in your own children, so I would have appeared to them at times as, you know, frustratingly withdrawn, uncommunicative, probably to the extent where I didn't seem to care about any of the things they were trying to push me to do. But, yeah, I think my mother, in particular, I think, was ambitious for me.

VAN BENSCHOTEN: How did that express itself? How did you know?

KERNAN: Because she'd be always after me. [laughs] "Have you done this? Have you done that?"

VAN BENSCHOTEN: Making sure you did your homework?

KERNAN: Making sure I did my homework.

VAN BENSCHOTEN: Let's get back to Trinity College. When you entered Trinity College, by the second year, as you say, you had to choose, pretty much, your major. That's what we would call it in America, I guess. Describe a little bit of your time at Trinity College, both sort of academically and also socially. Maybe one way would be what were some of— Were you

part of groups or fraternities?

KERNAN: Yes. Nothing so organized as a fraternity. Fraternities, to Irish eyes, just seem so strange, so weird. The idea of sticking a label on yourself like that would just be not done. But that said, however, there were things that functionally were fraternities, or fraternity plus sorority, a mixed group of— I mean, one of the big changes was, "Gosh, I get to hang around with women," because Belvedere, of course, was an all-male school. In biology in particular, you know, there was probably more women than men in it, and groups nucleated. There was a pretty close-knit group that nucleated certainly once— That socially functioned extremely well as a social group. It was almost a matter of social survival, you know. Parties would be organized to make sure that nobody got left out. Lunches would be eaten together, always in groups. This seems, you know, in some ways, normal, but I found, for me, that was the place I felt I really belonged, both from the fact that now I was studying what I really wanted to study, you know. Leaving cert[ification]— You were taking seven or eight subjects, many of which I found difficult, only one of which I really was both good at and wanted to do. So once I had that focus and once I had a social world that I found more congenial than an all-male, somewhat sports-oriented school, then I felt I belonged much more so, and probably has— You know, those friendships have tended to outlast the ones that I made at Belvedere.

VAN BENSCHOTEN: Now, were you starting to date at this point or had you dated earlier?

KERNAN: No, no. All that was way, way retarded, by American standards.

VAN BENSCHOTEN: How about academically, though? I mean, you feel at home here. You've got your focus of research, or at least your studies. Are you coming upon teachers or mentors who are important here?

KERNAN: At Trinity?

VAN BENSCHOTEN: Right.

KERNAN: Yes, some, though the lecturing style there in some ways is more distant, and your interactions—social interactions, learning interactions—were much more among your peers. Lectures were very much— For the first two years, it was a larger group, it was all of the biology students together. Most lectures were, you know, you'd sit down and listen to a lecturer for an hour and take good notes and then try to work them out yourself. Smaller group interactions came in— There was a tutorial system and you were assigned a tutor who was, you know— Among their duties was to bail you out should you get in jail. That didn't happen. But,

actually, for one reason or another, I never felt the need to make use of it. It wasn't necessary. I don't think it was a science faculty person. It was just somebody that somebody assigned.

VAN BENSCHOTEN: So, sort of like a big brother, kind of?

KERNAN: The intention was just as an academic advisor.

Yeah, there were good lecturers there, but at that stage you were looking at people by how effectively they could convey information. So I think back to those days when I'm now trying to do the same thing for large classes here at [State University of New York at] Stony Brook, and trying to remember what made a good lecturer then, which was exactly many of the qualities that I didn't have then and have had to pick up. And there were a couple of very energetic lecturers. I remember being impressed and continually have learned how important energy and stage presence is for a good lecturer, you know, and how you can have a great mind and still be totally— Turn a whole audience off, if you don't put it across with enough oomph.

VAN BENSCHOTEN: That's true. Were you doing lab work at this point at Trinity?

KERNAN: No. The first two years tended to be classes, large classes. There were some labs, but they were more like demonstrations, and no research. Once you were in your third year, though, then you got into it, specialized, and then you were tracked into small departments, with much smaller, I would say about a group of twelve undergraduates, which would function much more like a first-year graduate class here than an undergraduate class. That is, we were given desks in a room in the department, would have small-group lectures from faculty members in that department. So there, yes, you got a much stronger impression of individual faculty.

VAN BENSCHOTEN: In what year did you get— I couldn't find this. In what year did you get your B.A. at Trinity?

KERNAN: In 1984. So, that's the final degree. That's the primary degree, and that would have been at the end of the fourth year. So, for research experience, one of the reasons, probably, that I picked genetics in particular to go into, was because they were running this program where you got to go to, usually, the labs of alumnae, who'd graduated and gone on to do research in other places, frequently in the U.S. [United States], and it was a chance to go into research between your third and fourth years. So my first real taste of research was as a summer project in Cornell [University].

VAN BENSCHOTEN: Oh, really? Okay.

KERNAN: Where there is an Indian guy [Mittur Jagadish] who'd been a graduate student, I think, at Trinity and was working as a postdoc[toral fellow] in a large research group at Cornell.

VAN BENSCHOTEN: So at that point, you're sixteen, seventeen?

KERNAN: No, no, no. This is at college.

VAN BENSCHOTEN: No, I'm sorry. You're twenty-one, twenty-two.

KERNAN: Twenty-one, yes.

VAN BENSCHOTEN: Yes, get my math right, here. And talk about that experience at Cornell.

KERNAN: That was great. That was expanding the range again, to answer two questions. You know, do what I like—research—but also that actually was the first extended time living away from my house, which seems incredibly late, but since I was living at home all the time I was in college, though for much of the time towards the end I was basically just sleeping there and that was pretty much it. There I was just first living independently and figuring out whether I liked lab research. So the answer was, yeah, I liked both, and liked being in the U.S. as well.

VAN BENSCHOTEN: And what did you study? What was your focus?

KERNAN: It was the Boyce Thompson Institute for Plant Research, which was, I think, a privately funded plant research institute on the Cornell campus, where they were doing a lot of work on nitrogen fixation, which is done by a symbiotic bacterium, *Rhizobium,* in root nodules of legume plants. I think they wanted it for genetic manipulation of that bacterium. They wanted to try and isolate the rec-A gene, the recombinase, from that bacterium, and I was trying to do it by complementation, testing transformed *E.[Escherichia] coli,* rec-A deficient *E. coli,* with bits of *Rhizobium* DNA. And it didn't work. I had a lot of fun trying, though, trying to do it.

One thing that I remember— There were a couple of significant things. One was just the impression that the director of the institute, a guy called Aladar Szalay — I don't know if he was the director of the institute, or just of the large group with which my immediate supervisor was working. He was a big scientist in his field and was asking me, you know, "What did you get? What happened?" And it was my first taste of having to produce a lab result that really

wasn't just, you know, for purposes of getting a grade, but because they wanted to know. Although I was desperately inexperienced, you know, if I produced anything, that result would have been worth just as much as if it had been produced by a real scientist at the place.

It was that sense of the scientific world as an egalitarian world that appealed me to a lot; where data is data. It doesn't matter who produces it. In fact, it does matter who produces it, but, you know, you question data a lot more if it's a weird result produced by an undergrad who's just come into the lab, as was in this case, than if it's somebody you trust. But, still, when it comes down to it, it's the same datum, and I liked that, almost that impersonality—or personality independence —of the scientific world a lot.

The other thing that happened there was that I heard a lecture by Allan [C.] Spradling, who with Jerry [Gerald M.] Rubin had just figured out how to make transgenic *Drosophila* with P elements. I don't know whether I consciously made the decision, but it looks like— You know, that was the thing that revived *Drosophila* as a tractable experimental system: being able to put genes back into flies easily. Later on, when I went back into graduate school, my first three rotations were all in fly labs. That system appealed.

VAN BENSCHOTEN: How long did you spend in this lab at Cornell?

KERNAN: Maybe two months, ten weeks.

VAN BENSCHOTEN: And what was your impression of America when you first came here?

KERNAN: So, I have to go back a little ways, then, because my first impression of America came when I was fourteen. I had mentioned that my mother's brother, Des Perry, ended up in Chicago after previous careers as a missionary priest in Nigeria and ended up marrying a previous nun. Now they divide their time between Chicago and Florida, but she was a Brazilian and had got into early childhood education, got into the Montessori field, and was running a Montessori school in Chicago when I visited them when I was fourteen. So my first impression of America came when I visited them for a summer. I visited them again when I was at Cornell, that summer, and did some looking around at graduate schools at that stage as well.

First impression of America: big. [laughs] Everything is big. And the reverse holds true when you go back to Ireland; everything is small. And you think, "I can't possibly fit into that car," when they come to get you. [mutual laughter]

Yeah, stuff like that. Arby's roast beef sandwich. Everyone remembers their first American meal. And it sounds like you're coming from this desperately deprived background, which is not the case at all. You know, by emphasizing the middle-class thing, I was trying to make sure that impression wasn't conveyed. Americans often assume that any immigrant is, by

definition, coming from a deprived background, but it wasn't the case.

But it's just so different and there's that sense of, you know— A sandwich in Ireland is something that has two pieces of white bread with, maybe, if you're lucky, one layer of ham in it, and maybe, if you're feeling luxurious, a layer of cheese. Not because you can't afford more, but, you know, who needs more? And this thing that I was presented with on the way back from the airport when I said, oh, I was a little bit hungry, was this mound of— Half a carcass of a cow stuffed between these two great big buns. I can't get my mouth around half of this. So that was strange from the outset.

Heat. It was Chicago in the summer. I was coming from Ireland and, you know, the local kids thought, "Wow," you know, here's this kid over from Dublin, "They play soccer there all the time." Soccer was just coming in. "He's going to be great at soccer. He can tell us how to play soccer." I wasn't all that— I'd played some on the street, yes. That is what I would normally do on the street at home. But I got there, it was 95 degrees. I just wanted to stay draped over an air conditioner when they would come try to drag me out.

I was politically aware enough, I think, at the time to be— I think that was the year *E.T.* came out as a movie [actually, although my first visit to the U.S. was in 1976, *E.T.* came out in 1982, when I made a second trip to visit relatives in the U.S., so this must be a recollection from that visit, when I was twenty], and I thought that was a very American movie, because it pretended to an innocence that the rest of the world didn't view America as having, or having it be entitled to. So that probably tells— Yes, I was in my disagreeable teenager phase at that stage. [mutual laughter] Well into it.

VAN BENSCHOTEN: Any other memories then? *E.T.* I like those. E.T., Arby's. Very concrete.

KERNAN: Yeah.

VAN BENSCHOTEN: How about the American character, though? I just spoke with someone, a Swiss man. I met him on 47th [Street] and Fifth [Avenue], where I was having lunch. He had just run the New York Marathon. I asked him, you know, what he thought of America, and he said, oh, he loved it, because Americans were just so optimistic, they were always smiling, they were always happy. He talked a little bit about Europe and the conditions there and perhaps why, to his mind, they were smiling less and why Americans have reasons to smile more. But, you know, it was interesting: his perception of the American national type, again. And I was wondering what was your own perception of that.

KERNAN: I didn't know enough then to make any perceptive conclusions. People seemed generous, friendly. People will, I think invite you into their homes, they'll share— There seemed to be a lot more of the world at their disposal, you know. "Oh, why don't you come up and visit our place on the lake?" A place on a lake. We don't do places on lakes. We don't have

places on lakes. We bike fifty miles to see a place on a lake and then we go home again. There is something of a sense of abundance, but particularly for— You know, at that time in particular, I think, there was this mixture of anti-American political feeling, which would have been perfectly expressed by a teenager in disagreeable mode. So I would have both been reacting against that— Again, I'm sure I was just as disagreeable a guest as I would have been a son.

VAN BENSCHOTEN: If you like, we can leave it there and we'll pick it up again tomorrow.

KERNAN: Right. Let's see. That last digression brought us away from impressions of the U.S. back when I was at Cornell first. So, yes, I think impressions got more concrete when I went back in that summer and then subsequently when I went back to graduate school.

VAN BENSCHOTEN: So Cornell then was, what, your third visit, then, to the U.S.?

KERNAN: Sorry, no, Cornell would have been my second visit to the U.S., first when I was fourteen, then I went over for that summer, and then I would have gone back to go to graduate school after graduating.

VAN BENSCHOTEN: So what we'll do then next time when we begin, is we'll talk a little bit about finishing up at Trinity and then coming across to do your graduate studies at the University of Wisconsin [Madison].

[END OF TAPE 2, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Maurice J. Kernan**

INTERVIEWER: **William Van Benschoten**

LOCATION: **State University of New York, Stony Brook**
Stony Brook, New York

DATE: **11 November 2002**

VAN BENSCHOTEN: Today is November 11, 2002. I'm with Maurice Kernan, and this is tape three, side A.

You wanted to add a few things from our last interview.

KERNAN: Oh, yes. Over the weekend, I thought back over the last interview and things kept recurring to me. Books. Just a couple of mornings that recurred. C.S. Lewis, [J.R.R.] Tolkien, that fantasy-related— C.S. Lewis was actually something that was pushed very much by the Jesuits when you're at school in Belvedere. I think they liked the moral clarity of the stories and that certainly had an appeal to me for a while, that idea of clarity. Clarity in argument is something that I have always aspired to, but never really achieved. The school had and has a definite character in some ways. It probably was downplayed during the years I was there, but originally it had been a school to educate the Catholic elite. It was expected, and did train a lot of the people who went into government, and they have archbishops and probably a prime minister or two, or certainly some ministers, among their alumnae.

So there was an elitism there that was in some ways expected, certainly by the older members of the faculty, but it was also in conflict at the time with the liberation theology influence that the Jesuits were in the thick of, around the time when I was at school, which was very much identifying with underclassed. I think I mentioned in the last interview, the school was right in the middle— It was in what had been a wealthy man's house set down right in the middle of the poor areas of the city.

So the messages were kind of mixed, but probably the school is still performing the function that it did then. In that sense, I always had the feeling of, "Well, that wasn't my message." It was obviously a mission, but neither the— Although I always felt guilty about not being as altruistic and doing all the community service things, the community services that you were meant to do to keep up the altruistic side of the mission. Nor was I comfortable with the people who were at ease in debate and being groomed for positions of leadership. I always felt slightly out of the ordinary there, and always felt that I had this pull toward science that was something different. It was a third direction, not in the direction towards leadership or towards service.

What else? The relationship with my dad [Thomas Kernan], going back a couple years further. I think the most sustained contact I had with him was in the years between when I was eight and when I was twelve. At eight, I switched from going to a local public school and started commuting into the city to go to Belvedere, and from those four years, we'd go on the train together, and the morning commute was actually the time when I had a lot of contact with him. We'd walk to the train together. I learned to walk fast with short legs, something I still do, to the annoyance of people who are trying to keep up with me. I think there and afterwards, still, when asked to reflect on my relationship with him, gentleness, frugality, those words sort of come to mind when describing him.

Let's see. [Irish] national character, you asked me about earlier, and, yes, I mean, I actually was surprised I didn't come up with more of an answer at that point, and maybe we'll get back to some of them later. But flexibility of mind, I think I touched on. Pessimism, constructive pessimism, and historical awareness. I'm reading off notes here. A degree of insecurity, especially when dealing with land or property. All of those things. They'll come up again when we talk about science.

VAN BENSCHOTEN: I'm sorry. The last one, I didn't hear it quite.

KERNAN: A degree of insecurity when talking especially about— It's one of the reasons why the concept of tenure of so appealing to me and why actually it made me pretty happy to have it.

Back to school again. Religion. At one point, I was probably identified as one of those people in my class who they would have had a chance of recruiting to be a priest. I remember the sit down and "Have you ever thought about—?" talk. I think I was able to give a fairly definite "no." One reason was, like I said, because I was already too interested in science. That was what I wanted to do and that really wasn't what priests did. The other thing, even though I had very little reason for saying so at the time, I was just too fond of women, and until they gave up that celibacy thing, I knew it wasn't for me.

VAN BENSCHOTEN: Now, who sat you down and said, "Have you ever thought about it?"

KERNAN: Oh, there was a religious counselor who would have been the person. I'm blanking on the name.

VAN BENSCHOTEN: Okay. We'll pick up the chronology, and, again, if you want to add anything, too, from the notes you have there, feel free.

KERNAN: I have some more things to add. If they don't come up, we'll pick them up the end

of this session or the beginning of next.

VAN BENSCHOTEN: Good. We left off last session, we had gotten you to near the end of Trinity College, and you had talked about how comfortable you felt there. That was a fairly good period in your life, it sounds as if. What I'd like to do, maybe, is if you could describe— I know that you had gone to Cornell [University] also, you had gotten a taste of laboratory science, and you told me what that meant to you and the discoveries you were making at that time. How do you get from Trinity College, then, to the University of Wisconsin in Madison? Describe that transition, if you would.

KERNAN: Let's see. The ground was set in the summer between the third and fourth years, the summer before I would have graduated. I think at that point one of the reasons that I had probably picked genetics was because it provided this opportunity to go to the U.S. [United States] to do a summer in lab. I'd gone to Cornell, decided I liked research. At the time, I'd visited— I'd used that time to go and visit my mother's brother, my Uncle Des [Perry] in Chicago, had taken some time to go around campuses that were in reach of there. Northwestern [University]— I think Northwestern was one; [University of Wisconsin] Madison was one; University of Chicago was one. And applied to those and to some others when I got back, when it was time to apply to graduate schools.

Of those, Madison was the one that I'd seen before, could see myself going there, and had a great reputation in research, had a really broad base with a very large variety of labs. Since I really didn't [know what I wanted] to do at that stage— Someplace where I couldn't go wrong by going, and then choosing once I was there seemed to fit.

I mean, there's an unspoken assumption there that I would have had to reach as to why continue to go into research. In part, that was very much the ethos of the genetics department where I was finishing up my undergraduate work, and as I said already, the departments truly were— Since you specialized early, you worked almost within a department. You had a desk in the department, what was a small department.

Actually, this keeps recurring to me. One of the buildings that I had been in, though it wasn't designed for science, it was an ideal building, ideal research building. It was actually the hospital that had been built by Oscar Wilde's father, Sir William Wilde. There is an interesting genetic connection, because he almost got to Mendelian genetics before [Gregor] Mendel did, or before Mendel was rediscovered, by following the patterns of inheritance of deafness. He noticed that deafness seemed to be inherited in families and had also noticed that it seemed to be—though he didn't have anything like the concepts to put it together—that there was difference seen, in families at random where it was inherited dominantly and those where it seemed to occur sporadically, which would have been recessive mutations, which reappeared when there was inbreeding or cousin marriages.

So it kind of chimed nicely with the fact that now I get my funding from the National

Institute on Deafness [and Other Communicative Disorders] and have at least a peripheral connection. The same building, I also learned later, was a building where we had lectures, or possibly the same room, was where my dad had once taken dancing classes, so it was used before it was bought by the university, it passed into— Was used for various other purposes.

What else? The idea of that building, had a central atrium, a stairway that ran all around and labs that opened off that stairwell, so to get from any—no elevators, of course—floor to any other, you had to pass through this central area, which meant that you saw everybody else in the building several times a day, and you could pretty much hear what was going on in the building, all over. There was very much a sense of community, even among the undergraduates who were in the building, which was, perhaps, unusual [compared to] a U.S. setting.

But, again, it was something I was comfortable in, the idea of being accepted in a scientific community where although it was recognized that you were very inexperienced, still, the science you were doing was treated as real science. I remember the explicit recognition as such when we'd all passed our final oral exams and I think [John R.S.] Fincham the professor of genetics at Cambridge [University], was brought in as the extern. I remember noticing the degree of difference in the greeting after the oral exam and before was definitely a "welcome to the club" experience.

The chair of genetics at Trinity was George [M.] Dawson, who'd been, I think, one of the last people, probably, to be a leading scientist without a Ph.D. He'd got his master's degree and that was sufficient, all that was needed. So he had a very idiosyncratic way of lecturing, very elegant, polished. Very fond of aphorisms, especially to do with genetics. There was a poster up quoting [William] Bateson's "Treasure your exceptions!", and this sort of seeking after the odd or the unusual was an early lesson. Also the same lesson in elegance that one of my high school teachers had tried to—probably failed—to teach me early on, that same lesson of elegance in expression. One of the ways we were taught how to write was by having to produce a four-page essay on a particular scientific topic and having that critiqued each week. I remember once being justly criticized for an extremely weak ending and told to finish it off with a bit more panache, which sort of connects up with one of the other points I was going to make, so I'll make it now.

This idea of a national characteristic, of course, that's famous among the Irish, is storytelling. And the ability to tell a good story is maybe more honored than the ability to get the facts straight, which means that when you're a scientist in Ireland, you're not quite in tune with the national culture. And when you leave Ireland to do science, you're exiled in more ways than one. Even when you're doing science in Ireland, you're an exile once over, because you're doing something which isn't entirely valued on the larger scene. And science has not been valued in Ireland all that much, in part because until, well, ten years ago, there wasn't really the money to mount a national research effort, and even since, when the country's got a lot richer, the funding for science has lagged back of the economy by quite a bit.

So that's probably the flaw in the Irish culture, that getting the facts right doesn't— What makes the Irish good lawyers, good storytellers— It means that science doesn't get recognized

and valued for what it can bring to a country.

But the other side of the story, of course, is that to be a good scientist, especially to teach, especially to write a grant proposal, to write a paper, you have to be a good storyteller. I've learned how to construct a story, and it goes a little deeper when you think of genes and gene histories as stories. This one connection which I make explicit in lectures is the idea of a gene on a chromosome as containing the history, and the chromosome as being sort of like a Dead Sea scroll, having this whole very ancient source of information, very— It's been written and overwritten and contains keys to stories that go back not just for thousands, but for millions of years. And that sense of history and a need for the awareness of a history and the ability to tell a story about the history of a gene is something that I value a lot in science.

VAN BENSCHOTEN: That's interesting, all those connections that you make. I was also thinking, too, that I've come across scientists, some in the Pew [Scholars Program in the Biomedical Sciences], who are sort of entrepreneurial scientists. You know, who really almost have left the lab completely and spend their time trying to generate funds for their lab, and that usually means being able to present your science in a very effective, telling way, usually to nonscientists in many cases. So there's another case where storytelling becomes more and more important.

KERNAN: Yes. I like it. As I'm getting better at it, I like doing it. But I also find it's a problem. I find, particularly a couple of years ago, I was going through cycles of having to shorten, shorthand things and dumb it down so much, that eventually you start thinking of the concepts yourself in shorthand. You can get too far away from the actual data and the complexity and the mistakes and the exceptions, you know, which should be treasured, and maybe are telling you that the story isn't as simple as you were thinking. I guess it's a typical relation of people, postdoc[toral fellow]s and graduate students in the lab is to restrain the P.I. [principal investigator] from going off and telling these wonderful simple stories that don't quite fit the facts.

VAN BENSCHOTEN: From Trinity College, you go to the University of Wisconsin at Madison. You've already described why, but what was your first experience at Madison?

KERNAN: Let's see.

VAN BENSCHOTEN: I assume you went through rotations.

KERNAN: Okay, I was actually thinking of the corn boil that was the first— Almost a requirement that we go and taste the triple recessive ultra-sweet corn that they specially

developed there on the campus.

But, first experience was rotations. Again, going into a close-knit community of first-year students. We all had our offices together in the same room. Again, that put me then into a situation where I felt quite comfortable because it was so much like what I'd left at Trinity.

Rotations in— We had, I think, four three-week rotations. I did mine in three *Drosophila* labs and one virus lab, Barry Ganetzky's lab, which I think I rotated in first and where I ended up. Mike [F. Michael] Hoffman's lab, working on *Drosophila* development and oncogenes in flies. Janet [E.] Mertz's, where I did— Let's see. No, the last *Drosophila* lab was Elizabeth [A.] Craig's, working with Karen [B.] Palter in heat shock proteins [HSPs], and Janet Mertz's lab, which was a virology lab, working on *SV40* [simian virus 40].

In the *SV40* lab, that was my, I think, last experience with cell culture, and which I'm beginning to need now again. I'll have to go way back to that to try and relearn it. But at the time, it seemed that *SV40* was, I think— It seemed to me as if there was a researcher for every nucleotide in the genome and that it was too crowded. Still, I had that pull towards things that were unexplored, and also a dislike of and maybe even a fear of competition. I'd rather not compete, if possible, and I didn't want to go into an area that was already so crowded.

Among the fly labs, I think that Barry's lab was one where— Barry Ganetzky's lab, I think it was the idea to have a neurobiological phenotype affecting behavior— There was a degree of—probably from natural history interests—a liking for having a whole behaving animal around, and having phenotypes where you could observe the whole behaving animal and make inferences at the molecular level and go back and forth from those. I'm not sure— I doubt that all of this was thought out at the time, but I remember thinking that Barry's lab and the projects there were the ones that seemed to generate those questions in my mind, so that's where I ended up.

VAN BENSCHOTEN: Eventually you do genetic molecular analysis of the *nap* [no action potential] locus.

KERNAN: Right. *nap* was a— That turned out to be a great project, though at the beginning it seemed like a bit of a slog, not necessarily because it was a bad project to give a student, but because I seemed to be— At least some of the other people in the lab thought I was going about it in a very unimaginative way. It was a positional cloning project, which then meant that— Or it still does mean that you're given a mutant strain and told to find the affected gene. And without a knowledge of what the gene does or what it looks like at the molecular level, but based on the mutant phenotype, and based particularly on the location of the mutation on the chromosome, hence positional cloning.

That strategy is one that I followed all the way since until very recently. It's incredibly valuable, because it tells you things— If you're willing to admit ignorance at the start, which I

was, it tells you things that you're not going to find out otherwise. It tells you things. It's an exploratory rather than a goal-directed way of doing science, and I prefer that, as always. So what that meant, though, when a geneticist tells you something is elegant, it probably means it's going to take him years without any guarantee of success.

What I was doing was chromosome-walking, from the nearest point that we knew about on the chromosome, initially in two directions, because we didn't know which way we were facing, and walking both directions to find the gene, looking for changes associated with mutations in the gene. The phenotype was, though, something very clear, and that was also reassuring, because the great thing about starting with a phenotype is you know you have a story. You may not know what that story is, but you know there's something there. The phenotype with *nap* [nap^{ts}] was that if you heated the flies from room temperature up to about 37 degrees, they would instantly paralyze, and if you cooled them down again, they'd instantly wake up. And that was just so neat to watch. You could do it over and over again, and I like watching things over and over again, especially strange behaviors.

In fact, one of the key breakthroughs in the project came from observing subtle distinctions in the phenotype, that some genotypes of fly would paralyze at temperatures a little lower than others. I remember thinking at one stage that I could look at some flies, at flies inside tubes in a water bath to keep the temperature, and I could tell from the way they were behaving that I would know instantly what the temperature was to within some fraction of a degree. But in order to actually convey that in a way that could be put down on paper, it would take me so much longer, so much more quantifying, and I would actually have to throw out so much data, and that idea that I think I touched on before, about the difference between— Not intuitive, but just unanalytical observation and the necessity to throw away much of that to actually make something convincing sort of came from those observations.

The strange thing about *nap,* and one of the reasons why the project took so long, was that, by the end of it, I'd done a chromosome walk of almost 250 kilobases, walking in phage libraries, which got you maybe 10 kilobases to 12 kilobases at a time. Most of that turned out to be unnecessary, because, in fact, we'd already walked across the gene within the first 30 kb [kilobases], except we never knew that we'd crossed it.

The reason was that the mutation [a translocation, T(2;3) maleless] which broke the gene in half, knocked it out completely, had a completely different phenotype from *nap.* It had a phenotype that wasn't paralytic, wasn't anything to do, apparently, with the nervous system, and instead was male-lethal. It turned out, for the geneticist reading this or listening to this, is that *nap* [nap^{ts}] is a recessive gain-of-function mutation. Gain-of-function mutations are usually dominant; recessive mutations are usually loss-of-function. So, *nap* was an exception of this kind, which I just loved. And working out the implications of that made the last two, two and a half years of the project— It was turned into a particularly really great genetics project.

I should say the best and most revealing experiment, though, was not done by me, but was one I should have done, but was done by a postdoc [Michael J. Stern] in the lab, who showed that extra doses of the sodium-channel gene [$para^{+}$] cured the *nap* phenotype, and *nap*

turned out to be due to reduction of sodium-channel gene expression.

VAN BENSCHOTEN: That eventually led to a *Cell* paper [M.J. Kernan et al., 1991. *nap*^{ts}, a mutation affecting sodium channel activity in Drosophila, is an allele of *mle,* a regulator of X chromosome transcription. *Cell* 66: 949-59], wasn't it?

KERNAN: That eventually led to a pair of *Cell* papers [Kuroda, M.I. et al., 1991. The maleness protein associates with the X chromosome to regulate dosage compensation in Drosophila. *Cell* 66: 935-47], because we were able to collaborate with the people [Mitzi Kuroda and Bruce Baker] who'd been coming at the same gene from the dosage compensation side, the thing that was causing the male lethality.

That was also an important early experience because that meant that my first experience of collaboration was entirely positive. I think people are imprinted by the first experiences when the collaborations or competitions turn out good or bad. I've since, I hope, erred on the side of being willing to collaborate and share information, because it worked out for us. It has since worked out for me in almost all cases.

VAN BENSCHOTEN: Let's talk a little bit about Barry Ganetzky, if you would. What was his laboratory management style?

KERNAN: Fairly hands-off. He would manage more by casual interaction than by direct instruction, though with the important exception that one of the things I always regret having done was, at one stage he did teach us some early morning classes on fly chromosome mechanics, advanced *Drosophila* genetics, which I kick myself—not quite daily, but weekly, monthly, daily on the days when I'm trying to teach it myself—that I never got up early enough in the morning to make the effort to go in and attend.

Early on, though, I did figure out, when I had a chance, that the most useful thing I could do or the best place to be in the lab was to make sure that my fly-pushing station was on the same day as Barry's, because it was that— Fly pushing is just sitting at a microscope, counting flies, sorting the flies from the crosses that you've done, is a great stimulator of scientific interaction. It occupies just about the right fraction of your brain that you can pay attention to the flies and yet talk about something. So a lot of interactions. I would even decide, "Barry's sitting down sorting flies. I think I'll go down and sort some flies and see what happens." And a lot of interactions, I think, valuable interactions happen like that.

Barry himself was very much trained as a geneticist. He is proud of the fact, and so am I now, that his scientific lineage as supervisor-student goes back to T.H. Morgan, rather than a molecular biologist. So that since the earlier part of my project was chromosome-walking, which is plain molecular biology, I probably was more closely instructed by a postdoc in the

lab, Kate [Katherine] Loughney, and later on a graduate student, Rachel Drysdale, than I was by him, about that part of the project. Once the project turned genetic, I think both his interest— We had this weird genetics, and he got more interested.

His own training and his own Ph.D. thesis had been on some very weird genetics indeed, called segregation distorter, which I've still also maintained a fascination with. In fact, his lab had this idea, I thought, of having two different stories going on. Having something about the genetics of the nervous system, for which a long time was the bread and butter of the lab: what the major grants came in to work on. This was the genetics of mutations which affected sodium channels and potassium channels and many ion channels in the nervous system. That strategy of, by positional cloning, going after genes based on phenotype, has led, in fact, to the identification of many of the potassium channels that people work on now. It's been sometimes a sore point with him [Ganetzky]. *Drosophila* neurogeneticists always take care to point out that many of these channels were, in fact, originally identified as *Drosophila* mutations based on that strategy, that we wouldn't know what a potassium channel looks like were it not for the fact that people had identified the Shaker mutation with the intent of finding out what was behind that mutation.

So that strategy goes back to Barry's postdoc with Seymour Benzer and, I think— I remember at a Cold Spring Harbor meeting, Barry was quite happy when Seymour Benzer passed by my poster and thought that it was sort of okay.

The other story, though, in the lab was Segregation Distorter, which Barry had taken for his thesis and was probably his true love—and I think still is—which he's managed not only to keep going, but to make the major breakthrough in it by people in his lab have identified the segregation distorter locus. This is a gene which breaks the Mendelian rules and propagates itself at the expense of its homolog. A selfish gene, *the* archetypal selfish gene, and probably the best analyzed example of the case.

Even though I never worked on that project, the selfish gene theory has actually— I think about it a lot and it's also been an influence on the way I do science. Going back to books that have had an influence, I'd have to include *The Selfish Gene* by [Richard] Dawkins, something I read as a late undergraduate, very much influenced by.

I remember thinking— It may have been the GRE [graduate record examination] exam that I took, but one of the questions I got wrong, I think, was the question: at what level does natural selection operate, gene, organism, or group? And I think I answered, having just read *The Selfish Gene*, "Of course, it's the gene." And I'm pretty sure the right answer was intended to be the organism. I still debate those questions.

VAN BENSCHOTEN: I'm sorry, does Richard Dawkins also have the thing about the meme theory?

KERNAN: Yes.

VAN BENSCHOTEN: What are your views of meme theory? I've just recently come across it and I'd be interested.

KERNAN: I find the theory very appealing. I mean, it's a concept that's it's own proof, in some ways.

VAN BENSCHOTEN: Right. Exactly.

KERNAN: And you should always be suspicious of concepts that are their own proof. The idea of information generally applied is something that appealed to me a lot. And what I think— You know, when I start a genetics class, I start it by saying that genetics is an information science, and that information can be good or bad, depending on what your interests are and what the information is. Something that can have its own interests. I think I don't use the word *meme* in argument too often, because I'm not sure about the extent to which it's being used by people who debate more philosophy than I do. So I'm not sure exactly what people are hearing when I'm saying it. But what I understand by it is cultural information treated genetically, and yet I do believe that, think that it is a concept that should be more widely understood, that there are some very bad memes around.

Let's see. I think the classic, the most revealing one and the most horrible one I thought of recently, or that seemed to be operating, was the time there were those massacres in Rwanda, where the meme was propagated by radio, which is a relatively low tech, but highly effective way of propagating memes: kill all the Tutsis before they kill you. Which seemed to be particularly easily propagable in that context. So, yes, books written with that philosophical basis, books by Dan [Daniel C.] Dennett, for instance, have a big appeal for me.

I think the other meme that probably contributed to why I no longer consider myself religious is "Blessed are they that have not seen and yet believe," which contains its own, you know— If you passed on the message, good things will happen to you, which is your basic chain letter. Chain letters are memes, memes or viruses. So I'm thinking of it—as you'll notice, maybe being Irish, maybe being pessimistic, defining in negative terms—as something to be avoided. Insofar as I have principles, they include an aversion to things like chain letters, chain e-mail, and religion.

VAN BENSCHOTEN: That sort of blind mass— Sort of mass behavior, too.

KERNAN: Right.

VAN BENSCHOTEN: That's sort of a vague way of putting it, but I think I see the pattern you're sort of pointing out here, though.

KERNAN: It goes along, I think, with being independent-minded. Of course, I've tried, you know— A couple of times [I've] gone on marches and joined in chants and always felt that I was giving up something of myself that I didn't want to give up. Usually afterwards. I mean, I wouldn't say I'm particularly morally courageous, so I've never been put in that position, or probably more likely, I've always taken care not to put myself in that position. I've always felt it more as a selfish thing that I was being asked to give up something of myself that would be a surrender to memes of those type.

VAN BENSCHOTEN: We were talking— This is a very interesting train of thought, though. We originally started with Barry Ganetzky and I asked you about his lab management style, and then you sort of got onto his legacy, or his lineage.

KERNAN: Is it really hard interviewing me? [mutual laughter]

VAN BENSCHOTEN: I think it'd be fascinating to sort of go back and trace our own lineage, our own biologic lineage, maybe. But anyway. I wonder if you— You said, if I understood you correctly, he's pretty much hands-off.

KERNAN: Yes.

VAN BENSCHOTEN: Is there anything else, though, that you'd like to add other than, of course, his interest in genetics and the lineage that you spoke about as well? How did he run his lab, for instance?

KERNAN: At the time, it was sort of the way that positional cloning project labs— It was one gene, one person. You were assigned a gene and I think once or twice, when it— Positional cloning was not going at all well, and we had both kicked around the idea of, "Look, this may or may not— We've no reason for thinking this is going to work anytime soon. It's obviously a sodium-channel defect," which was wrong, but at least— That is, the phenotype was due to sodium channels, but we said, well, "We probably really think this is going to be the beta subunit of the sodium channel. Let's try it directly." Then I think we both said, "Nah, we really don't want to do that. Let's stick to what the mutation's telling us."

When I entered the lab, I was given a choice of projects. It could have been *SD*, segregation distorter, or the *nap* project. I think, had I really wanted to switch projects, I would have been let. So he was not directed in that way.

I think I was unhappy at some point with feeling that I wasn't getting enough supervision, but that usually— We would go and find Barry rather than have him come to us. We never felt pestered by him. Sometimes we felt like we could have done with more pestering, or we needed to go find him in order to convince him that we had something worth looking at, which, of course, almost exactly describes my own management style; that is, being hard to find and not pestering people.

VAN BENSCHOTEN: How big was his lab?

KERNAN: It varied in size. It grew during the years I was there and has been up and down a couple of times since, I think. Myself, usually about two or three postdocs, two or three graduate students. At its height, maybe, towards the end of my time there, maybe four or five postdocs, three graduate students.

VAN BENSCHOTEN: What was a typical day there for you in the Ganetzky lab? When would you usually come in, for instance?

KERNAN: Let's see. Not too early. Nine-thirty or so. Depending on whether it was the molecular work or fly work day, it would be— At some point the lab switched, expanded, and changed geography a little bit. That's why I'm trying to decide on whether to construct my day on the basis of the new lab geography or the old.

When I was doing chromosome-walking, it would be pick up at whatever cycle of the phage isolation or phage library screening I was on that day. It was usually about a two-week cycle between screenings, probably with extra time for my rather inefficient attempts at phage mapping and restriction mapping, something I always found quite difficult.

I'm not sure that I could completely construct a typical day chronologically, but it would have involved usually phage library screening, a lot of manipulation of plates, plaques, filters, developing film and trying to interpret spots on film, mixed with fly pushing, which is setting up, sorting flies, finding crosses that you set up two months ago and then forgot about—and trying to sneak them out of the lab before Bob [Robert] Kreber, the fly overseer, got mad at you for letting mites into the lab— wrestling with restriction maps, and then a couple of times feeling that you're really going someplace.

It surprises me now the extent or the amount of time I let go by, or could plug away at what was, to begin with, a very boring project, without giving up in frustration, but I probably

was— I perhaps should have given up earlier. I should have taken warning from the fact that, you know, I'd already walked 100 kb with not only no mutation site, but no real absolute assurance that I was going to find one, or that I might not have walked past it already. It worked out well, but I was not entirely sure that it was a good use of my time.

VAN BENSCHOTEN: Okay. Let's hold it right there and I'll flip this over.

[END OF TAPE 3, SIDE 1]

VAN BENSCHOTEN: We're back. This is tape three, side B now.

We were talking about the [Barry] Ganetzky lab. You had described sort of a typical day. Oh, here, there's one thing in your CV [curriculum vitae] that I thought was interesting. A couple things, but this was one of them. In 1989, you were given the Lubrizol Industrial Fellowship. Did I pronounce that correctly? Lubrizol?

KERNAN: Yes, but I'm not sure that that had any particular significance to me, because I think it was the way of the department generously finding a way to pay me when I wasn't eligible for NIH [National Institutes of Health] training grant funds. In fact, I did well out of being ineligible for training grant funds, because I think it so happens that various fellowships they had available and were trying to spread around often paid a little more than the usual, which was unfair, but true. I always felt that I was never poor as a graduate student. I always felt well off.

VAN BENSCHOTEN: How do you get to the Howard Hughes Medical Institute in 1990, after your thesis?

KERNAN: I went there to do a particular project: to do the work on mechanotransduction of cells, figure out how cells and *Drosophila*—since I chose to be stuck with *Drosophila*—convert stimuli like touch or sound or pressure into electrical stimuli.

VAN BENSCHOTEN: And how did you come by that project?

KERNAN: So we need to go back to when I was in graduate school [University of Wisconsin], and something way back, to when I was reading books by Lewis Thomas, *The Lives of a Cell*, which had an appeal because, like many things that had an appeal for me, it went back into the early history of things and how those are popularizations of the idea that cells were composite, having been put together from—that eukaryotic cells are composite—smaller cells. So that

things like mitochondria and, in particular, cilia, are relics of formerly free-living, independent bacterial cells. It's a theory that's been popularized by and originated— I don't know if it originated with Lynn Margulis, but certainly popularized by her.

While I was in graduate school, I'd taken some of the courses there on trying to remedy the gaps in my education in neurobiology, because one of the things I hadn't learned at all in Trinity [College, Dublin, Ireland] was any amount of neurobiology or neurophysiology, so I'd taken some great courses from people like Donata Oertel, Tony [Anthony] Stratton about cell physiology, and neurophysiology in particular.

Among the papers that we read there was one by A J. [A. James] Hudspeth on, I think, one of the earliest demonstrations of where the mechanosensing cells were on a hair cell. And they had done that by iontophoresing an antibiotic, streptomycin, which acts as a blocker of the channel on the hair cell. Even though we still don't know what the molecular identity of the channel on the hair cell is, they were able to show where it was on the cell and that it was, in fact, located at the tips of these little bundles of microvilli at the tip of the cell, by focal iontophoresis, puffing on little squirts of this antibiotic, streptomycin, that blocks the channels.

Streptomycin is an aminoglycocide and, in fact, if you overdose on aminoglycocides, that class of antibiotics, or if you give somebody too much of it because you have to give them too much to cure an infectious disease, very often you go deaf, because they have a toxic effect on the hair cells, perhaps by blocking the channels, perhaps by some other effects in the cell. I'm not sure; that's not entirely clear.

The reason I'm going into this is because the other thing I knew about streptomycin was its site of action on bacteria, and how it kills or blocks the replication of bacteria. It binds to a small ribosomal subunit, in fact, to a small ribosomal RNA. I knew about that because it was the subject of my undergraduate thesis that I'd done way back. I also knew, from *Drosophila* work, that there was a mutation called technical knockout [TKO]—you'll have to ask me about fly mutant names later—which is one of the bang-sensitive mutations: knock the flies around their vial a bit and they paralyze. They go into a seizure and they can't get up for a while, hence TKO.

TKO, when somebody did the positional cloning on it—I'm not sure where it was done, or whom to give the credit to—turned out to be what looked like the *S12,* the small ribosomal subunit protein, which is the site of mutations to streptomycin resistance. I thought, wow, here's a connection that seems strange and maybe is really telling us something deep about— That ribosomal proteins and ribosomal RNAs might be doing something other than just being ribosomes. And since I've got a taste in science for the things that go way back in evolution and are slightly flaky, the idea that ribosomal RNAs might be doing things other than just being ribosomal RNAs in the cell appealed. I wondered if there might be a deeper connection there.

So I went and did a lot of reading on what was known about hair cells and what was known about— To see if there was any possible connection, and as far as I could tell then, can tell now, there isn't. But the amount of reading I did was enough to get me hooked on the idea

of here's an apparently simple but strangely unexplored process of mechanotransduction. It should be simple. It seems like, okay, you hit a cell, it changes its membrane potential. This is something electrophysiologists always try to avoid. We've got our electrophysiology rigs mounted on these big, awkward, heavy air tables to avoid all mechanical vibration. This is something that seems to happen. Mechanical disturbance changes cells' potential, sometimes when we don't want it to, and yet we still don't know how it happens.

So that combination of—you know, the place on the map that says "Here be dragons"—unexplored area. It was at once interesting and possibly a genetic approach. People had already done quite a bit of work on phototransduction. They were on their way, it appeared, to solving smell, and this seemed to be the last unexplored sense. So I thought it would be a good idea to think about doing a postdoc[toral fellowship] on that project.

I considered a couple of labs to do it in, and ended up in Charles's lab, Charles [S.] Zuker's lab, in part because he'd come and given a talk on photoreception, on the work his lab was doing then, at Wisconsin, and I thought it was a great talk. He had that quality, and has that quality, which I mentioned before, of energy, of putting energy into a presentation, and which he also puts into his science. He has and gives off that aura of success; this is a place and somebody who could make a project work. Also, by then he was Howard Hughes-funded, and he obviously had the resources to make it work, and because he was very interested in the idea, too. He also wanted to do something other than photoreception, and the idea of starting a project in mechanoreception appealed to him as it did to me, so he was up for it. He's gone on since to cover all the other senses as well.

VAN BENSCHOTEN: We should say, before we start talking about your postdoc work, that your thesis eventually wins the Sandler Memorial Award.

KERNAN: Yes.

VAN BENSCHOTEN: That's really impressive.

KERNAN: That was the most terrifying experience of my life.

VAN BENSCHOTEN: In what way?

KERNAN: Because that requires you to give the first plenary presentation at the annual *Drosophila* meeting, which is a relatively small meeting, but, still, an audience of 2,000 or so can seem quite large, especially when it's in a mirror-lined hall in a big hotel in Chicago. [mutual laughter]

VAN BENSCHOTEN: That'll do it.

KERNAN: And particularly when, right as you take your first breath to speak, somebody starts playing the bagpipes outside the hall. [mutual laughter] No idea where that came from. It broke the tension.

VAN BENSCHOTEN: Before we talk about your work in Zuker's lab, talk about Charles Zuker and his laboratory management style.

KERNAN: Very different from Barry's. One rarely had to go looking for Charles, because he would often come looking for you just to find out what was going on. He was always very enthusiastic about whatever project. He would always bring a level of enthusiasm and energy to whatever project he wanted to back. He sometimes would be equally enthusiastic in his denunciations of why something wouldn't work or whether it was a lousy experiment or a lousy manuscript or— There are many Charlesisms that graduates of his lab repeat to remind themselves of their time there, the words in which a piece of bad writing would be described. But he did have what the project needed, lots of support and enthusiasm, willingness to take a risk on what was at the time a very long shot and back it all the way for several years. Again, even though, just as in Barry's lab, what I produced probably really wasn't what he was looking for.

Charles and I are quite opposite in the sense that I am exploratory rather than goal-directed. He's very goal-directed and mission-oriented. Very effective in that way. So he was looking for *the* mechanoreceptor and wanted to, you know, mount as efficient and as fast a way of finding it as possible. I was looking for whatever would happen to turn up, and probably wasn't nearly as fast as he would have liked, nor necessarily as goal-directed, but he still— For most of the time I was there, I was operating independently of the rest of the lab.

When I came, I was the first person there not to work on phototransduction for much of the time there. I was the only person working on mechanotransduction, but he still— You know, I got half of the time of a technician, Dave [David M.] Cowan, which was a big help when doing a lot of the genetic screening, and never lacked for resources all the time I was in the lab. And it was a long postdoc. It was four years, with no publication until the end of that project.

He recouped at least some of his investment, I think, in the long run. Since then, Richard [G.] Walker's been able to follow me on the project and they got a great paper out of that, beginning from one of the mutations that we'd isolated.

VAN BENSCHOTEN: How big was that lab?

KERNAN: Bigger. Up to twenty people at a time. It also has gone up and down a bit, but on average, much larger. Different lab space, too. Open plan lab space, for the most part. A larger group.

VAN BENSCHOTEN: Could you give us, maybe, an overview of the work that you did in mechanotransduction?

KERNAN: I started trying a nongenetic approach, trying to do subtractive hybridization, in part because that was something that had worked before for their lab in phototransduction. One of the resources had been a set of, I think, cDNA clone— No. It was a subtractive cDNA probe that had been isolated by taking flies with normal eyes, flies without eyes, subtracting the message from those two tissues so to leave things or enrich for things that are expressed specifically in the eye, and using that as a tool to get genes that would be expressed only in the eye, therefore genes that would probably function in the eye.

The first idea was to find flies without bristles, sensory— A lot of our work was on mechanosensory bristles. All the little hairs that cover a fly are all sense organs. We had a mutation that overproduced bristles from Jim [James W.] Posakony's lab, also at [University of California] San Diego, called Bearded, [and] a fly without bristles, or almost completely without bristles, scute$^{10\text{-}1}$, and [the idea] was to make RNA from both flies and try to subtract one from the other.

It didn't work, in part, because it's not quite the same as the eye. Bristles are scattered all over the fly, so you don't have a single tissue. A lot of the enrichment in the eye experiments came from the fact that you can cut off a fly's head and shake and sieve to get many, many thousands of flies' heads. A fly's head is probably half eye, so there's a lot of enrichment even just in that step, without the subtraction. We can't do that for bristles; they're evenly spread and scattered all over the fly.

The subtractive hybridization itself— I probably wasn't skilled enough to bring it off, and I'm not sure that the method I was trying to do, hydroxyapatite chromatography, eventually would have worked. There seems to be a fraction of message that just won't subtract, not for the reason that you want. I've since thought of other— There's been other technical improvements by other people that have improved it, and you could think about different ways doing it then.

But after about nine months, I think Charles and I sat down and pretty much he told me to go into it genetically, and since that was more what I was used to doing anyway, I was happier doing that.

So what we began with was figuring out a screen that would allow us to take flies that seemed to be insensitive to touch. And my bias was to be able to do it— Previously from Charles's background in phototransduction, there was work that had been done on things like a

little countercurrent apparatus for flies, where you would find flies that would or would not move towards light. We spent a while just thinking about different ways, tools, obstacle courses, substrates of different hardness, where you could build some sort of obstacle course for flies that would channel them into ones that were and ones that weren't sensitive to touch.

But my preference, again, was, I think, for direct observation, going back to the idea that you learn even without— When you look at something, you take in so much information just by looking, that it's better to do things that way. Even if you're not necessarily going to use all that information, it's always better to look at what you're screening. So I decided to screen flies when they were larvae. This conceptually mimicked a screen that had been done already in *Caenorhabditis*, which all along has been our not quite hated rival, but let's say, very well-respected rival in this work. Martin Chalfie working in England, I think with John Sulston, had done a screen for touch-insensitive worms, for touch-insensitive nematodes, and had pulled out a whole bunch of genes, and has since gone on to make probably the very complete model of mechanotransduction in, luckily, a different sort of cell than the one we were interested in doing.

Okay, let's look at a fly when it's a worm, when it's a larva. So what I did— and David Cowan also helped me— for several months was generate many, about five and a half thousand families of flies, each one of which would have a different mutagenized chromosome, where the sons in those families would express the mutant phenotype of any mutation that happened on the X chromosome. So we grew the flies in twenty-four-well tissue plates. So it was a matter of sorting, sitting down each day with maybe a stack of five or six twenty-four-well plates, each one with a two-day-old group of larvae in the bottom of each well, tickling those with a very expensive instrument, which was an eyelash stuck on the end of a stick, and trying to, first of all, work out is there a stereotypical behavior response when you stroke a larva with an eyelash? Yes, there was. Can we see any differences from that response in our mutagenized populations, our mutagenized families of larvae? And, yes, we could.

From that came a set of about fifty mutations, most of which we still don't know what's wrong with them, but the genes we seem to be hitting most often, the genes we hit several times, which is always a good indication that you're on to something, turned out to be what we were looking for, turned out to be larvae that were defective in mechanosensation.

One I still show as a slide of one of my favorite experiments is a genetic mosaic fly, mosaic being an animal where different parts of its body have different genotypes, usually random patches of mutant tissue and otherwise wildtype animal, where we could just tickle to show that fly had— Any mutant patch was numb and nonmutant patches weren't numb. That told us that it was a defect in the sensory [system]. When you do a behavioral screen, you can get a defect, and a defect could be due, in theory, to either the motor output from the central nervous system as well as the sensory input, or something in the central nervous system itself. That experiment—to show the mutant patches of tissue on the outside of the fly were the numb patches of tissue—told us that it was a sensory defect. So I always liked that experiment because it was so cheap. Did I mention that I was frugal? I still am.

VAN BENSCHOTEN: And how about a typical day? Did you pretty much keep the routine that you had when you were in grad[uate] school, coming in about nine-thirty?

KERNAN: Again, it went into phases, depending on whether we were in that early screening stage, or later on. I'm going to go way back, when I was commuting in to school with my dad. That was okay, commuting with my dad on the train, but when I started commuting on the bus and got stuck in traffic jams, I hated it. I've always hated commuting. And as soon as I could assert my independence, I was cycling in to school through inner-city traffic from the age of twelve onwards. And always since, I've always preferred to live within biking distance of work. That was the case in Madison. I've biked through not all of Madison, Wisconsin, winters, but some of them, and still live within biking— What should be a ten-minute, but it's actually a fifteen-minute trip on a bike from my lab. So I lived close enough so I could come in— Yes, I could have come in at eight a.m., but I somehow never managed to. I got up later. Mornings. People always managed to get in early to Charles's lab for lab meetings, because the punishment for arriving late was that you had to buy the pizza for—

VAN BENSCHOTEN: [laughs] Interesting. When would you usually leave? Or would that be, again, dependent on what experiments you were doing?

KERNAN: Again, it depended on what experiments. Again, I liked going home and then coming back in the evenings.

VAN BENSCHOTEN: How do you eventually come to accept a position here at [State University of New York] Stony Brook? And that happens in, what, 1994, '95?

KERNAN: Let's see. Well, came time to look for a job, when we thought we had enough of a portable project to bring with me. Did about eleven different interviews, job interviews, so it was a very strange period in my life. I'm not sure there's anything quite as strange as that first round of job interviews. Very educational, though. Particularly if you do that many, although they seem at times to blur into one another if you try to string more than two of them together on a single trip, just by interacting. Partly because of the sort of scientist I was; I was pretty insular and isolated as a postdoc. But all of a sudden having to both appear intelligent and talking to many different scientists about many different topics, forced you to think on your feet a lot better.

Of those, I got offers from a few places and Stony Brook appealed in some ways because— In large part because it was a place where I could do the science I wanted, where resources wouldn't be limiting, but which was a great place to live. And having a place to live was, by then, as important to me as optimizing the place to do science. There were one or two

places from which I had offers, I think, that people were surprised I didn't go to, because they were more prestigious. But I felt the quality of life there, I don't think, wouldn't have been as good. By that stage, I was married and had a child on the way.

VAN BENSCHOTEN: When did you marry?

KERNAN: In San Diego. 1982. 1992. Sorry.

VAN BENSCHOTEN: And you had one child?

KERNAN: Our daughter [Ciara Emily Kernan] was born two months after we moved to Stony Brook, so, let's see, Karen [Kernan, née Kwik] would have been pregnant. I'm not sure if she was pregnant at the time I was doing my first— But it was certainly a consideration at that time. By the time I was into the second round of interviews, she was.

VAN BENSCHOTEN: What was the startup package that Stony Brook gave you?

KERNAN: A hundred eighty thousand. No. A hundred sixty thousand, with, I think, about twenty-five thousand extra for supplies. They renovated lab space—not the space we're in currently; I've moved lab since—for me. And no salaries were included in that.

VAN BENSCHOTEN: So it all had to be made up by grants?

KERNAN: I'm trying to remember how we paid for the first few technicians. No, I think I was able to pay for salaries out of that, but there was no money specifically set aside for— We weren't given a technician or anything to start with. So recruiting people had to be done right from the start.

VAN BENSCHOTEN: What was the hardest part about setting up your lab?

KERNAN: Recruiting people, both from the point of view— I found some of the bureaucracy at Stony Brook not particular[ly] helpful. There's a lot of— It's not a particularly streamlined place. It seemed to be that—I think because of where they got some of the School of Medicine money that they'd got—[an order] had to pass through some exceptionally sticky desks before a purchase request ever got off campus. But that in the long run didn't prove to be— Well, no, I

think it didn't seem to be an issue at the time, but I think if you added up all of the time spent dealing with purchase orders that seemed to go awry, having to learn a system that wasn't all that functional to begin with, where who you knew was as important as to follow the rules. Some of the rules weren't followed and some were, and it wasn't clear which rules could be followed and which ones could be ignored. I think that amounted, at the time, to a considerable cost in starting up here.

I'm not sure I can say that I would have managed particularly well even if things had been better, but lab management was something that I've had to learn and still have to learn, lab management in the equipment and logistics sense.

The other— Say the question again.

VAN BENSCHOTEN: I was asking what was the hardest thing about setting up your lab.

KERNAN: Initially, I think the hardest part, the thing that gave me most stress, though, was the idea of being responsible for other people. Having even, at the time, undergrads in the lab, being responsible for their time, or being responsible for their salaries. Even the hires that I would make now at very short notice and without thinking or agonizing over too much, I'd spend probably way too much time agonizing over it.

Also recruiting graduate students and postdocs were not easy to come by. Graduate students because, although there are plenty of graduate students at Stony Brook, there are also a very large number of labs, and it seemed to be hard for more junior labs to recruit students. They understandably were often attracted to the programs by more famous, well-set-up, better-established labs. I found it necessary to be a member of four different graduate programs in order to have a chance of recruiting any graduate students at all.

VAN BENSCHOTEN: Wow.

KERNAN: So, for instance, in the neurobiology department. The neurobiology department has its own graduate program, but it was relatively small then, maybe six students a year. Now it's probably up to ten or eleven. But most of those people would come in, understandably, specifically interested in integrative neurobiology, which I wasn't doing. [The] molecular and cellular biology [graduate program] has a large number. Genetics has a large number of students, but they get spread out over quite a large number of labs.

In the long run, it's worked out okay. I think at one point I had a graduate student in the neurobiology program who is very good, but didn't like lab work. Eventually, she quit with a master's degree, much to my disappointment, but she was probably happier and probably better off. It was probably the right decision for her.

VAN BENSCHOTEN: How often do you confer with members of your lab?

KERNAN: It varies. We try to have lab meetings about weekly, but I'm mostly through— Once we get to the end of a cycle, many weeks will go by before it gets restarted. Though I'll try to, again, manage a lot by casual interaction and keep abreast of things in the lab by hanging out there sometimes, just as Barry [Ganetzky] used to do: push flies, and make sure that anybody else who's pushing flies around, that I know what they're doing and they know what they're doing and why they're doing it. It will vary a lot. Some of the people in my lab are extremely experienced by now, then it goes all the way down to newly arrived undergrads.

VAN BENSCHOTEN: Do you have, for instance, meetings, and how often, journal clubs? How do you structure your lab?

KERNAN: Less than I ought to. We do have weekly lab meetings. I have one project recently that is somewhat self-contained and on which most of the people working on it are more junior. I'm having an extra meeting each week on that project alone. That's the first time I've done something like that, having a meeting of a section of the lab. Others are welcome to attend, but they don't have to.

Journal clubs we've had in the past, but we also participate in some interlab journal clubs, or interlab research series. The center that we're in, which is about seven labs, there's a monthly research seminar that usually we present in about twice a year or so. I could probably do more to foster interaction than I'm doing now. A lot of it's done person to person. The big whiteboard in my office. I made sure to get the biggest one I could.

VAN BENSCHOTEN: You can never have enough whiteboard. [laughs]

KERNAN: You can never have enough whiteboard.

VAN BENSCHOTEN: Tell me a little bit about the current projects that you have under way and, again, I'll ask you to sort of explain it in terms that maybe a bright nonscientist might understand.

KERNAN: Right. Well, yes, I'm not sure they need to be dumbed-down, because I do tend to think in as simple and mechanical terms as much as possible myself.

So, sort of going from the original roots of the project, one line of work continues in looking for the molecules that mediate transduction of mechanical senses, and the first gene that we cloned was a gene called *nompA,* for no mechanoreceptor potential A. I feel my greatest failing in this area, by the way, is the failure to come up with imaginative names for my own mutants. One of the things I really like about *Drosophila* work is that you get to name your own mutants. I'll give you some of my favorites later.

nompA is a protein that we found by positional cloning, would not have found had we not been doing a positional cloning project, and looks like it's something that serves as an extracellular mechanical linker to the cell that's doing the transduction. That is, it's part of a structure that transmits the movement, or stretch, or whatever the mechanical stimulus, to the sensory ending of the neuron that's going to be affected by that stimulus. If you don't have it, the sensory endings come detached from structures on the outside, like bristles, and they no longer work.

When trying to get that work published, we came up against the phrase, which we now kind of repeat with varying degrees of irony to each other, called "nonspecific glue." "This is just nonspecific glue. It's not worthy of a *Neuron* paper." But we've been finding lately, particularly since the mosquito genome was sequenced—it makes a great comparison to the fly genes, separated by 250 million years of evolution— anything that can be changed, will be; but things that are functionally important are conserved.

nompA is really highly conserved, much more than you would expect for an extracellular protein. So even though once we got, perhaps, devalued a little bit, once we got our first paper in, now we're going back and revisiting it and thinking that it looks like— All those arguments that we made about the review [of our paper on *nompA]* are, in fact, true, and we're going back and doing things like swapping subdomains of it.

It's one of a larger class of protein called ZP domain proteins, zona-pellucida domain proteins, which are found in specialized matrices like coatings of vertebrate eggs, and also in the vertebrate inner ear, even though the particular proteins, tectorins, that are ZP proteins in the inner ear, we can't really claim them as homologs to *nompA,* but they're at least analogs. They do the same job. There are still hints of something of a deeper connection there that might be telling us about the way signaling in sensory cells is done.

The real prize, though, and probably what, certainly, Charles was hoping for when I began this project as a postdoc in his lab, is the ion channels that are opened. His lab was the first to clone one of those from a mutation that I'd isolated. Richard [G.] Walker then went on and did a great job of not only doing the molecular biology, but also the electrophysiology on that mutant and began to show that it, first of all, was likely to encode a channel, and then going on to show that it did, in fact, encode a particular ion channel of a superfamily called a TRP [transient receptor potential] family.

Though it's not nailed down yet, it's very likely that this is the transducer channel in fly bristles. Recently, in collaboration with a couple of other labs, one in Korea and one in Virginia,

we've been able to find two other channels that look like they may be the channels for a different sort of sense organ, the one that mediates fly hearing. So what I sort of set as the culmination— Not the culmination, but a goal for this line of work, would be how to reconstitute a mechanosensing system. That means that putting it back together when you've taken things apart, which— Geneticists are very good at breaking things so that they don't work anymore. The hard part is putting it back together again so that it works, that is, taking the isolated bits that you've identified and putting them back together in a different system in such a way that you can say, "Look, we understand this system so well that we can put it back together so that we now reconstitute transducing. We put these three molecules back in." And this shows that you really know what's going on if you can reproduce the response, the change in membrane potential in a reconstituted system.

The problem with mechanotransduction is that it's complex in a cellular way. It's very dependent on all the specialized architecture of the sensory cell. If you take, say, a ligand-gated receptor—a receptor that's activated or a channel that's opened by a single molecule binding to it, as many are—you can take that, put it into a standard expression system like a frog egg. The frog egg will obligingly produce the channel, put it in the membrane, and you can spritz on your ligand, your activator molecule, and you can record the activity of the channel, if you have the right channel.

Mechanotransduction, that's probably never going to be possible because it doesn't depend on a channel being activated by a single diffusing molecule, but by direct mechanical connection to specialized extracellular structures, possibly the structure in which the *nompA* protein is in.

So, rebuilding, reconstituting this system from isolated parts is going to be more difficult, we think, but we'll probably still do it. I think now that we have good channels, or the likely candidate transducer channels identified, that's an important step on the way there.

So that's transduction. The whole area that the mutants opened up to us, though, that we hadn't anticipated getting into, but we're now pursuing, is how you develop these sensory endings in the first place. This goes way back to Lewis Thomas, back to *Lives of a Cell*, back to the idea of— Back to this fascination with cilia. Cilia are these things that stick out of cells. They've got this specialized, very beautiful symmetric architecture. In most cells, they're either waving around to propel something else along or to propel the cell along, if it's something like a sperm cell or a flagellated algal cell. These are what cells swim with or propel fluids with, but they can also be modified as sense organs, as sensory endings. So the specialized light-sensitive endings in your eye are all modified cilia that stick out of a cell and have been modified, in that case, to contain a whole lot of light-sensitive molecules.

In our cells, they're modified. Each sensory neuron has a single-sensory ending tip, and at its tip is a modified cilium that, for reasons we don't know yet, is mechanosensitive. In fact, the very first mutant we isolated, which is called *unc*, because it's uncoordinated, turned out to be a defect in formation of the cilium. Not only did it not transduce, but it couldn't because those ciliated sensory endings are missing. The only ciliated cells in a fly are sensory neurons

and sperm, so flies are actually a good way of studying this because the fly can have no cilia and still survive.

The origins of cilia are very intriguing and mysterious. A cilium grows from a basal body, which is like a barrel built of triplets of microtubules, again, in a symmetric circle of nine triplets; and that basal body used to be a centriole. The centriole is the same thing that organizes the spindle of a mitotically dividing cell. At each end of a dividing cell, you have a spindle that pulls the chromosomes apart in a very carefully orchestrated dance, and each centrosome— At the poles of a spindle you have centrosomes. Each centrosome is two centrioles and other stuff. When a cell is finished dividing, those centrioles can migrate to the surface of the cell, and one of them does this completely different cell biological job of organizing the cilia in some cells.

How a centriole goes from organizing a spindle to doing this completely different job is pretty much not known. It's a really fascinating issue because it goes way back to the very earliest history of eukaryotic cells. They're very fundamental. Centrioles have been the objects of fascination and mystery for about a hundred years now, since they were first identified; and they have a strange way of being duplicated that's tied to the cell cycle, but still quite mysterious. It's not certain what their origins are, but my bet is that their origins were, in fact, as cilia, or as ciliary organizers. So there's been there's been this strain in cell biological thinking that cilia also, just like mitochondria, isolated as semi-independent entities, were some other sort of cell that became associated with the eukaryotic cell. So it is, in fact, something that now has a very central role in cell division was, in fact, an invader or a symbiant or a parasite.

We're hoping to at least get a clue about how these— Get an insight back into the deep history of centrioles and how basal bodies and cilia, by looking at our mutants in which cilia fail to form, particularly from one of them, where the protein we found, which is called the *Unc* protein, seems to associate specifically with centrioles.

Here's where we get into stuff that's hard to convey on tape, because the things that have been most informative to us have been pictures, and the technique that's been most informative has been the method of tagging proteins with GFP, green fluorescent protein. So we do a lot of cell biological techniques of confocal microscopy, cytochemical staining, to localize these proteins in cells and in sensory endings to try and find out what they do.

So reading down my list of projects— We have so many projects going on in the lab that I have them written on the board. I'm reading, facing the whiteboard now, reading from the list. We're about half way down now, but I'll summarize the rest more briefly.

Besides ciliogenesis and transduction, we've started a third project area now on polycystins, a set of ion channels that in humans are the site of mutations to polycystic kidney disease and, we think, also have a lot to do with signaling in ciliated cells and in cilia.

We have a couple of other smaller projects going, but they tend to fall into those three main areas.

VAN BENSCHOTEN: Okay. I'm going to flip over the tape. I'll get a new one.

[END OF TAPE 3, SIDE 2]

VAN BENSCHOTEN: This is tape four, side A.

What role has serendipity played in your research, if at all?

KERNAN: Everything. When you start with mutations, everything is serendipitous, in a sense. Chance and necessity. I'm not sure. In a couple of projects, we certainly had the opposite of serendipity, whatever that is. What was the opposite to the Isle of Serendip? Someplace where there were a lot of shipwrecks, I guess. Maybe the Bermuda Triangle. We've had Bermuda triangularity, you know, in a couple of projects. We thought we had a serendipity going for us when cloning the *nompA* gene. We thought we had the obvious break point, and it turned out to have nothing at all with the gene, but basically cost me a year of the best technician that I had in the lab. His time was totally wasted working on that.

The fast way to clone a gene in *Drosophila* is to make a P element transposon that can both mutate the gene and tag it for quick cloning. Just by trying, we've never been able to get a P element mutation in one of our genes, certainly not the ones we were originally going for. So some of the time I felt like we were going uphill against serendipity.

Some of the other things—I think it was serendipitous that the first mutations that we found in the X chromosome screens were, in fact, the right ones; that the ones that we decided to focus on were the ones which eventually led to the ciliogenesis project. Even though they weren't transduction, they had the right phenotype, they were sensory-defective.

I think the biggest role serendipity has played, though, and where I've been luckiest, has been in the people that I've got in the lab. Yun Doo Chung, the postdoc[toral fellow] who's working on the *nompA* project, and now working on the hearing ion channels, applied to Charles's [S. Zuker] lab, from Korea. Charles didn't want to take the risk of hiring somebody without meeting and seeing them first. By that stage, I was certainly willing to take any risk to get a postdoc, and he's been fantastic. I think it's worked out very well.

Dan [Daniel F.] Eberl, who has spent a year in the lab helping me work out how to record fly hearing, doing the electrophysiology for auditory recordings, that worked out great. James [D.] Baker, who did the *unc* project, he came with his wife. She went into another lab at [State University of New York at] Stony Brook. And I think that James is fantastic. He's worked out great.

So from my point of view, I've been luckiest in the people that I've been able to attract

to the lab. I guess anything involving personal decisions always seems like there's that element of uncertainty, and so serendipity enters there. You know, I find it serendipitous that I'm doing science, and that I get to do this job.

VAN BENSCHOTEN: What are some of the long- and short-term applications of your work?

KERNAN: Short term, there's the chance still, I think, that we may identify some of the genes that are involved in mutations to human deafness. Some extremely intriguing points of similarity between fly bristles and human ears: they both have this very strange physiological mechanism in which the extracellular space from which the sensory cells draw the ion current that makes the receptor potential. That space is physiologically very peculiar in both systems, and peculiar in the same way in each system: it's high in potassium. Usually, extracellular solutions are high in sodium and low in potassium. So an inward receptor current is usually a sodium current. Here, in both fly bristles and in the human inner ear, it's a potassium inward current. That, however, the more we know about it, it seems to be a case of convergence, that is, not of similarity because of evolutionary descent, but a similarity because of selection; that, basically, both flies and humans have happened on the same way to solve some physiological problem, even though we're not sure why--quote--"they have chosen"--unquote--to do so.

But both sorts of sensory cells are ciliated. It's clear that there's a lot of developmental similarities between hair cells and bristles, so we think maybe some of the channels we're coming on now, the vertebrate homologs of those channels are expressed in hair cells, and that was shown before we found that they had an effect on fly hearing. They'd not previously been suspected to be involved directly in transduction, but we think they might be.

Other molecules, anything involved with ciliogenesis, like I said, things like many vertebrate sensory cells, even the sensory cells like photoreceptors, olfactory receptors, are also ciliated. We may be turning up molecules that have to do with either retinal development or retinal degeneration by turning up molecules involved in the general process.

Again, I put my money on the ciliogenesis projects for long-term payoff because they get to this very fundamental cell biological process of cell division, which is the basis of cancer. Polycystic kidney disease is a cell proliferation defect. It's a dominant inheritance pattern, but a loss of function mutation, which means that if you inherit one knocked-out allele, even a mutation in the other allele, in even a single cell, can lead to the formation of a cyst, which is the equivalent of saying a tumor, in this case, because that single cell will then, once it's lost both copies of a relevant gene, will then go and proliferate. We don't know what the relevant molecules— Why knocking out a polycystin gene in a cell leads it to proliferate, but because of the connection between basal bodies and centrioles, my guess is that it's involved in the centrioles and cell division, how they can be deprogrammed from their role as a basal body back into a cell-division-promoting centriole. So, currently that's, I think, potentially the biggest payoff from the work that we're doing.

But I've got to say, my motivation isn't driven by payoffs or by applications, even though I know that's where we get our money, but it's always driven by exploration, exploration back into the deep history of cells and into those things we don't know about.

The most exciting bit of research that I did in the lab myself recently, probably the only bit of research I did in the lab recently, had to do with polycystins, but finding a role in *Drosophila* for them. We were asking, okay, flies have polycystins, they don't have kidneys. They have an equivalent of kidneys, but polycystins don't seem to be expressed there and don't seem to have a role there. What do they do in *Drosophila?* It turns out they're expressed in sperm, only in males, and they seem to be involved in sperm function, not in development. But a normal female fly cannot store sperm from a male that has its polycystins mutated, or one of them.

This is a collaboration. The mutation was made by Terry Watnick working at Johns Hopkins [University] with Craig Montell, so we were lucky enough to be provided with the mutant, but I got to find out the phenotype. That ranks up there with the discoveries that I've made. That was really a lot of fun biology I was able to do this summer.

But it's a case of—It's that sort of discovery that we don't know what it means now. I think Terry in particular is having some difficulty convincing the people in her medical department that fly sperm have any value in terms of applications to medical problems now, but I still think things like that are worth pursuing.

VAN BENSCHOTEN: I'll change gears a little bit and move away from your research, current research, and talk a little bit about your duties as a principal investigator. What do you spend the bulk of your time doing as a P.I. [principal investigator] now?

KERNAN: Right now I'm not sure I spend the bulk of my time doing anything. It seems to be so subdivided, so interrupted. I certainly work better when I can spend the bulk of my time doing one thing, but right now my main teaching semester is the spring, not the fall, so right now I'm not doing a lot of teaching. But it's time to organize the graduate genetics course, which I direct, so I've got to assemble, recruit, and schedule all the faculty who teach that course.

Right now the bulk of the time, though, this semester is doing what I should be doing, and actually like doing, which is trying to keep up with all the projects that are going in the lab, interpret the results, and get them into publishable form, which has been our main failing lately, or my main failing as a P.I., not getting enough papers out fast enough. I don't have the excuse any longer that we really don't know what's going on yet, which we used to have in the days when we were doing positional cloning only, where you cannot publish a story, really, until you've found the gene. And you can't really predict how long it's going to take you to find the gene, much to my chairman's dismay. He would say, "Well, can't you give me a time estimate?" And I'd say honestly, I really couldn't, because there were so many contingencies

going into that sort of project.

But now we've got some of our genes cloned, we know what's going on, that makes things like collaborations a whole lot easier, and a lot more of my time now, just in the last six months, even, is taken up with communicating with collaborators. All of a sudden, we've got about five collaborative projects going on, whereas before, we had none. The difference is in, once you know what molecule you're working on, once you know what system you're working on, it makes making connections to other labs a lot easier.

VAN BENSCHOTEN: Do you still do benchwork?

KERNAN: Yes, but not as— In the summer, as I mentioned, I was able to. Probably that was the first time I was able to do some sustained benchwork. I'd say for about half the year, yes, I can manage to do some fly work, then, more often than not, those crossing schemes get abandoned half way through, because something comes up and I just don't get back to the flies in time.

It's been a long time since I did any molecular work. Electrophysiology I still do. I'm the person in the lab who does probably most of the electrophysiology. But our electrophysiology is pretty cheap and straightforward. A major step in the project was developing the electrophysiological assays, which showed that our mutants were, in fact, sensory defects. Things like each of the techniques had been done before, but actually using them for this purpose, that was a big advance. But a real electrophysiologist probably wouldn't consider what we do with much respect at all, but it's effective. It's fast, it's cheap, again, and it's geneticist electrophysiology. It's repeatable, so you can do it perhaps not as a first-order screen, where you're doing thousands, but you could think of screening up to a hundred lines, electrophysiologically, by this method, by those methods.

VAN BENSCHOTEN: Another duty you've already mentioned, teaching. I know you do some— I spoke with Nancy [M.] Hollingsworth today and, of course, both of you—I didn't know this—do a genetics course.

KERNAN: Right. We team-teach the genetics course.

VAN BENSCHOTEN: So, is it my understanding, then, that you teach one semester of the year, then.

KERNAN: No, we each teach half of that course, so she teaches the first half of the semester, I teach the second half. That's the major undergraduate teaching commitment. I'd say all the

various graduate courses and things in which I give a couple of lectures probably amount to—Would be the equivalent of the other half of the semester. So it's a pretty light teaching load, just one large course equivalent. Things do tend to slow down. Nothing much else tends to happen while particularly that large undergrad course is being taught, not because it's all that many contact hours, but there just seems to be enough stuff to do when you're dealing with 400 students; it takes up time. Getting all of my lectures into a PowerPoint presentation format was something I invested a lot of time in last semester, and hopefully, that will pay off in terms of making it easier to review and easier to update lectures, and easier to provide materials for the students as well.

VAN BENSCHOTEN: On average, how much do you travel during a year?

KERNAN: I would guess about, on average, one and a half meetings a year. That is, travel to one meeting every year and a second meeting about every other year, and maybe, these days, I'm getting maybe about two invited lectures a year, those mostly in the New York area.

VAN BENSCHOTEN: How do you feel about job-related travel? Is it something you want to do more of, or is that something that you sort of—

KERNAN: It's something that I wouldn't want to do too much more of it than I'm doing now, mainly for family reasons. I definitely notice the difference in my relationship with my kids [Ciara Emily and Thomas Piers Kernan] when I've been able to spend a full weekend with them versus not. So I don't want to be away from home too much. Karen [Kernan, née Kwik], my wife, started working and she's been working full time for a year now, and that's put an extra constraint on the amount of time we both can spend. But that said, though, job-related travel is definitely one of the perks of being a scientist. The idea of having this transnational community of scientists is something that I value a lot.

The trip that I went to most recently, where I managed to string together a European *Drosophila* meeting and a meeting on centrosomes and centrioles in Dijon, Heidelberg, and then visit collaborators in Lyon, that was a great trip. Particularly the centrosomes meeting was enormously valuable. We've got at least two collaborations coming directly as a result of that meeting.

Typically, I'll go to the annual *Drosophila* meeting every other year, and I've started going to the cell biological meeting, the ASCB [American Society of Cell Biology] meeting last year. We'll probably do that every other year as well. It's strange that cell biology is so much—Just as an undergrad, I never learned any neurobiology and I had to learn that. Never learned any cell biology in graduate school. That was the gap there. Of course, now we're doing the cell biology of neurons. It's been important to remedy those gaps in my education by learning from real cell biology and real neurobiology labs.

VAN BENSCHOTEN: Another duty you have is administrative responsibilities, and by those, I mean things like search committees.

KERNAN: Yes.

VAN BENSCHOTEN: Thesis committees. What are your responsibilities there as well?

KERNAN: Quite a number of thesis committees. Let's see, have I been— Yes, I was on one faculty search committee for the neurobiology department. Right now there's a strategic research planning committee for the medical school. The [genetics] graduate [program] admissions committee. I'm not doing anything now, but I did put in a fair amount of time on doing graduate school admissions. I'll be on that again for neurobiology next year. I organized the departmental seminar for a couple of years, in neurobiology. It all adds up.

VAN BENSCHOTEN: What are the sources of your funding?

KERNAN: Major funding comes from the National Institute on Deafness and Communicative Disorders, NIDCD, which funds a lot of the work on transduction, ciliogenesis, again, with the idea that gene discovery in flies might get us some of the candidate genes for deafness in humans.

The Pew [Scholars Program in the Biomedical Sciences] funding, of course, was enormously valuable. It helped us expand particularly the research in *nompA.* And then lately I've got a small two-year grant from Polycystic Kidney Disease Foundation, funding the work in polycystins. That's going well, so we're hoping to parlay that into an NIH [National Institutes of Health] grant.

VAN BENSCHOTEN: And how, sort of, day to day, how concerned are you about your funding? How much does that sort of press down on all your—

KERNAN: It comes and goes on a four- or five-year cycle, depending on—

VAN BENSCHOTEN: So when renewals come around.

KERNAN: So when renewals come closer, I'd say the renewal of the NIH grant was fairly touch-and-go. We might easily not have got it, but, luckily, we did. I think the— I don't tend to worry about it too much, but typically I underspend my grants. That is, in the past, we haven't got enough work done, so we haven't spent the grants as soon as we should, in some view. But it also means that we run a big surplus, so we've got a reasonably large safety margin. Luckily, the people who administer the grants have been reasonably tolerant and allowed me to carry over, so we've done extensions of grants for a full year.

VAN BENSCHOTEN: You did that with the Pew [Scholars Program in the Biomedical Sciences grant], right?

KERNAN: We did that with the Pew [Scholars Program in the Biomedical Sciences grant]. We've also done it with the NIH grant, which allowed us an extra— We got an extra cycle, an extra year before renewing them.

Overall, yes, it's a measure that work in the lab could probably proceed a bit faster than it is now because it feeds on success. Once you have some of the genes identified, that gives you a whole lot more to do in ways of experiments to think about. Positional cloning can be a bottleneck in that way. I justify to myself that, okay, the research is progressing maybe one year in six more slowly than it should, but I could be in the lab four more hours perhaps than I am, but I'm not sure I'd be as happy or as productive in the long run. So I'm grateful for the flexibility that the funding agencies have shown in that way.

VAN BENSCHOTEN: This will probably be my last question. I was going to ask, and I will, how much has the sources of your funding informed the projects that you've taken on? From what you've already said, though, I take it that the curiosity— You're driven by curiosity first. You go to a project and that's there first.

KERNAN: Right.

VAN BENSCHOTEN: Is that more or less the answer to that question, or do you want to elaborate on that a bit more?

KERNAN: I'd say curiosity first, yes, but I do feel an obligation that, you know, we've made promises to granting agencies to do— But the promises have largely been, "We'll discover what there is to discover about the systems," in the way that I've described it. So I don't think we're doing applied research. I do think it's important that we discover whatever the real story is. And by the real story, it takes into account that we may be wrong. The way we described our specific aims may not be according to the way that the cell is constructed, so if that's true, we need to

know about it.

VAN BENSCHOTEN: Okay. We'll leave it there and we'll pick up again on Thursday.

[END OF TAPE 4, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Maurice J. Kernan**

INTERVIEWER: **William Van Benschoten**

LOCATION: **State University of New York, Stony Brook**
Stony Brook, New York

DATE: **18 November 2002**

VAN BENSCHOTEN: This is tape five, side A.

We were talking, in our last session, about duties that you have. We talked about your teaching, recruiting students. What is the writing process in your lab for journal articles?

KERNAN: It is varied between whether I'm the primary writer of the manuscript or whether somebody in the lab is. Up till now, I've been pretty much the person who's written most of the primary articles, been the primary author, the person who writes the first draft of the manuscript. In part, that's because papers on which we've— The first-author papers we've had to date have been, with the exception of one by Dan [Daniel F.] Eberl, have been by people for whom English isn't their first language, so it's a lot faster for me to write a first draft and then have them check it out for adherence to the facts than it is for them to struggle with producing a first draft.

Right now, though, I've got a student [Young-Goo Han] who's from Korea, who's got a really good data set for a first paper on which he's first author, so he's just written the first draft of that. I think that's important that the first author, when possible, should be the person who writes the first draft of the paper. Then it goes to me and I'll probably take it apart and put it back together again. Then it will get bounced back a couple of times.

VAN BENSCHOTEN: In his particular case, how long do you think that process will be? Just sort of a ballpark figure.

KERNAN: It's already been probably about two months since I first asked him to give me a manuscript, and then he gave it to me just last week. Hopefully, I'll probably get it back to him later than I ought, but, hopefully, within about two weeks.

We haven't yet published often enough to really have it down to anything like as smooth and as efficient a process as I'd like it to be. What I'm trying to work towards is a system where we'll have— I guess I learned early on that right from the beginning you should be thinking about how the data is going to appear in a paper. That, although it's nice—and by tendency has

been—to sort of poke around in the lab in pure exploration, it's really good as soon as possible to think of the data you're collecting as— That every datum you collect should be the final version, should be as carefully collected as you would as if it was going to be the one that was going to be published.

You structure papers around figures and build sort of— Right now on my desk I've got about five different binders, each of which is an assembly of data which are being transformed into figures for papers in about five different projects. So I'm trying to use that as a tool for more efficiently assembling papers.

VAN BENSCHOTEN: You described earlier, in our earlier sessions, the lab management styles of some of the people you've worked under. How would you describe your own lab management style?

KERNAN: Much more towards the hands-off style than towards detailed. It's been pretty unstructured. I think I tend to like people in the lab who work more independently. I've always felt from early experiences in my life, where I felt that I was being criticized too much, not by the P.I. [principal investigator] in the lab, but by some of the senior postdoc[toral fellow] s in the lab, that for the most part, scientists are self-critical people, and you're not— If you see somebody doing something wrong, once you've alerted them to the fact that they're really not conscious of, after that, you're not going to say anything to them, probably, that they're not already saying to themselves. Usually it's too easy to err on the side of overcriticism, so if you tend to let people— Maybe point out something they might have done better, but tend to let them come up with their own fixes.

I've found that people who've come to join the lab and stayed in the lab, or done rotations and decided to stay, or undergraduates who've stayed in the lab for long terms, have tended to be those people who work best in that sort of environment, tend to be more independent-minded, people who are self-motivated. There's probably an element of Darwinian selection that goes on, so I'm not sure whether I serve all types of students best, but I think more independent people, I think, do well.

VAN BENSCHOTEN: One other duty you have is towards your professional community. What are the services that you give to that community, things like study sections, editorial boards? Are you part of those?

KERNAN: I've done reviewing, but I'm not on any editorial boards. I just probably haven't published enough to be visible enough to be asked. I've been on a couple of study sections. No major— One-time study sections: one small grant and a couple of ad hoc panels.

That was a real education, the first one of those I was on. Everyone ought to be able to

observe that process before writing any grant, any major grant at all. To see even a videotape of a panel in action would be a really useful thing, because what it really brought across to me clearly was how very, very important it was to make the grant reviewer-friendly, to structure it so that it would— It was so clear to me, having gone through it, that your only real advocate was going to be the primary reviewer, or possibly the secondary reviewer, but you had to make their job as easy as possible, that how very compressed, how very short a time they had to either make the case for your grant or not, and how it was very important to structure grants so that the strong points were laid out as easily as possible for them to communicate to other people on the panel.

I was kind of shocked at how— The first panel I was on was one that was for the National Institute on Deafness [and Other Communicative Disorders], reviewing small grants, but anything at all deafness-related, which meant that we had proposals from engineers, from space scientists, as well as from people doing laboratory biomedical work, that I was a little more familiar with, and I was shocked that my vote would count equally with anybody else on the panel in reviewing something like that. Most people, of course, probably followed the view of the primary reviewers, but it was interesting to see how much influence they had. So, yes, basically I found out in some ways that I've learned a lot more from that process than contributed to it.

I felt service to the community where I think I've done a bit more is in teaching in summer courses off campus. I was a student on the neurobiology course at the [Woods Hole] Marine Biology Labs way back in 1987, which is about a ten-week course, where it's a one residential faculty member to two students ratio and, in fact, it's three residential faculty members, over the entire length of the course, to two students, so it's a big investment in a small number of students. I felt like when I was asked later on to be on the faculty, I felt like it was something I really should repay. Being a student there was one of the best scientific experiences I'd had, and one of the most intense scientific experiences, and something that really bolstered my conviction that this was the right life for me. So this coming summer, I just got invited to be on the resident faculty again there, and accepted, and I'm really happy about that.

So I do a lecture and a summer course in *Drosophila* neurobiology also at Cold Spring Harbor [Laboratory] most years, when it doesn't overlap with the Marine Biology Lab course.

VAN BENSCHOTEN: That's at Woods Hole, right? The Marine Biology—

KERNAN: The MBL [Marine Biology Laboratories] is at Woods Hole, yes. One of my favorite places is the Woods Hole, and particularly, the library at Woods Hole. I don't know if it's a sign I'm getting too old already, thinking of places I'd like to retire, but that will be one place for an ideal retirement, at least during the summer.

VAN BENSCHOTEN: So it has a really good library, then?

KERNAN: It's got a great library, a really old library. They've got holdings that go way back. Some of our scientific interests in cell biology and centrioles go back—I've got a hundred-year-old datum that I like showing and that comes out of some of the early cell biology work that was done there.

VAN BENSCHOTEN: How do you negotiate the demands of work and family life? That's very pertinent today, isn't it?

KERNAN: Right. We just got off to a late start because I'm— It's an unusual situation. My wife [Karen Kernan, née Kwik], who's been working full time for a year now, has just gone away to her first conference, so this morning, I was, for the first time, in charge of getting both kids [Ciara Emily and Thomas Piers Kernan] on the bus and settled at daycare.

It's always a tension. I think one of the transitions in my life I've found somewhat difficult was the idea of conflicting demands. Right until most of my time through graduate school, I was either unattached or then living with a woman, not currently my wife, but we didn't have children then. I was pretty certain that I did want children, but when you're unattached or in a relationship like that with somebody else who's also a student, a graduate student in the same program that I was, that I was living with then, it was pretty much there is only one— To be a good student was really the only demand on your life. When you're living with another scientist even meant investing enough in a relationship was— When both people are scientists, there is a certain tolerance for the amount of time you're going to spend in the lab.

So the answer to how should you live your life, a lot of it was just be as good a scientist as you could, and things were simple. If you were blowing off the lab to go sailing and, you know, you knew a certain amount of that was fine, but it was much easier to keep— It seems now, I'm not sure it seemed like it then, that there was plenty of time to go around, that you had enough time to do something well. Now it's like my general principle is, that if I feel like I've got a handle on things and everything's going pretty well, there's something really important that I've forgotten. [laughs]

VAN BENSCHOTEN: Or more than one.

KERNAN: So it's always a tension. I think one of the reasons I came to Stony Brook, though, was because it's such a good place to bring up a family. That's a decision I really haven't regretted. It really has been. I think I mentioned, when I was talking about job searching, I said that was a major consideration. I turned down offers at places that might scientifically have been better, or at least perceived as being more scientifically prestigious, in order to come here. I still, most days, don't regret that. I've been lucky, I guess. Maybe I should— There's a story to be

told that I haven't told yet, which was how I met my wife and how and when we got married, so I'll digress a bit.

VAN BENSCHOTEN: Good. Good.

KERNAN: So, Karen [Kernan]. We'd met actually when I was an undergraduate, but she's American, I'm Irish. I did my undergraduate degree in [Trinity College] Dublin. I met her through family right before she was due to spend her third year of college in Dublin, studying Anglo-Irish literature. She's an English major. She was at Cornell [University] then. Probably the fact that she was at Cornell probably had some influence on the fact that I spent the following summer at Cornell doing research, even though we didn't get romantically involved in any way then, or for about ten years afterwards, but we maintained contacts to a greater or lesser degree. I was in a relationship with somebody else while I was a graduate student, for most of probably— Yes, most of the time I was at graduate school. We were living together for a couple of years out of that time, and that eventually— The combination of just needing to move on to a postdoc but, more importantly, the fact that pretty much she didn't want children and I did, and I couldn't see— It was one of those nonnegotiable, insuperable obstacles that a relationship founders on, and that one did.

So I ended up going by myself to San Diego and found my first— The first nine months at San Diego was probably the most difficult time of my life. It was the time I came closest to being severely depressed. My measure of it then was that I lost about ten pounds, which is unusual. I hadn't all that much weight at the time to lose. I'd mentioned way back that there may be a genetic strain of tending toward depression running in our family. I don't know how much that contributed, but it could be explained situationally. The combination of moving to a new place, which was quite alien, and the break-up of a relationship; the combination was pretty distressing.

VAN BENSCHOTEN: I can imagine.

KERNAN: So I think Karen, around the— I'd moved in September, October, and Karen came out to visit me in the spring. I think I was aware that we'd had this sort of probably something more than platonic relationship for many years by then, and I felt that things had been brought to a state that obviously I wasn't meant to be alone and I needed to see if it would go, so I asked her to stay, and she did. At the time, she'd done a master's degree in English literature at Harvard [University], having graduated from Cornell, and was teaching at Rutgers [University], but she left that to come out with me, for which I'm extremely grateful, then and since. And we were married in the Mission in San Diego in '92.

So we didn't have any children while we were in San Diego and were beginning to get— Not for want of trying. I think we were beginning to get to the stage of probably a lot of thirty-,

thirty-something-year-old couples of about eighteen months or so, of when you wonder are you going to have children. We were beginning to wonder if we would, but then Karen got pregnant shortly before we moved to Stony Brook, and we've had another child since.

VAN BENSCHOTEN: What does she do now?

KERNAN: Karen now, she's worked at— In San Diego, she worked as an environmental consultant. She has an amazing talent for turning to and being expert at whatever she picks up. She'd worked at Stony Brook, between children. There's a three-year separation there. She worked part time for a local software company that's run by one of the biology professors here. Then from September of last year, she's been working full time for the university as the director for undergraduate research. She coordinates a lot of the fellowships and writes grants to get undergrads into research labs.

VAN BENSCHOTEN: You have two children. Can you give us their full names, just for the record?

KERNAN: Our daughter, who is seven now, is Ciara Kernan. It's an Irish name, C-IA-R-A, very common [in Ireland], but in Long Island, you always have to explain to people that it's not Italian and not pronounced "See-ar-ah." My son, who's four, is Thomas, after my father, Thomas Piers Kernan. And they're great. They're, on the whole, two pretty easy and manageable kids, which doesn't mean that it's still— I think in the first years and in the year or two after Thomas was born, when you make the change, the big change from one child to two, those were still stressful in terms of demands on time, though a lot of the time Karen wasn't working, or was working only part time, so she did the bulk of the childrearing.

VAN BENSCHOTEN: How do you divide the mundane tasks now, things like laundry, preparing food? How does that normally go?

KERNAN: Karen does laundry, finances. I'm usually the cook, and I do— We probably divide up the shopping, but I'll do most of it. Karen tends to be the person who does— We just took the big step of going from one car to two cars, which is having still some environmental conscience I feel bad about, but it was necessary, because just one person picking up everybody else after work was just too much time.

So we tend to divide child pick-up and drop-off, and I was just discovering this morning that, yeah, Karen probably does do most of the things in the morning. [mutual laughter] You know, making sure they're both dressed and shoes are on the right feet is mostly her.

VAN BENSCHOTEN: What do you do for fun and leisure, when you want to decompress?

KERNAN: What I would like to do and what I what I actually get time to do are two different things. I actually find cooking relaxing, so if there's something I do every day, it's that. And I still read an awful lot, usually between— Later than I should at night, so usually between the hours of eleven and sometimes midnight, sometimes one a.m.

VAN BENSCHOTEN: What are you reading now?

KERNAN: Right now, Stephen [L.] Carter's novel, *The Emperor of Ocean Park,* I was reading last night. A book by Mark Ridley called [*The Cooperative Gene: How Mendel's Demon Explains the Evolution of Complex Beings],* sort of selfish gene theory.

Let's see. The other thing I like doing and was one of the nice things about Stony Brook was just what I call pretty much destructive gardening. One of the nice things about Stony Brook was being able to— When we moved here, property was cheap enough we could buy a house that was on point-seven of an acre, with some trees on it. For someone Irish, that's particularly significant, because there aren't very many trees in Ireland, not because they can't grow there, but because they were all chopped down a long time ago, and the only place you'd find trees was around wealthy houses with lots of land, so it's sort of something of a status thing, having mature trees on your property, even though at this time of year what they do, of course, they're all oak and they dump several tons of leaves, which I'm in the middle of raking right now. The whole place has to be raked several times over to clear off everything. But compared to our neighbors, our place looks pretty unkempt. The way I like to put it is that we manage our property for maximum biological diversity. [mutual laughter]

VAN BENSCHOTEN: That's a good way to put it.

KERNAN: We don't do much, but I still like wielding an axe and chopping wood and stuff like that. I find that's relaxing.

VAN BENSCHOTEN: If you would, describe a typical workday, from the time you get up in the morning till the time that you put your head on the pillow.

KERNAN: Get up, drive in, have a momentary thought that I really should be cycling in. Park in a somewhat distant parking lot. Walk through the encounters with nature, or the shortcut through the woods from the parking lot, which actually— That actually means quite a bit. Of

course, it was people from the ecology and evolution department who showed me the shortcut through the woods, which they all tend to take, being closer to nature.

Get in at about somewhere around nine o'clock, check e-mail. If I've had a well-planned day, I've had waiting for me a list, on the rare days, of the things I ought to be doing, or at least the first things I should be, but didn't get done the day before, and try and catch up with that. Good days are days when I actually get a fair way through the things I intended to do. Bad days are the days when the interruptions start and pile up almost immediately.

What I still find I'm getting faster at, but still find takes a long time, is writing anything, and that could be anything even from responses to e-mails, response to something I've— I've actually taken some pride in carefully maintaining relationships with collaborators and, say, the letter or e-mail that initiates a collaboration, I think is pretty important, so I'll take probably too much time over that. But I don't yet have the knack of writing naturally, so everything has to be written as gibberish first and then revised.

Most days it'll be that sort of writing. Somewhere around mid-morning I'll try, if I have any fly work going, I'll break away and do that for a while and talk to people in the lab while I'm doing that or on my way down to the fly bench.

Grab lunch, usually eat in my office. Somewhere in there there'll be a class to prepare or an upcoming presentation to prepare. Thereafter, the day sort of degenerates. I find that if I'm doing experimental work or seriously committed to doing experimental work, which is usually either fly work or electrophysiology, that'll get done in the afternoon. I've considered that even since we've moved into this nice office, it's probably bad for me because it's a nice place to hang out.

VAN BENSCHOTEN: It is.

KERNAN: And it probably would be better if I had less of an office and just even came into the lab and put my bags down on the floor first at the lab bench. It might be a better way of initiating the day. I tend to be probably too easily diverted from a plan, so I'm the victim of whatever e-mail I got last, or whatever the last interruption was.

Serious time management. I'm not sure I'll ever achieve it, but there are a couple of things to do. One of the most important time management and lab management tools is this, which I've just picked up, which is the door stop, which keeps open or closed the door between the lab and the office. Most of the time, I like to have both the door to the lab and the front door to the office open, and the principle is that anyone can come in and interrupt me at any time. That's not good for time management, but if you're going to have a hands-off lab management style, that's important: that you can be interruptible.

VAN BENSCHOTEN: What time do you usually knock off and go home?

KERNAN: Almost always, these days, I'm picking up one child or the other, which means a hard deadline of leaving the lab no later than five-thirty. Then, some days I find if there's an upcoming deadline or I have a lot to do or I really felt that I didn't get anything at all done, I'll come back into the lab after around eight-thirty or so, after having done some of the work, cooked dinner or helped get the kids— If it's eight-thirty, the kids are not in bed yet, but I've helped to get them some way towards there.

VAN BENSCHOTEN: Then, I take it, when you return, since you've already mentioned, you know, reading, you probably read and go to bed too late?

KERNAN: Yes. Right.

VAN BENSCHOTEN: Assess, if you would, your efforts so far in achieving your professional goals.

KERNAN: I seem to keep being on the cusp, if there's such a thing as an extended cusp of achieving my professional goals, but I would say, yes, I have achieved some of them, some of the personal ones, but I feel like I haven't yet paid back the investment that's been made in me. I think we're on the right track to doing it, but we're not there yet. It may well happen in the next year or two, I feel.

For the first time, we have multiple papers that could come out in the next year, and it really has been, over the last year or two, the fact that now we have gone from being a lab with a collection of mutants, but didn't know which gene we worked on. When people asked me, "What do you work on?" the straight answer was, "Well, I really don't know." That is, we had the mutant with the phenotype, but we didn't know what molecule and we didn't really know what cellular mechanisms we were working on. Now we do. We can divide things up into whether we think they affect transduction or they affect cell differentiation, ciliogenesis.

We've got handles on molecules, and what I find is that collaborations come much faster when you can— They're much easier to say, "I work on TRP-related channels," or, "I work on ZP [zona pellucida] -domain proteins," or, "I work on intraflagellar transport proteins." That gives people a much easier, faster grasp of the sort of thing you work on than saying, "Well, we've got this mutant and it sort of does this and we really don't know what it's involved in yet."

In a way, we had to go through that in the earlier stage, and the payoff is coming now, where we're finding either novel molecules or familiar molecules but involved in novel mechanisms.

The graduate student [Young-Goo Han] in the lab, who just gave me that manuscript, he's found a new— I think it's going to be a really nice last section of his thesis that will deal with a completely new phenotype for what was, up until now, a rather familiar molecule. Where our publication would have been just another "me, too" story, showing that something— We've now shown that the molecule in flies is doing something quite different from—in addition to the things we expect—the things that have been already well characterized in other systems. So the new and unexpected is always good, especially when you're a geneticist. And [I'm] glad for Young-Goo that his graduate thesis is going to be, hopefully, even more interesting than mine was.

VAN BENSCHOTEN: Where do you see the lab going, and yourself, in, say, the next five years?

KERNAN: For the three main areas that we're working on, for transduction, we have to decide whether or not to make a real stab at reconstituting the transducer system. Now we've got a couple of ion channels, which was the missing ingredient, that we can try and do this with. That means getting serious about going back to cell culture techniques, deciding which cells to try and express our molecules in, and how to do some real electrophysiology on them. Not only doing standard cell culture techniques, but then doing something different, which is to try and put our candidate extracellular ligand *nompA* protein on something that we can mechanically manipulate when it's in contact with cells. I've been talking about this as a thought experiment for a long time, but now we've got to put up or shut up.

The cell differentiation work is something that I think could be extremely fertile. We'll try to pursue both. The lab, right now, is a little topheavy in that we've got two fourth- and fifth-year postdocs, who are now actually no longer postdocs. They have fourth-year research scientist appointments. They'll be gone within— Maybe towards the end of a year from now, or certainly within the next two years, so I've got to do some recruiting and I've got to do some fundraising to make sure that set of projects go ahead.

The polycystin project, I hope, is going to turn out well. In general, I think it's important for a lab to throw off enough new and different projects certainly that people— The postdocs who've worked on them can take them with them. And that's something I'd like to— It was always a source of some tension in the [Charles S.] Zuker lab as to whether postdocs would be or would not be competing with Charles's lab, which was a large, well-funded Howard Hughes [Medical] Lab, highly competitive. In my case, somewhat less so than others because I was working on a new— My project was different from what the bulk of the lab was doing at the time, but, still, by the time I left, there was, I think, another postdoc and a couple of students working on the same project. Since we had done a mutant screen that had thrown up a lot of mutations and a lot of—as it turned out, thankfully—different mechanisms, there was plenty of work to go around, so it didn't turn out to be an issue.

But I still think it's important that postdocs, towards the end of their career, be given the opportunity to— The bulk of the work that they do in the lab, if it's giving rise to a new project, it should be a well-defined, portable project that they can take with them and establish their own lab without fear of having to grow out from under the shade of a tree. Not that our lab currently casts a terribly big shadow.

So I hope that we'll continue to throw off new projects and new ideas like that.

VAN BENSCHOTEN: How about in ten years, or is that too far down the road?

KERNAN: In general, I would like us to have— I don't know. I construct these imaginative theories, you know, that serve as not really even hard experimental hypotheses, but as ways of structuring ambitions. So part of the reason we're in polycystin research, these molecules that are involved in kidney disease, was that we thought there was a sort of grand unified theory in which our extracellular proteins, ZP domain protein, would be generally the extracellular ligand for polycystins and that we'd have a whole new signaling system here, and we'd sort put together a table of instances in which this seemed to be the case. It's probably not true in exactly those terms, but there may still be something to it.

So, yeah, I could spin a story right now as to "Wouldn't it be great if—" lines, which is quite often how I start generating theories, "Wouldn't it be cool if this happened." My current "Wouldn't it be cool if—" is that ion channels have a lot more to do with the cytoskeleton and the regulation of cell division than currently they're given credit for.

The ion channels have typically been studied at the periphery of the cell, where most of them are located, and studied in isolation from cytoskeletal structures. If there has been a connection made, it's the effect that the cytoskeleton has had on the ion channel function, and ion channels have been studied mostly by people who care about how a cell works when it's mature, whether it's a neuron or other cell.

I think there's the potential for connections to be made between ion channels, particularly calcium channel feedback on cell divisions, with the particular case being the polycystins, which are probably located on cilia, which cilia grow out of basal bodies, and basal bodies are the same thing as centrioles, which go back and organize cell division. When you have a defect in a polycystin, you get—at least in kidney cells—an over-cell proliferation defect, and I think that's really significant. So one big question is to follow that line of reasoning as far as we can.

The big issue for us that's connected with that, is when do we make, or do we make, the jump from *Drosophila*, or do we always stay in flies. Flies don't have dividing ciliated cells. Their ciliogenic cells seem to be at the— They're only in cells that are completely differentiated and don't go on and divide. So we're not in the best system for tackling that particular issue. We can't do it now. We could only do it if we did another cycle of recruitment and probably if we recruited a real cell biologist. But we're right in the middle of this integrated center for

developmental genetics in which there are good cell culture people, there are good cell biologists, so we're in the right place to do it if we so choose.

VAN BENSCHOTEN: The next set of questions takes sort of different tack. They have more to do with technology, patents, ethical questions as well. Do you have any patents?

KERNAN: No.

VAN BENSCHOTEN: Okay. And what is your own view about— I mean, are patents good for academic science, in your point of view?

KERNAN: A nice profitable patent would be awfully good for me. [mutual laughter] And it would be awfully good for Stony Brook, too. You know, this is not a rich university. They have one or two big patents that have brought in a lot of money and are extraordinarily important for the well-being not just of the research enterprise, but of the university as a whole. There's no big endowments. They don't have a big alumnae pool, and the alumnae they do have are not, on the whole, rich. So, for this university, the possibility of patent income, which is generated— And there's been so much investment in research here the possibility of patent income— I wouldn't want to turn down any patents for the sake of the university and, in addition, for our own sake.

All the same, though, I have this suspicion arising generally from the observation that healthcare costs keep going up, that although the research enterprise in the U.S., generally, is seen as a way of— You know, as being a great boon to biomedical care. It could also be viewed as a very efficient way of making healthcare more expensive, that it introduces— That the whole paradigm of where research leads to drug, first of all. The fact that the end point of research is seen as a drug, and that, therefore, you have to go through both a pharmaceutical company and the extremely expensive drug-testing process, means that you're bringing many, many levels of layers of people, each of whom have high salaries, into a process where— That seems to be the obvious reason why healthcare is getting so expensive, and you know, by staking patents on biomedical territory or on extending it to genomic sequences, it's making it worse.

So it should not have to be that way, because good information—good biological information—should lead to cheaper medicine. It should be the case and maybe it will, it just hasn't got there yet, that you should be able to have more efficient treatment, be able to select people, tailor treatment to people better and cheaper. So many of the techniques that have been used, like PCR [polymerase chain reaction] or DNA array, because they're miniaturized, they're cheap. The only expense is the cost of development. Once you have it developed, they're pretty cheap and they're probably a lot more expensive than they need to be, and that's because they're developed under this connection of research to private enterprise.

I don't know nearly enough about the economics of it to theorize that it could be otherwise, but I just have my suspicions that I'm part of a mechanism that both while it advances our knowledge of medicine, in fact, it's a way of making things more expensive.

VAN BENSCHOTEN: A question about ideas. Where do your ideas come from?

KERNAN: In the shower I say, "Gosh, wouldn't it be cool if—." [mutual laughter] They come from that. I'm a lumper rather than a splitter. I like to make connections between things. I'm sort of a scientific conspiracy theorist. I like to imagine probably more broader and far-reaching connections than might exist and then see— That means most of my theories turn out to be wrong.

Ideas come from thinking about things in evolutionary terms. Going back, say, and asking, which came first, the cilium or the centriole or the basal body? Did centrioles have an origin as basal bodies of cilia, that is, the motility function of the ciliary function came first, before they were co-opted as a mechanism for dividing cells? Or did their mitotic function come first and then after, [were] co-opted as cilia?

And how would we find out that? What experiments would you do that would give you a certain answer? So it's the kind of posing questions to myself that are a little flaky and vague and the tension between that and trying to say, okay, how do we get to it? What can we say for sure? How do we get to an answer that we can be really certain about? Putting the unknown territory on the map.

VAN BENSCHOTEN: Another question is about the history of science. Did you take any history of science classes at any point in your education? I know that you read— I mean, you were reading science.

KERNAN: I read a lot. One of the really important books that I read was Horace [Freeland] Judson's book, *The Eighth Day of Creation.* I think I read that as an early undergrad[uate]. It was from our local public library in Dublin. I got that out, and that's a very readable and very appealing book, and probably one of the major things that converted me from the ecology, field biology track into molecular biology and genetics. I don't know why I hadn't mentioned it before. It definitely give the aura of this sort of—something that I both am susceptible to and skeptical of—the golden age of molecular biology theory. Was it really that there were these people and that only those people could have made this field, or would the field have developed at that time no matter who was doing the science? And I don't have the answer to that.

VAN BENSCHOTEN: We're near the end of the tape.

[END OF TAPE 5, SIDE 1]

VAN BENSCHOTEN: This is tape five again, side B.

I'm sorry. I interrupted you when you were talking about [Horace Freeland] Judson's book [*The Eighth Day of Creation]* and you were talking about—

KERNAN: The history of science generally. I find thinking of histories of things comes naturally to me and, probably, my first papers, I attempted to write in a historical sequence, that is, reporting the data in the order that we found it, and it doesn't work. Not always. Or it rarely works to report it in exactly the way you found it, because in order to compress for clarity's sake, you've got to fake it. You've got to present the data as if it was better organized and the experiments more clearly thought out than they actually were. But I still like to think in historical terms. Maybe it comes from being Irish, but you both think in historical terms or are historically aware and, at the same time, aware that—again, for the Irish—one of the things that has been their problem is being a prisoner of your history. So it's good to know your history, but not to be bound by it.

VAN BENSCHOTEN: Joyce said, "History's the nightmare that I'm trying to wake..."

KERNAN: "From which I'm trying to awake." So it's an issue for any teacher of genetics, actually. It's the big question for every genetics textbook and every genetics course. Do you start with [Gregor] Mendel or do you start with DNA? Do you start with what we now know is the mechanism or do you start with what some people would think is the more satisfying way of starting with the observations and then working your way through the stories? I think I would tend these days to think that it's not fair to expect students to work their way through— To share the pain, to expect them to work their way through the history, that you should convey our best understanding of the way the world works first, and then throw in the history where it's relevant, but not necessarily do Mendel first.

VAN BENSCHOTEN: Right. So it supplements it, in a sense.

KERNAN: Well, not so much— Well, yes, so historical insight might supplement it, but that can come later. "We know this because—."

For some selected experiments, where the design of the experiment is so nice and the design itself is so informative, then it's worth establishing we know why we know what we know, and going into the background for the experiments. But there's a certain amount of

nostalgia that everyone who learns genetics takes on board and likes talking about, and that can be jettisoned for a lot of genetics courses, I think.

VAN BENSCHOTEN: Do you feel that scientific progress is inevitable?

KERNAN: I think that it's unidirectional in the sense that it is real. We do learn more about the world. We do know more about the world than we did before. If I didn't think that, I wouldn't enjoy what I do, because what I enjoy most, the top moments in my scientific career have been: now I know something real about the world that I didn't know before, and perhaps that no one knew before.

But I think the idea that [progress towards] enlightenment is always historically [inevitable]— And scientifically, enlightenment can be taken for granted, I don't believe anymore. Scientific knowledge and knowledge of the scientific world is not natural to people. You can see, I think, in the last decade or so, I think we've regressed rather than progressed in that way, that there's more people who view the world in a less empirical way than there were before, and those people have more influence than they did before. So I would say that maybe for too long, scientists and people who view the world in an empirical way have been too quiet and too reticent— and, of course, I'd include myself in that—about defending not just their turf in kind of the narrower funding-for-research way, but also in the way of why this is the best way to look at the world. Why, when you see things that are just wrong scientifically and empirically, and are a bad way of looking at the world— It goes back to the idea about bad information. There is such a thing as bad information and it should always be defended against. There're lots of harmless errors out there, but there are some really harmful ones, and our only defense, at bottom, is either religious principles, which don't work because they are different for different people, or empiricism.

VAN BENSCHOTEN: A question about technology. What effect has technology and technological innovation had on your work in research?

KERNAN: We've benefited by it without contributing all that much to it. My own technological innovations have been so homemade and crafty, things like my little fly motel or the way of doing auditory recordings for flies. You know, when it comes to my own work, I'm very cheap, very— I like making things simple, and elegance for me means frugality and cheapness.

However, we've been benefited enormously from confocal microscopy, which, you know, we've got a nice, new Leica sitting across the corridor, which [is] shared with other labs, and that's a fantastic tool, especially now that we're trying to do GFP [green fluorescent protein] tagging of proteins that localize to very small structures in the cell, and in very small amounts. We would not be able to acquire that data if we didn't have access to that technology,

so we benefit by it a lot. It's a truism, for cell biology especially, that the field is driven by technical advances.

VAN BENSCHOTEN: I mean, there are some who will argue, too, that this technology's becoming more and more expensive, and that it is creating a new division between, you know, one group of labs, who have good funding and can buy these gizmos, and the others that don't. Do you feel that that division is growing, or even that that division exists?

KERNAN: Yes. I'm not sure. It's related to the issue of why medicine is so expensive, in some ways. Once developed and patented and out there, instrumentation has to stay expensive in order to keep the revenue stream flowing. Perhaps things like confocal microscopes could become cheaper and there would be a way for some scientific equipment— There's a tendency, just like in cars or whatever, to add more bells and whistles rather than to develop a cheaper, more streamlined version of something that will do the job. It might be good if there was some more simplicity there.

The confocal is probably the most expensive thing we use, and it's shared between about five labs. Well, it's shared between more labs now, but used intensively by about five labs. That, so far, is working out well as a way of doing things. I think, yeah, I'm talking from the site of a lab that's been funded for a while, so probably I would be saying things differently—

I think there's a responsibility on people like paper reviewers and journal editors not to demand— I remember, for a while, it was back in Barry [Ganetzky] 's lab, papers were turned down because we hadn't done single-channel recording, or single-channel voltage [unclear]. There really was not reason to. It was just there was that technology was there, it was state of the art, and every paper ought to have it in it, even though it wasn't necessarily going to answer the scientific question that the paper posed or answered. So it was an issue of whether technology can be applied or not.

There's a lot of technology out there now that's in search of questions. A lot of the genomics, but that's inevitable. Genomics has been such a big boon to labs, and I think it has made that sort of centrally available technology that produces a lot of information as publicly available is great. It's been nothing but positive for researchers, at least.

VAN BENSCHOTEN: This question, too, is apropos of—

KERNAN: I'm sorry. That comes from my own experience of positional cloning. When I had to do that, by going from having to positionally clone, do a 200 kb [kilobase] chromosome walk by hand. Three years of a graduate career. Most things are better than that.

VAN BENSCHOTEN: This question gets a little bit at— You mentioned Judson's book, *The Eighth Day of Creation.* We often read, or the public does, at least, of a biological revolution taking place. In your opinion, I mean, has that happened?

KERNAN: Yes.

VAN BENSCHOTEN: In what way? What type of revolution?

KERNAN: Because this is a time not like no other. Over the last three years I teach the genetics class, I tell them, "This is unique. This is a unique time in history. Never again will you be the class that have learned genetics in the year when you know your own genome sequence. The last class didn't. You do. You've got 35,000 genes." We didn't know that last year. Now we do, with some error, but we know to a much greater certainty than we did before.

The impact that I'm most impressed by is the impact in the legal system, the impact in courts. That's going to have really, I hope, deep philosophical underpinnings, because it goes all the way to— One of the attractions of molecular biology is the certainty of the information. You clone, therefore you can amplify a thing, and something that was a fuzzy piece of datum becomes a yes or no answer. It's this base or that base, and there's no arguing with that. You can argue about interpretations, you can argue about the statistics behind it, but you can't argue with the primary data. That's been the big attraction for so many people to go into molecular biology, and probably was for me, too, being able to approach questions and to be sure that, however difficult it was to get to, that certain answer was there waiting for you.

Translate that degree of certainty to DNA identification. This morning I read, you know, a guy who, I think in St. Paul, for the first time, a prosecutor initiated a DNA investigation in retro— You know, freed a man who was convicted of rape sixteen years ago. That's fantastic. Bringing that degree of certainty to a legal area, where there was an equal degree of certainty but with far less justification, has now led to things as far-reaching as being able to question people's trust in eyewitness evidence, very justifiably. So that degree of knowledge about ourselves is something that is— That's an instance where I think the effects are largely good, being able to exonerate with certainty.

Of course, there's the flip side of the coin, which is that now we can have large-scale microscreening, mass screening for DNA polymorphisms. That's possible in theory, probably within a year or two in practice. That may or may not be a good thing, depending on how much information you think governments should have. I think one of the questions I like posing to a genetics class arises from— I give an example of, you know, to what degree genotypes should determine the way we treat people? There's a paper came out a few years ago now, saying polymorphisms in serotonin transport made people more or less anxious. So I posed the hypothetical question, as a letter from a student addressed to me as a genetics lecturer. You know in existing practice, we give accommodation to students with disabilities, and disability

means, in effect, they can go to the Student Health Service, or the Student Disability Service and get a note from them saying "So-and-so has a disability"— unspecified—"that will affect their exam-taking performance." That means they should be able to take the exam in a separate room and have extended time for it. That's current university policy.

I say, "Okay, so here's a letter saying somebody says, 'Dear professor, I'm homozygous for the long polymorphism of the serotonin transporter. It has been shown that—which is true—this is associated with increased anxiety levels. Therefore, I would like to claim special circumstances under which I should take the exam.' Should they be given that?"

VAN BENSCHOTEN: That's good.

KERNAN: The class is always divided, and you can influence the outcome of their answers by telling them, for instance, that most people carry the more anxious polymorphism. So that means that we've got to make the minority of the class take the test under more anxious, anxiety-inducing conditions [unclear].

VAN BENSCHOTEN: That's a good example, too. It hits home.

KERNAN: But I like bringing the example to an issue that they really care about right there and then, and say, "Okay, you have this decision. What do you do?" I like to leave it somewhat open, but the bottom line, if you don't judge on genotype, you judge on— If you're going to make the judgment, judge on performance, not the genotype. We don't know enough about the way genotype causes phenotype. It's not justifiable to do it. And also that you don't know the way in which— The chain of reasoning is false, because you cannot make— Because a polymorphism may be associated with anxiety, and anxiety may be induced with lower test performance. You don't know that the type of anxiety caused by the polymorphism is associated with test performance. It might make you more anxious, so you might study better. So you've got to be very sure. No, we cannot be sure enough of that chain of reasoning in order to make judgment based on genotype alone.

The other way in which genetic information— I think it goes deeper in that sense, in that I think it really will ultimately challenge the way we view our sense of self. "Who am I? Am I determined by my genotype or not?" It's always been easy to say, you know, people have a self, have the idea of themselves as independent agents. They make their own decisions. Those decisions are the product of something that they view themselves as independent agents in making those decisions. I've always been a little less sure about that, because, in some ways—I've always, in my own career track, I'm undecided. Have I always been following the path of least resistance, and have I been governed by circumstances the whole way along? I don't know. It's clear to me that one of the things that people do best is post-hoc rationalization, making up stories to justify the things that they've just done, and rationalizing why that was an independent

decision, according to them.

So that is going to be challenged by genotyping, by the ability to correlate at least quantitative traits with screening for many polymorphisms, or ones that might be associated with those traits.

VAN BENSCHOTEN: In an earlier session, you talked about you have tenure now and how important tenure is. What is the tenure-getting process here at SUNY [State University of New York at] Stony Brook?

KERNAN: Fairly conventional. By the end of your sixth year, a package is put together, letters are solicited from outside people, asking for opinions. Teaching is perceived as being quite important here, and perhaps more important more recently than in the past, since the university as a whole has moved towards putting more emphasis on looking after undergraduates, at least in intent. I'd say the bar has not been set discouragingly high here. The tenure process, as it turned out, was actually fairly reasonable, even though it never seems so as you're going through it, and it was a close enough thing for me, I think, due primarily to the lack of publications, or low numbers of publications, that I was worried about it. But since I'm by nature a worrier, a concept like tenure, I've found, has actually made me, I think, a better scientist. Hopefully, I won't ever get too comfortable, but it's certainly given me the freedom to expand research into areas that may or may not pay off, to ask questions that might or might not pay off.

VAN BENSCHOTEN: Questions about competition and collaboration. Is competition generally good, do you believe, for science?

KERNAN: No, not always. Again, I'm not a confident scientist, therefore, I'm usually— In the past, I've been worried about losing out in competitions more than confident of my ability to win a competition. I would rather collaborate than compete. I'm not sure whether that's a good thing either, that you can have too many cozy arrangements. But it seems like the brass-ring model of science distorts things. The idea that, "Here, let's set up a goal," and let's everyone chase after this goal for a while in a thundering herd, and somebody gets it, and then everyone chases off after another goal, is not necessarily the best way of doing things. It is if the goal is worth achieving, perhaps, but a lot of effort is wasted on a lot of— You can't but devalue other areas by fixing on one as something more important.

It's a spur to complete, though. It's needed, particularly, again, for somebody like me, perhaps, to complete papers, to get things out faster. Ideas do need advocates, so if there's a scientific dispute, it's probably no bad thing to have one person invested a little more heavily than complete objectivity would dictate in one interpretation, and somebody else invested in the other. A little bit of the legal adversarial model is probably good, just to make sure all ideas get

thoroughly aired or tested or defended.

But temperamentally, I would rather not find myself in competition. I'd rather, "Oh, look, we're working on the same gene. How about we do this, you do that, and let's try to avoid duplicating the work," is typically the situation I'd rather find myself in.

VAN BENSCHOTEN: You've talked about some collaborations, but talk, if you would, I mean, what other collaborations do you have now with scientists under way?

KERNAN: Under way. Let's see. Working from ones that have already resulted in publications, one of our mutations turned out to be an identified protein involved with a process called intraflagellar transport. There seems to be a conserved transcription factor that regulates genes involved in the process, including, we think, our gene. We've been collaborating with a group in France who had isolated mutations affecting the transcription factor, regulating the factor. Since we end up doing the electrophysiology part of an analysis of a mutation like that, we did the electrophysiology for that paper, even though the electrophysiology techniques we do are pretty simple. I keep saying, "Look, anyone could do this." But electrophysiology, I think, seems very intimidating. Anything involving oscilloscopes seems very intimidating to most molecular biologists, so they're happy to let us do it, and that's actually worked out well for us.

Arising from the stuff on ciliogenesis, we've got, from this last recent meeting that I went to, people who've come at centrosome- and centriole-associated genes and proteins have found in a couple of instances that their mutants—some of the viable mutants—tend to have behavioral phenotypes that they have been somewhat puzzled by, and we recognize as probably sensory defects. We've been going after those and trying to, again, do electrophysiology and followed up what are the connections with our genes, the reason being that our sense organs are ciliated, things involved in centrioles and centrosomes often have defects in ciliogenesis.

Then the collaboration on polycystins. This is a case where we are going against our usual strategy, starting with a molecule without a mutation. Another group, one person in particular, Terry Watnick at Johns Hopkins [University], had made the investment in [unclear] flies as substantial, generating a targeted mutation in one of the molecules, come up with the mutation, with a sort of an iffy male-sterile phenotype. They'd wondered about whether they had a sensory defect, sent it to us to do electrophysiology; that was fine. I asked, since we were also interested in spermatogenesis ourselves, I said, "Do you mind if I take a look at spermatogenesis and other reasons for male fertility defects?"

They said, "Sure," and we found out this really interesting defect in sperm function, sperm storage, and that seems to be what polycystins are involved in doing in flies. That, I think, is a— I felt privileged to be allowed to do that experiment, since somebody else had put in all the work on making the mutation, hard work. But I think that's going to work out really well.

We've got other collaborations, again, where other people have made mutations in ion

channels. Mutations which have turned out— We've done the electrophysiology to show that they were deaf. So a lot of them have been spurred by— Yes, we do this little thing that most other labs don't do. We can provide—

VAN BENSCHOTEN: As a service.

KERNAN: As a service, yes.

VAN BENSCHOTEN: Given the limited resources of your lab—for that matter, any lab—and then also, I think, the constraints that sometimes are attached to those resources, what criteria do you use in determining what projects move forward and what projects are shelved or not even begun?

KERNAN: The likelihood of getting to an answer. The standards have changed as we know more. We now need to know more about a gene than we would have a few years ago in order to commit to it, to commit a full, say, positional cloning effort to find an unknown gene. We've got quite a few mutations on the shelf that could well be interesting, but right now we've got too few people working on projects that we're already engaged in, to start another open-ended project with an unknown outcome.

That said, though, we do spread ourselves pretty thin. We've got more projects in the lab going than we have people doing them. In some cases, undergraduates have been very useful as people, low-cost people, at [State University of New York,] Stony Brook. You can get some extremely talented undergraduates. The top end of the Stony Brook undergrad[uate] population is really talented, and very hungry. So an undergrad who generated the data kicked off the polycystin project.

Another one right now is working on a very different side project, a larva which turned up as a mutant larva in the original screen that I did for touch-insensitive mutations. It sensed touch just fine, but all the larva are twisted. You can take the whole larva and give it a quarter turn, and always in the same direction. It was such a strange phenotype and so intriguing, that I just always sort of kept it on the shelf as my spare-time Saturday-afternoon mutant. And it's probably telling us about the mechanism of muscle differentiation. That's quite unrelated to the rest of the lab. For now, as long as we can have an undergrad working on it, it's worth keeping up, but it wouldn't be worth putting the rest of the lab to work on.

VAN BENSCHOTEN: To take it to a much broader level now, talk about the nation's scientific agenda, its criteria for choosing what science has done and what science isn't. Recently, fairly recently, there was a controversy about stem-cell research, as you know. What that pointed up, among other things, was just, you know, who should be at the table to determine

the direction of U.S. [United States] science, where the money should be spent. Among the players are pressure groups, there are policy makers, there are politicians, scientists, all of these people, celebrities in there as well. If you were the head, let's say, of an imaginary commission that signs off on research projects, some of which might be as controversial as, say, stem-cell research, who would you put on that commission, generically? Who do you believe has a right to dictate the nation's research agenda? Maybe not the right, but who should?

KERNAN: It's getting to a level where— I'm not sure. There should be a full spectrum of research going on between completely unfettered investigator initiated— people-following-their-noses—research, the sort of stuff which is, for the most part, what we do, and targeted research. I'm not sure that there's really been a good model yet for targeted research. In some ways I think there's a lot of— I'm wondering if more people my age and at my stage in their scientific career would be willing to—for a short time, maybe, like a sabbatical—take a break from the purely exploratory stuff and be presented with a problem, maybe in a working group, maybe, and say, "Okay, here we've got this—," probably a problem that's just beyond the edge of an obvious solution. And say, "You've got two months to think about this, and a year or two years to have a best shot as possible, devise a solution to it."

Let's see. If I've wandered a little bit from the original point that you were asking for, it's because I don't know the answer. Who should have a role in deciding—

VAN BENSCHOTEN: Another way to look at it is that obviously there are institutions afoot, the NSF [National Science Foundation], the NIH [National Institutes of Health], institutions that set these guidelines that in a direct— You know, have some control over the rudder, let's say, of the ship. So maybe the system isn't broke, too. It more or less functions properly. That's also another possibility.

KERNAN: Right. It's hard to tell. I mean, you'll get a biased answer, because once you're asking it to a scientist in a funded working lab, you're asking somebody for whom the system is working, basically. I'm happy. I get to do what I want. I'm incredibly lucky. I still will get that feeling that I can't believe I get paid for this when I'm in the lab, not when I'm doing all the other stuff.

So the system is providing me with what I need. I don't know if it's providing other people with what they need. Maybe I'm a little more conscious of it now, since the spouse of somebody in the lab has a serious cancer that she is struggling with right now. And it's right on the cusp of how you could see that, for instance, maybe next year the DNA array information for characterizing tumors might actually be useful in determining a treatment strategy for that, and wishing that things had moved a little faster, even though it's still moving incredibly fast. That's one area in which targeted research would seem to be really timely and possibly really useful.

You've got to cast a broad net. I mean, in some ways it's the best thing about coming to the U.S., was that this is one place in which the net can be so broad. I think that as you go up the levels, the responsibility is more to see that every area is covered and that areas don't slip through the cracks just because— That it isn't driven by force of personality of a single investigator or designation of a particular goal as being too broad.

I mean, this last year, you know, one of my colleagues told me that he quickly wrote a terrorism-related grant that he himself had quite a low opinion of, had no expectation of funding at all, had got a relatively moderate score, and was then called up and they said, "We've got lots of money. This is going to get funded." That model is not necessarily good, having the issue of the moment dictate the research.

It's a self-correcting system. People will use suddenly available funds like that and do whatever research they want, pretty much. So I think when you have not a market-driven, but marketplace-of-ideas-driven system, as NIH extramural research is currently, or a lot of it, it has its own inherent stability just because of the sheer numbers of cussedly independent-minded people who are involved in it. That's another ship that's too hard to divert in one direction or another. Overall, you could see the atmosphere in science change when funding— When percentile funding levels go back down to the teens [from more than 20% to below %19], then people get more demoralized and study sections get more demoralized. You get the sense that a lot of good science is not getting done that should be. So as long as the percentile funds are somewhere a little over 20 percent, then the net's being broadly cast.

One thing that I feel right now, is that perhaps the institute organization, National Eye Institute [NEI], hearing [National Institute on Deafness and Other Communicative Disorders [NIDCD], is still a little— Doesn't take enough into account the basic unity of biology, that— For instance, our ciliogenesis mutants. We expect to have defects on hearing, sight, and vision. In each case we're going back to a level that's so broad in evolutionary terms, going back far enough in time, to a last common ancestor that was ancestral to all of these sensory cell types. [To] which institute do you apply? We could apply to any of them, but none is really aware of the deep history of the cell biology of the organism that underlies human biology, and even medicine. But to recognize the overall interest, maybe there should be a, you know, institute for basic cell biology that would be responsible for funding projects like that, so you wouldn't have to have the slightly dishonest feeling of saying that, yes, targeting your very broadly based research to target a specific disease.

VAN BENSCHOTEN: To pick up again on the stem-cell research, just to use it as an example. Part of the problem with the controversy, I think, was sort of the alarmism surrounding it, and people were saying, "Why is there this alarmism?" At some point the scientists hadn't maybe performed their function or performed a better function of getting the word out: why is this important. And that would apply to other things as well, obviously, other types of research. What, to your mind, should the proper role of the average P.I. [principal investigator] be in helping to determine public policy, questions about public policy?

KERNAN: I think it should be for those—among whom I do not include myself—with the gift of speaking clearly in public, that they should be able to give a very clear idea of what we know for sure and what we don't. The stem-cell issue and the whole idea of what constitutes human life and when is it permissible to abort a potential human life or a human life, goes to a definition of what it means to be human. In some ways it's— And the distinction between life and human life is something that science now has something to say, I think. We could be more courageous about no longer ducking behind the idea that there's the religious ethical view of things and the scientific view of things, and these are two different ways of looking at the world that have nothing to say to each other. I don't think that's true anymore.

I think that when we do something like producing the human genome sequence or know intimately the cell-by-cell development of humans, we do know something— There is a piece of knowledge that has moral and ethical implications. It's almost an issue for— Do you define a human by what they are or what they do? In some ways, the idea of what you take as human, what the law and mainstream opinion might take as human, seems to be a more genetical definition than most geneticists would make, and that is [unclear] with the human genome.

Any neurobiologist would say that you've got to have a certain level of nervous system complexity before you can be considered human, and that the degree to which you can make— That scientists are going to be fuzzier in their definitions because they're more aware of complexity than most of the people who are currently making the definitions and making the decisions now.

However, since we live in a democracy, you can't have— Even the best informed opinions cannot dictate the majority decision, nor should they. So you've got to put the information out there that you know is right, with as much force and clarity as you can.

VAN BENSCHOTEN: Does Stony Brook encourage P.I.'s like you and others to take part in these public policy questions, or give you opportunities to do so?

KERNAN: There are public lectures that Stony Brook faculty give on a variety of public policy issues. I haven't yet been asked to give one directly, nor do I expect I would be. If I felt confident enough and that I had enough well thought-through opinions, I'm sure I could get the opportunity to do so.

Stony Brook has just opened a sort of little mini campus, just a small suite of [offices and classrooms] in Manhattan. I've been thinking recently it'd be a good place to maybe— If they were to initiate some sort of policy discussion, I was more thinking along the lines of a discussion of whether the research model for biomedicine is really an engine for making things more expensive, but it might be a good place to initiate something like that. It's slightly peripheral out here on Long Island.

One of the issues that was dealt with recently was whether or not there is an increased risk of breast cancer on Long Island[, New York]. Historically, there's been this long apparently established fact that turns out to be a little fuzzy when you look at it close, that there is a significantly increased risk of breast cancer on Long Island. That's given rise to a lot of— Some celebrity-driven, some increased funding for research both at the university and elsewhere, which on the whole is good, but has also— I know some of our local friends are extremely— It makes them very scared. People for whom, say, breast cancer runs in their family and there are genetic risks rather than environmental risks; they are just nonspecifically scared by that sort of publicity, more than they should be.

VAN BENSCHOTEN: It's been recently stated in an article I read in *The Scientist* that about almost 60, I think about 65 percent of the R & D [research and development] now for research in science comes from private sources, and more and more we see sort of the privatization of scientific research. Do you believe that's a good trend?

KERNAN: No.

VAN BENSCHOTEN: In what way is it not good?

KERNAN: Because I think the business model is not a good model for doing science, at least not all of science. Again, because it's goal-directed, and goals are mistaken. They are short-term and, also, because I think it sucks a lot of the joy out of doing science. You know, it's something that—for not everyone, but for some people— should be something you enjoy doing all the time and not because of the money that you're going to make. People talk about how poorly paid scientists are. In my experience, that hasn't been the case, but I've been lucky. I've made it through the— I'm a moderately well-paid person at a moderately successful institution. I feel incredibly lucky that I get to do what I do. Don't tell my chairman, but I think my salary's pretty good. Can we seal this part of it? [mutual laughter]

VAN BENSCHOTEN: Let me flip it over.

[END OF TAPE 5, SIDE 2]

VAN BENSCHOTEN: All right, this is tape 6, side A.

I had asked about the rise of private labs and research, privately funded research, and you just said that you felt that that was probably not a good trend. Did you want to add anything to that?

KERNAN: Yes, it's a biased view, because I've been in publicly funded labs in public universities all the way along, and none of the labs that I've been in, while I was in them, I don't think were getting a great degree of funding from private sources. Charles [S. Zuker]'s lab is probably now getting more so.

I think the public research enterprise is one of the great things. I sometimes make the analogy with the position of the monasteries in medieval Europe. There, you know, it was seen as a good thing, generally, that these incredibly large, expensive buildings, staffed by thousands of people, were put up and the people were doing what was then viewed as a public function, which was prayer. The equivalent now is research. I think it's more beneficial than prayer and I think the sometimes unspoken compact is, "Pay us enough to live okay. We'll do the research, and our obligation is we tell the truth. We don't fudge the data. We'll be driven either by our curiosity, mostly, but to find out as much as we can about the world." And that's a good thing.

I'd hate to see that compact lost, and maybe that contract that scientists make with the community should be expanded on more, and the idea of what it is to be a scientist. The idea of "We won't fudge the data. We'll tell you the way it is," should be clung to and adhered to more than it is. I think replacement of that by a business model of doing science can only be bad. People know, especially after the last year, that you cannot trust yourself in your own business dealings, even. No one expects a businessman, even an honest businessman, to act in the public interest completely, and certainly less so in the last year. Maybe that's a way in which scientists should communicate with the public: "Look. This is the contract we make with you."

VAN BENSCHOTEN: So, clarify that contract, make it explicit.

KERNAN: Make it explicit. Make it a real contract with America, and transnationally, because the other great thing, I think, about science is that it is transnational. It is a community that goes beyond borders, always has been, and should be so. In time of war, scientists were that cross between countries. Data was exchanged. That's probably gone by the board.

VAN BENSCHOTEN: We'll turn more now to more mundane—I say "more mundane"—more concrete matters, matters closer to home and less, maybe, philosophical. How big is your lab, total, personnel?

KERNAN: Two graduate students, two senior postdoc[toral fellow]s, who now have research scientist appointments, one technician, two undergraduates.

VAN BENSCHOTEN: And what is the racial breakup of that group?

KERNAN: Two people are from Korea— Sorry. Three people from Korea, and even the technician is of Korean descent. We have somehow got this connection with— Originally from the postdoc [Yun Doo Chung] that came here from Seoul National University, so there's— For initially chance, and thereafter, probably somewhat personal connection reasons, that's a big bias in the lab. One of the other postdocs is American, from English-speaking parents. The Koreans are both the graduate students and one of the postdocs. One of the undergraduates is a Muslim name, Fatima Malik, I presume from Muslim parentage; I'm not sure from which country. And the other is American from English-speaking parents.

For graduate programs at public universities, we've had a lot of— We could recruit as many Asian students as we— You could fill up your whole program with Asian students, and it's been limited by the amount of— The need to have training grant fundable American students as well. Maintain diversity, to keep some fraction of American students.

VAN BENSCHOTEN: And what is the breakdown genderwise? How many women, for instance, do you have in the lab?

KERNAN: It fluctuates. In the past, we haven't had all that many graduate students, and the graduate students who have been in the lab so far, have been two female, one male. One of those, one of the women left with a master's degree, so we have the one male student, who will graduate, and one female student, who is new in the lab, and I hope will go all the way to a Ph.D.

Postdocs have been three male, one female, over the history of the lab. Undergraduates have been both biased towards female. Recently, the lab was almost entirely male. Now, with the newer graduate student and new undergrads in the lab, there are more female.

VAN BENSCHOTEN: How about in your department? How many female P.I.'s [principal investigators] are there? You don't have to give me an exact number, but just sort of a ballpark.

KERNAN: In the immediate department, there's two, and in the extended department, it's one or two more. Neurobiology has been seen as one of the departments which have had a problem recruiting and retaining female faculty. The person who was hired just before me, just a few months before me, did not get tenure, and that was a female faculty member. I would, perhaps, plead a little on the department's behalf, that the pool doesn't seem to be all that female-rich, particularly in neurobiology.

VAN BENSCHOTEN: In the immediate department, how many P.I.'s, overall, are there?

KERNAN: Twenty.

VAN BENSCHOTEN: Do you feel that women do science differently than men, in your experience?

KERNAN: Perhaps, but I'm hesitant to put it in those gender terms, because I don't like the idea of constraining people by genotype, and that's a genotype distinction. So, yes, you could caricature a female and a male mode of doing science and probably I would say that in the lab where I did my graduate work, there was, in [University of Wisconsin] Madison, a very— Probably an island of left-wing liberalism in the [University of California at] Berkeley of the Midwest. There were lots of strong and politically aware women, generally, in the university and particularly in our lab. So at times I felt I was almost the lone male among strong-minded women. My political views were appropriately shaped, for the better, probably.

Then when I went to my postdoc lab, the [Charles S.] Zuker lab, I felt like it was almost— You could see the difference in sort of what I call "jock science" mode, more competitive, people more willing to, you know, compete with each other in terms of things like hours spent in the lab and compete with each other to meet with the investigator, the P.I. And there were plenty of men and women in both labs, but I'd say the women were probably more stressed-out in the other environment [the Zuker lab environment].

That said, though, I mean, I've felt like it's not necessarily a helpful way of viewing things. I think you should give people the credit for being independent-minded enough so that rather than saying— Just like you shouldn't say, "You've got this polymorphism in your serotonin transporter, therefore you should be behaving like this," you shouldn't say, "You've got a Y chromosome, therefore you should be behaving like that." So my tendency has been to— Things should be made on a gender-blind basis. If you think that there is a male or female type of performance that might be better or worse, you should discriminate on the basis of the performance, not on the basis of the genotype.

VAN BENSCHOTEN: Well, then, that leads well with the next question. Is science, in your experience, both, you know, at every level, graduate, postdoc, and now as P.I., has it been gender-blind? In other words, is the playing field sort of level between men and women P.I.'s? And graduate students, as well.

KERNAN: I would guess that it's [more] level than in other occupations, but it's still pretty— It can be level, but still pretty bumpy. [mutual laughter] There can be all sorts of local biases, and I would view— I would be skeptical of broad generalizations and focus on the local biases and the local power relations and the local— Within the lab, within the university.

I think that in certain areas here, for instance, in the undergraduate fellowships that my wife [Karen Kernan, née Kwik] helps administer, females are far favored, especially, and minority groups are much favored. I've cooperated with all those various things because, on the whole, the university gets funds and fellowships that it wouldn't get otherwise, but I probably don't agree at base with anything other than a completely gender-blind, color-blind way of making judgments. The judgments should be based on performance, not on preexisting genotype-related things.

VAN BENSCHOTEN: Let's turn to race, questions of race.

KERNAN: We're already there, I guess.

VAN BENSCHOTEN: Right. We're there. But I have to ask this anyway, even if you just say, "Ditto." There are several under-represented groups in science, African Americans, Latinos, just to name two. How specifically might more members from these under-represented groups be brought into science? How can we add more players on this playing field?

KERNAN: In a couple of ways. In the long run, it's got to be— You've got to go right back to early education. Once again, not on the basis of, in that case, genotype, race assignment, which biologically has no meaning. Culturally it does have a meaning, but the cultural distinction of race should not, in my view, determine, be used to make distinctions in government-administered funding. It should be on the basis of performance. If minority groups tend to have fewer resources and fewer means, the distinction should be made on resources and means.

But a lot can be done by disseminating information. I think for many of the under-represented groups, though, it's often, because of the nature of Stony Brook's population, which is recent immigrants and children of immigrants for a large part, people who come in from the city, from Queens and the nearer city suburbs; they may be over-represented in our population.

I think a lot of them come in with the idea of being a doctor as the pinnacle of ambition. Again, the idea of publicizing the scientific life and the scientific contract, the nature of the contract, and the idea of that as being "You can be a success in this way," would be useful. Stony Brook's a big premed school. Particularly in teaching genetics, you see a lot of— All of these people who come in with the ambition of going into medicine, because, in some ways, that's the cultural definition of success. I'd say let's try and broaden those cultural definitions of success a little as well as providing, where possible, the means for people to succeed.

And one of the means to succeed is actually providing a university like Stony Brook, with relatively low tuition. Making it a friendlier place to minorities could possibly help, but not being a member of a—quote—"under-represented group"— unquote—minority, I'm not sure

how to do that. I always felt funny, especially when I was at Madison, where I was, I think, to my knowledge, one of about twenty-five Irish people in town, and yet I was always part of the over-represented majority, whereas there were thousands and thousands of Asian students, for instance, in town. It was the—quote—"Madison had the largest minority representation." It seemed sort of oxymoronic, an oxymoronic definition. I guess there were reasons why.

For me, it goes back, also, to not being a prisoner of history. There are historical reasons why cultural minorities are under-represented, but I don't think that you can go from the general to the particular and make discriminations between people today, in this generation, for historical reasons.

VAN BENSCHOTEN: So, affirmative action is not going to solve this problem then, to your mind?

KERNAN: I don't think so, because I think it focuses the— It tends to focus resources on the privileged, or members of the minority, the people who will collect fellowships, and a very few students will. And it's true that there are under-recognized minorities, who, because of the way that— Anytime you create a minority, you've got to make a definition on paper, and those definitions do not match the real world, so you're going to leave people out that should not be left out.

VAN BENSCHOTEN: But let's say if we accept, then, that, you know, this is a sort of— Race, it's a cultural term. Nevertheless—

KERNAN: And a very U.S. [United States]-specific cultural term.

VAN BENSCHOTEN: Yes, it really is. That's true. But given— Regardless of its artificiality, its cultural foundation, nevertheless, it has real results in the real world.

KERNAN: Oh, I'm not at all quarreling with the reality of it, and in some ways, one of the paradoxes, let's say, in some ways, it's getting easier to change your genotype, your genotypic inheritance than it is your cultural inheritance. Believe me. I'm Irish. I know how difficult it is to change one's cultural baggage. So I don't at all quarrel with the real effects of a cultural concept.

VAN BENSCHOTEN: And, I mean, this is a hard question. People have been wrestling with this for a long time. Affirmative action, I mean, by your reasoning, will not work. Is there anything, then, though, that can be done to overcome these cultural hurdles, I guess, and bring

people again back into science, people who don't have that opportunity now?

KERNAN: You have to be sure that there is— I'd say one place where it would be justifiable to make distinctions is in asking the questions of, you know, is it really true that we have no institutional racism, that people are not favored? But it's very hard to. Most of these look at a pipeline of progression to a career and ask— You know, identify a greater or lower representation of a particular class as you go through that career and ask why. It's so much easier to identify the phenomenon than to figure out the why, because the reasoning can be so personal in each case.

VAN BENSCHOTEN: In one way, it feels a little unfair for me to ask you, a native Irishman, to talk about basically, as you said, a congenital American problem, but—

KERNAN: Well, in some ways the Irish are only free from this problem because they've been insulated from it. They have a long and sorry history of being among the forefront of racists, both when they've come over here, and also right at home. Now in Ireland, people are dealing with this for the first time. For the first time they're getting an influx of Eastern European immigrants, and they've had, for a long time, an underclass that have been labeled. You know, it doesn't take much to physically label a class as distinct, but there are a class of people called "tinkers" or "itinerants" or "traveling people," depending on the scale of political correct nomenclature, that have been discriminated against. So that problem exists there, too.

Where there is a language difference, it's a big issue. And yet science is one area in which you can succeed quite a lot, despite considerable language difficulties. I know, because I've had Korean students and Korean postdocs in my lab, who are doing well despite real observable language difficulties.

I keep coming up against the difficulty of dealing with this in a general concept. I can't and won't make distinctions with as broad a brush as "underrepresented minority." That, for me, has little meaning, even in reality. Even though the concept is real, the problem has to be dealt with in particularities. Which underrepresented minority? Which person? What exactly is the problem for a person? Is it that they don't have enough money? Then let's make sure that people with not enough money, rather than people who are a minority, can get access to funding for fellowships and scholarships for graduate school.

Is it that there is a language problem? Let's provide the classes that you need in order to get up to speed in scientific English.

Is it racism? But that can be as much a feeling of— Coming to a different country is a difficult thing, and for people coming from some minority communities here must be something similar. When you come to a research university, it must be something like coming to a different country. But there's a certain amount of adaptation that has to go on on both sides. It's

undeniable; you have to give up some of your culture when you do that. I had to give up a lot of culture when I came to the U.S., a lot of my identity, and it's always a source of tension. The great benefits of coming here to a country that's big enough and homogeneous enough to mount a huge public research enterprise, which is the reason I'm here; the costs are you're going to lose some of your identity, your cultural identity, and that has to be allowed for. You gain a lot, too. You gain freedom. You gain freedom from your historical constraints.

To go back to that question I asked the class about the polymorphism, should we provide for people on the basis of genotype, I think the conclusion that I ended up making was, you try and maximize access to knowledge for everybody, and identify obstacles that prevent access to knowledge based on nongenotype-related things, but draw the line at making distinctions among people, or preferentially awarding people based on things other than performance.

VAN BENSCHOTEN: An easier question. What is the best part of being a scientist?

KERNAN: You never leave the playground. [mutual laughter] It's an extended period of juvenile delinquency. It's a livelihood, and at the same time, it's exploration. It's finding out. I mean, how few people, the tiny percentage of people in each generation who have been explorers. You get to be an explorer. On the good days, it's finding out something different about the world, having that sort of satisfying curiosity. Curiosity is one of those basic human drives, and satisfying it is always good. Whether the consequences are good or not, the feeling of satisfying it is good.

VAN BENSCHOTEN: How about the other side of the coin? What is the least pleasant part of your job?

KERNAN: Feeling that I'm swamped, that I'm so many degrees away, you know, whether because of nested interruptions or so many tasks removed from actually having my hands in contact with the real world at the bench, is one. I'd say there's a certain frustration that comes again with the tension between family and work, that feeling of never being able to do anything really well, that I used to find satisfaction in, liking for the craft aspects of science means taking the time to do something really well, perhaps even better than it need be done. Also, that's part of being on the cutting edge of science, you know. The first new data is rarely clean. It's usually fuzzy, and it's figuring out the fuzziness, that's part of the puzzle. That's pleasant.

VAN BENSCHOTEN: What were the consequences on you and your lab of the Pew [Scholars Program in the Biomedical Sciences] grant?

KERNAN: For me personally, it was an enormous morale booster. I got it at a stage when I was

still stuck in the positional cloning stages and just beginning to get a break on one of the first genes we cloned. We hadn't really produced anything, and I was not— It was validation for the worth of the work that for me personally, was— Whether it was just around the time that I got the grant, but it was before hearing about that grant and getting it, was one of the things— Before, things looked awfully cloudy; afterwards, they seemed a lot brighter.

You know, in NIH [National Institutes of Health] grant terms, it was sort of a half a grant. That grant-and-a-half level was about the ideal level of funding for the lab, and has been for the past couple of years since we got this polycystin-related grant in the sense that it's taken the place of the amount of funding that the Pew [Scholars Program in the Biomedical Sciences] grant provided, and we're now at the stage, I think, where we could expand to two full NIH grants, if we manage to get it. I'm not sure that we will.

I feel like I haven't given back as much to the Pew [Scholars Program in the Biomedical Sciences], in terms of the scientific productivity that they expected, but I think it's just a slower developing— For me, I'm a slower-producing scientist. It probably takes a year or two longer for the input money to produce the papers than for— And probably because of my character and probably because of the nature of the work that I started. So, in the long run, I feel that if can produce the papers over the next couple of years, that'll actually be— The seed money was the Pew [Scholars Program in the Biomedical Sciences] for at least two of those papers, even though that funding has expired now. Having flexible funding, having funding where you could roll it over and extend it for a year, that also helped.

VAN BENSCHOTEN: We're at the end of the question set, and usually we end with giving you the mic[rophone] and letting you say anything that you wish, anything that you'd like to add that has—

KERNAN: Not that I've found it terribly constraining so far. [laughs]

VAN BENSCHOTEN: Or, you know, maybe clarify anything, too.

KERNAN: I'm just looking back over, kind of, notes and cue words that I'd scribbled down, seeing if I'm— [long pause]

Notes. So this is an example of how I much often find it easier to react to a question than to say something, come up with something independently. Of course, I'm probably struck by the disconnect between maybe what I'm saying and the way other people in the lab would perceive me. Like, my wife was saying, "Are you going to tell them exactly how much of control freak you are?" [mutual laughter] And this hands-off management style.

So, in wondering about this process, you know, I've got a scientist's skepticism that a

one-person report has any objective meaning whatsoever. On the other hand, I was wondering today whether you, as interviewer, are going— It must be strange having each person give what they view as their unique feelings, and I bet you've got people classified into, "Oh, this is a Type 3B. We're going to have another— "

VAN BENSCHOTEN: You know exactly how this is going to go. [mutual laughter]

KERNAN: The whole philosophical issue of human uniqueness that we've talked about and the self is something I still find puzzling. I remember, you know, way back when I was very young, I'd go into, you know, "Here I am. This is me. This is me thinking about being me. This is me thinking about thinking about being me." Going into that infinite, into that regress, and then thinking, "I wonder how many other people think like this?" You know, that idea, which is, you know, common to everyone and yet ultimately unanswerable. I'm sure, to many people I would have appeared, particularly when I was younger, as pretty solipsistic and living in my own little world, and self-contained. But a lot of it was trying to answer that question. "Do other people think the same? How weird am I, anyway?"

VAN BENSCHOTEN: Right. Right. Where do I stand?

KERNAN: Later on, I think, the sense that I expected science and wanted science to provide fame and fortune. One of the ways I expressed it is, "I want to be so famous that people will call me eccentric instead of wacko." And I wonder if science has been wonderful in that way in that, you know, it's given somebody—that's me—who is viewed as pretty socially inept, pretty intellectually able—up to a point, but not all that much—self-contained, given the tools to interact with the world and, ultimately, to interact with other people in a way that's productive and semi-productive, you know, I think is, on the whole, hopefully, in the long run, contributes.

Still, one worries about, as you get to be more secure, and tenured, perhaps, whether this just gives your eccentricities free rein— takes off some of the filter—so that you become less socially rather than more socially adept. But I think, on the whole, it's good not needing to worry about it.

VAN BENSCHOTEN: I remember William Blake saying something about the road to wisdom being excess, or, you know, it was some sense, you know, of being allowed, maybe to follow those whims and eccentricities in some respects.

KERNAN: "Or moderation in all things, especially moderation." [mutual laughter]

I'd say my goals are still as lofty as they were. They weren't always lofty. They weren't

all that lofty to begin with. I don't have grand ambitions. I remember I was asked to— In school, I think one essay was "Who are your heroes?" Well, actually, I don't really have any heroes. I'm too much of an Irishman, or maybe a North Dubliner, to really respect anyone to that extent. You're always skeptical. I was asked the question again by my sister-in-law [Jeanne F. "Gigi" Kwik], who's also been, for a while, interviewing scientists, for unrelated reasons, and I was asked the question, in an e-mail, "Which scientists would you identify and respect as political leaders, who you as a scientist, would respect, and would be able to convey—" this particular message that she was interested in having conveyed. I'm from New York now. I don't respect anyone. Maybe Harold [E.] Varmus, but only because he bikes to work. [mutual laughter]

I think an issue that I'm not sure is related to what we've touched on was—I also think about—scientists as an elite; the extent to which you're allowed to think of yourself as an elite. I was brought up and educated in a high school that was having this conflict, you know. Were they educating the elite? They were educating the elite. There was no question about that, but was that a good or a bad thing? Anytime you make—One of my mottos, or not mottos but mantra, involuntary mantra, has always been "Judge not, lest ye be judged," because I've always been afraid of being judged, feeling inadequate. Yet, particularly since I've become a faculty member, so much of the job is making judgments. You're grading, you're saying, you know, who's in the top level of the class. Who do I want in my lab? Who should get this undergraduate scholarship and who should not get it? Who should get this grant and who should not get it? It's judgments, judgments all the way. Always with imperfect data; always with real consequences.

And I'm fed up with it. I'd much rather not have to grade at all. The graduate genetics course I organize, the students are so much more concerned with the grades in the courses than they should be at a graduate-school level, you know. We should be just able to teach them the knowledge for its own sake at this stage. Anyone in graduate school should be motivated enough that we should not have to grade this course. I would much rather not grade it.

And yet the school requires that graduate students maintain a B average, therefore, that something must come out of it. And, yes, you can make obvious distinctions among— You know, there's a small fraction of students who should not be in graduate school and it's our duty to identify them as early as possible so they don't waste their own time, as well as everyone else's, and get more demoralized as they go on.

So, the necessity of making judgments about people is something that I don't like having to do. I have this, on the one hand, this view of science as something that everyone ought to be able to do, and it would be great if—And part of it is, you know, you don't really understand what science is about until you've done it, to some extent. Until then, a scientist is a cartoon figure in a white coat. Until you get the idea of figuring out about how the world works by experiment, it's hard to grasp that. That may be a limit to the extent to which scientists can educate the public, the extent to which the public don't identify themselves as scientists because they don't see themselves as able to master this arcane of knowledge.

I would rather see science not become professionalized in that way. I guess getting as many undergrads into the lab as possible is one way of doing it. But then, you know, it's obvious when you do science there are some people with an aptitude for it and some not. It's hard to admit that sometimes, that there are people who are never going to be able to do science. There are people who are much better at it than others.

I remember somebody quoting one of Jim [James D.] Watson's quotes to me, that they admired a lot, "It's very important not to hang around with stupid people." But I found that offensive, and still do.

VAN BENSCHOTEN: It sounds elitist.

KERNAN: It is elitist, and there is a truth to it, but it also— You know, my retort was, there are many kinds of stupidity, and uttering that is one of them. [mutual laughter]

VAN BENSCHOTEN: That's good.

KERNAN: But there are also many ways of doing science. I think of my own way of doing science as somewhat atypical. It's a little less analytical, it's more associative, more imaginative in a fuzzy way, maybe, and not in a— I find it hard to master the, say, details of a signal transduction pathway, but do find it helps, and I would like writing out maps and putting things in physical, concrete terms and manipulating them in physical terms.

So I do think there are different ways of doing science and you have to allow for all of them. In some ways, when we were talking about gender in science, I think it's important not to constrain people, identify people and expect them to perform in a certain way because of their overt genotype and overt phenotype, and also to allow for the different ways that science can be done. You know, if somebody finds it hard to produce a restriction map one way, you don't insist that they produce it that way. Maybe there's a different way for them to think about it.

For me, I can take on board and identify words, texts, phrases. In some ways I do my sequence analyses by identifying— You know, even trying to pronounce the way strings of amino acids would feel: "That's DDD, VVV, LLI, that sort of region— Oh, there's another one." That I can do, which is nonsensical in some ways, but it's just a tool, it's a way of grappling with data. People bring their own differences and strengths to doing science, and that ought to be allowed for.

I'm rambling a bit too much here.

Turned forty, so I've started saying to myself, "I don't feel like I can get away with being young anymore." I don't feel like I can— I don't want to hear myself described as "promising"

anymore. It's time to put up or shut up. And things have skated far too long. I've got by too long on that sort of promise that actually hasn't produced. So some productivity is needed over the next few years if I'm to justify my way of doing science.

Still haven't figured out how I got here, whether it was the path of least resistance, whether— I have, at the same time, the feeling that things have been pretty easy for me all along, that I have had these opportunities, that the few big decisions I've had to make did not involve a great deal of sacrifice on my part, and yet I can see more clearly, perhaps even after this process now, that many of the traits and this drive I have, it wasn't just a random process, that I've always had this, by temperament and inclination and curiosity. You can see how it was directed in a sense, even when I wasn't aware of it. In some way, when we have children it becomes clearer. It's fascinating to see your traits, in the same way that you recognize your parents in yourself, recognizing yourself appear in your children in some ways and not in others.

VAN BENSCHOTEN: It touches the question, too, of serendipity, which I didn't ask, which I should have. What role has serendipity played in your career and in your research?

KERNAN: No, you did ask that.

VAN BENSCHOTEN: I did? Okay.

KERNAN: And I answered it in two ways then. One was the concept of anti-serendipity, which I felt at a fairly shallow level, I felt that we hadn't got lucky breaks in our cloning; the other, that I had been very lucky in the people that I'd got into the lab. And lucky chances coming into my life— Well, the fact that Karen [Kernan] 's aunt [Eleanor Kwik Letcher] lived beside my mother's cousin [Patricia Colley Uí Connell] is the reason, in one sense, that I'm married to the person I'm married to now, which, you know, choice being the issue, whether it is choice or not, of a life partner, and somebody you live with is one of those things that one would like to think is choice, but is determined by serendipity.

You can't do the controlled experiment. [mutual laughter] There are no controlled experiments in life. So I don't know how it would have turned out had I gone to the other university. I don't know. I think that deciding which place to go to was something where the choices became a little more stark, but even then, I'm not sure that I would have got tenure at some other universities. I think that choice was well made, not because I don't think the work in the long run isn't justified; the timing wouldn't have been right.

Grant funding. There's quite a lot of serendipity involved in that, and that has very real consequences. The renewal percentile score wasn't all that far above the funding cutoff. It could have gone either way and I think somebody, a friend, just got a second grant proposal, I think, with pretty much the identical percentile score, but the funding level was on the wrong side, so,

probably a better grant than mine, it didn't get funded. There's a lot of serendipity involved there. Or it's the reverse.

[END OF TAPE 6, SIDE 1]

VAN BENSCHOTEN: I'm sorry.

KERNAN: So, being a geneticist, you believe in the laws of probability as started by [Blaise] Pascal, so you know better than to rely on serendipity as a guiding force, and probably I find it easy to imagine disaster, things that might happen. I'm typically rather cautious in most of my life choices, probably somewhat indecisive, take longer than I might in deciding things, so I've found that in order to— When I've had to kick myself, it's been to complete things and to go for choices, move faster, make decisions quicker.

I think I'm pretty much all done. Nothing else is occurring right now.

VAN BENSCHOTEN: Okay. All right.

[END OF TAPE 6, SIDE 2]

[END OF INTERVIEW]

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