CHEMICAL HERITAGE FOUNDATION

ANDREW CAMILLI

The Pew Scholars Program in the Biomedical Sciences

Transcript of an Interview
Conducted by

William Van Benschoten

at

Tufts University Medical School
Boston, Massachusetts

on

7, 8, 10, and 11 October 2002

From the Original Collection of the University of California, Los Angeles

Andrew Camilli

ACKNOWLEDGEMENT

This oral history is part of a series supported by a grant from the Pew Charitable Trusts based on the Pew Scholars Program in the Biomedical Sciences. This collection is an important resource for the history of biomedicine, recording the life and careers of young, distinguished biomedical scientists and of Pew Biomedical Scholar Advisory Committee members.

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UNIVERSITY OF CALIFORNIA, LOS ANGELES

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This Interview Agreement is made and entered into this 5 day _____ of November, 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 ANDREW CAMILLI, having an address at Department of Molecular Biology and Microbiology, Tufts Universitiy, 136 Harrison Avenue, MV 402, Boston, Massachusetts 02111, hereinafter called "Interviewee."

Interviewee agrees to participate in a series of University-conducted tape-recorded interviews, commencing on or about October 7, 2002, and tentatively entitled "Interview with Andrew Camilli. 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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4. Interviewee will receive from University, free of charge, one bound copy of the typewritten manuscript of the interviews.

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

If to University:

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Los Angeles, California 90095-1575

Attention: Janice L. Reiff

If to Interviewee:

Andrew Camilli
Department of Biology and Microbiology
Tufts University
136 Harrison Avenue
Boston MA 02111

University and Interviewee have executed this Agreement on the date first written above.

INTERVIEWEE	THE REGENTS OF THE UNIVERSITY OF CALIFORNIA
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Andrew Camilli (Typed Name)	Janice L. Reiff (Typed Name)
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Tufts University	
Boston MA 02111	
X Date 10/07/02	Date 5 Nov 2002

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Interviewee's Name

August 8, 2022
Date

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
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ANDREW CAMILLI

1963 Born in Lima, Ohio, on 27 September

Education

1987 B.S., Microbiology, University of Michigan, Ann Arbor
1992 Ph.D., Microbiology, University of Pennsylvania

Professional Experience

Harvard Medical School
1992-1995 Postdoctoral Fellow, Damon Runyon-Walter Winchell Cancer Fund

Tufts University Medical School
1995-present Assistant Professor

Honors

1983 William J. Branstrom Award for Academic Achievement, University of Michigan
1983 Michigan Competitive Scholarship, University of Michigan
1987 Graduated with high distinction, University of Michigan
1992-1995 Damon Runyon-Walter Winchell Cancer Fund Postdoctoral Fellowship Award
1997-2001 Pew Scholars Program in the Biomedical Sciences Grant
2002 Eli Lilly and Company Research Award

Selected Publications

Camilli, A., C. R. Paynton, and D. A. Portnoy. 1989. "Intracellular Methicillin Selection of *Listeria monocytogenes* Mutants Unable to Replicate in a Macrophage Cell Line". *Proc. Natl. Acad. Sci. (USA)* 86: 5522-5526.

Camilli, A., D. A. Portnoy, and P. Youngman. 1990. "Insertional Mutagenesis of *Listeria monocytogenes* with a Novel Tn*91 7*-Derivative that Allows Direct Cloning of DNA Flanking Transposon Insertions". *J. Bacteriol.* 172:3738-3744.

Sun, A., A. Camilli, and D. A. Portnoy. 1990. "Isolation of *Listeria monocytogenes* Small-Plaque Mutants Defective for Intracellular Growth and Cell-toCell Spread". *Infect.*

Immun. 58:3770-3778.

Camilli, A., H. Goldfine, and D. A. Portnoy. 1991. "*Listeria monocytogenes* Mutants Lacking Phosphatidylinositol-specific Phospholipase C are Avirulent". *J. Exp. Med.* 173:751-754.

Camilli, A., L.G. Tilney, and D.A. Portnoy. 1993. "Dual Roles of *plcA* in *Listeria monocytogenes* Pathogenesis". *Mol. Microbiol.* 8:143-157.

Brundage, R. A., G. A. Smith, A. Camilli, J. A. Theriot, and D. A. Portnoy. 1993. "Expression and Phosphorylation of the *Listeria monocytogenes* ActA Protein in Mammalian Cells". *Proc. Natl. Acad. Sci. (USA).* 90:11890-11894.

Wanda, S. Y., A. Camilli, H. M. Murchinson, and R. Curtiss III. 1994. "Overproduction of a Dextranase Inhibitor by *Streptococcus sobrinus* Mutants". *J. Bacteriol.* 176:7206-7212.

Sun, J. W., S. Y. Wanda, A. Camilli, and R. Curtiss III. 1994. "Cloning and DNA Sequencing of the Dextranase Inhibitor Gene (*dei)* from *Streptococcus sobrinus". J. Bacteriol.* 176:7213-7222.

Camilli, A., D. T. Beattie, and J. J. Mekalanos. 1994. "Use of Genetic Recombination as a Reporter of Gene Expression". *Proc. Natl. Acad. Sci. (USA)* 91:2634-2638.

Camilli, A., and J. J. Mekalanos. 1995. "Use of Recombinase Gene Fusions to Identify *Vibrio cholerae* Genes Induced During Infection". *Mol. Microbiol.* 18:67 1- 683.

Lee, S. H., M. J. Angelichio, J. J. Mekalanos, and A. Camilli. 1998. "Nucleotide Sequence and Spatiotemporal Expression of the *Vibrio Cholerae vieSAB* Genes during Infection". *J. Bacteriol.* 180:2298-2305.

Martinez de Tejada, G., P. A. Cotter, U. Heininger, A. Camilli, B. J. Akerley, J. J. M. Mekalanos, and J. F. Miller. 1998. "Neither the Bvg$^-$ Phase nor the *vrg6* Locus of *Bordetella pertussis* Is Required for Respiratory Infection in Mice". *Infect. Immun.* 66:2762-2768.

Akerley, B. J., E. J. Rubin, A. Camilli, D. Lampe, H. M. Robertson, and J. J. M. Mekalanos. 1998. "Systematic identification of essential genes by *in vitro mariner* mutagenesis". *Proc. Natl. Acad. Sci. (USA)* 95:8927-8932.

A. L. Bricker and A. Camilli. 1999. "Transformation of a type 4 encapsulated strain of *Streptococcus pneumoniae". FEMS Microb. Lett.* 172:131-135.

Angelichio, M. J., J. Spector, M. K. Waldor, and A. Camilli. 1999. "*Vibrio cholerae* Intestinal Population Dynamics in the Suckling Mouse Infection Model". *Infect. Immun.* 67:3733-3739

MerrellD. S. and A. Camilli. 1999. "The *cadA* gene of *Vibrio cholerae* is Induced During Infection and Plays a Role in Acid Tolerance". *Mol. Microbiol.* 34:836-849.

Lee, S. H., D. L. Hava, M. K. Waldor, and A. Camilli. 1999. "Regulation and Temporal Expression Patterns of *Vibrio cholerae* Virulence Genes During Infection". *Cell* 99:625-634.

Merrell, D. S. and A. Camilli. 2000. "Regulation of *Vibrio cholerae* genes required for Acid Tolerance by a member of the 'ToxR-like' family of Transcriptional Regulators". *J. Bacteriol.* 182:5342-5350.

Golden, N. J., A. Camilli, and D. W. Acheson. 2000. "Random Transposon Mutagenesis of *Campylobacter jejuni". Infect. Immun.* 68:5450-5453.

Merrell, D. S., A. D. Tischler S. H. Lee, and A. Camilli. 2000. "*Vibrio cholerae* Requires *rpoS* for Efficient Intestinal Colonization". *Infect. Immun.* 68:6691-6696.

Lee, S. H., S. M. Butler, and A. Camilli. 2001. “Selection of *In Vivo* Regulators of Bacterial Virulence”. *Proc. Natl. Acad. Sci. (USA)* 98:6889-6894.

HavaD. L. and A. Camilli. 2001. “Isolation and Characterization of a Temperature-sensitive Generalized Transducing Bacteriophage for *Vibrio cholerae*”. *J. Microbiol. Methods* 46:217-225.

Merrell, D. S., C. Bailey, J. B. Kaper, and A. Camilli. 2001. “The ToxR-mediated Organic Acid Tolerance Response of *Vibrio cholerae* requires OmpU”. *J. Bacteriol.* 183:2746-54.

Li, C. C., D. S. Merrell, A. Camilli, and J. B. Kaper. 2002. “The Contribution of CRP-mediated Repression to the Modulation of OmpT Expression in *Vibrio cholerae”. Mol. Microbiol.* 43:1577-89.

Merrell D. S., S. M. Butler, F. Qadri, N. A. Dolganov, A. Alam, M. B. Cohen, S. B. Calderwood, G. K. Schoolnik, and A. Camilli . 2002. “Host-induced epidemic spread of the cholera bacterium”. *Nature* 417:642-645.

MerrellD. S., D. L. Hava, and A. Camilli. 2002. “Identification of Novel Factors Involved in Colonization and Acid Tolerance of *Vibrio cholerae*”. *Mol. Microbiol.* 43:1471-91

Tischler, A. D., S. H. Lee, and A. Camilli. 2002. “The Vibrio cholerae vieSAB Locus Encodes a Pathway Contributing to Cholera Toxin Production”. *J. Bacteriol.* 184:4104-13.

Hava, D. L. and A. Camilli. 2002. “Large-scale Identification of Serotype 4 *Streptococcus pneumoniae* Virulence Factors”. *Mol. Microbiol.* (in press).

ABSTRACT

Andrew Camilli was born in Lima, Ohio, a factory town, the fifth of seven children. After Andrew's birth, the family moved to Flint, Michigan, where his father worked as a banker. Camilli's parents were both quite influential in his intellectual development, both being proponents of obtaining a good education. His father's broad interests also introduced Andrew to science at a young age. He attended a public grammar school and then a parochial high school while in Flint, reading about science, being interested in and playing sports (though not for high school teams).

He enrolled at the University of Michigan, Flint, intending to pursue a degree in computer science, but after taking a human genetics course, he decided he wanted a career in biological research. Soon he transferred to the University of Michigan, Ann Arbor, to study biology and medical microbiology. While an undergraduate he had the opportunity to work in Robert B. Helling's and Julian Adams's laboratories, as well an opportunity to intern during his summers at Upjohn (about which he addressed the pros and cons of being in industry).

After graduation, Camilli matriculated at Washington University in St. Louis, rotating through Daniel A. Portnoy's, William L. Goldwin's, and Roy Curtiss III's laboratories. When Portnoy left for the University of Pennsylvania, Camilli followed in order to complete his doctoral work on the genes for virulence factors in *Listeria monocytogenes*. He undertook a postdoctoral fellowship in John J. Mekalanos's lab at Harvard University focusing on a recombinase reporter system for genetic expression before accepting a faculty position at the Tufts University School of Medicine.

After setting up his own laboratory Camilli received the Pew Scholars Program in the Biomedical Sciences award providing him with funding to explore important and interesting directions in his research. Genes remained the central aspect of his science, so he focused his lab on genetic expression in *Vibrio cholerae* and gene regulation in *Streptococcus pneumoniae*, which, he acknowledges, has practical applications to understandings of health and disease.

The interview concludes with Camilli's reflections on various topics related to his science, his life, and his career. He discusses the ways in which his role in the laboratory has changed over time, his teaching responsibilities, his management style, especially as it relates to his mentors' styles, and balancing his career with his family. He ends with his thoughts on competition in science; the national research agenda; collaboration; and, of course, what he enjoys most about being a scientist.

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: Camilli's office, Tufts University.

Dates, length of sessions: October 7, 2002; October 8, 2002; October 10, 2002; and October 11, 2002.

Total number of recorded hours: 7.0

Persons present during interview: Camilli 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 Camilli to obtain written background information (curriculum vitae, copies of published articles, etc.) and agree on an interviewing schedule. He also reviewed prior Pew scholars's interviews and the documentation in Camilli'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.

Camilli reviewed the transcript. He verified proper names and made a number of corrections and additions.

Carol Squires prepared the table of contents. William Van Benschoten, senior writer,

assembled the biographical summary and interview history, and compiled the index.

TABLE OF CONTENTS

INTERVIEWEE: **Andrew Camilli**

INTERVIEWER: **William Van Benschoten**

LOCATION: **Tufts University Medical School**
Boston, Massachusetts

DATE: **7 October 2002**

VAN BENSCHOTEN: Tape one, side A. I'm with Andrew Camilli, and today is October 6th , 2002.

CAMILLI: I think it's the seventh.

VAN BENSCHOTEN: Seventh?

CAMILLI: Yes.

VAN BENSCHOTEN: Okay. I've got to get my watch fixed here. [laughs]

CAMILLI: My kindergartener told me it was the seventh.

VAN BENSCHOTEN: All right. So, October 7th, 2002. We're in his office.

I want to thank you for sitting down with us, first of all, and letting us talk about your work and your life. I'm going to start with something fairly basic. What is your full name, and when were you born?

CAMILLI: My full name is Andrew Camilli. I have no middle name. I'm one of seven, and my parents [John P. and Maureen T. Camilli] started running out of names, apparently. I was born September 27th , 1963, in Ohio. So I'm a Buckeye.

VAN BENSCHOTEN: In what part of Ohio?

CAMILLI: I was born in Lima, which is a small town, kind of central north part, and is only known for a couple of large factories. They made tanks during World War II, so if anyone's heard of Lima it's, "Oh yeah, they made tanks there."

VAN BENSCHOTEN: And did you spend most of your childhood in Lima?

CAMILLI: No. We moved— My family moved about two months after I was born. My father was a banker. At that point we had five children. There were two more to come, for a total of seven, so I was the fifth born. So we moved to Flint, Michigan, from there, and that's where I grew up, until I went away to college.

VAN BENSCHOTEN: And when did you move? When was that move? How old were you?

CAMILLI: So, I was two months old when we moved. So I basically grew up my entire life in Flint, Michigan, which is an auto town, automotive town, and my father was a banker.

VAN BENSCHOTEN: Have you seen the documentary Roger and Me?

CAMILLI: Oh, yeah. I tell people that my mother was the rabbit lady.

VAN BENSCHOTEN: Oh, really?

CAMILLI: But it's just a joke.

VAN BENSCHOTEN: Okay. [Laughs] I was about to say, because of that documentary, that part of it has stuck in my mind the most, unfortunately.

CAMILLI: Yes, it was a little— He [Michael Moore] overdid it a little bit, but it definitely was a depressed economy there for decades, and to this day Flint is really missing something major, and that is, the economy there is just awful. Also in its history, it's won, at least once, maybe twice, worst city to live in in the United States, because of bad economy, crime. So it wasn't the most— I was definitely ready to get out of there and go away when I went to college. But that's home.

VAN BENSCHOTEN: Now, since the documentary, I mean, it's been quite a while, right, since that scale-down, that strike and the rest of it. Has the town lost quite a lot of people in the last fifteen to twenty years?

CAMILLI: It's kind of maintained its overall number, but other— There has been more growth in the suburbs, and other towns in Michigan, whereas Flint has kind of held steady. I mean, there are a couple of good things that are going on there, like the University of Michigan has— The main campus is in Ann Arbor, which is where I went and did my undergraduate work, but there are also a couple of other campuses, and there's one in Flint that's just been booming. It's been growing, and is a very nice campus for the University of Michigan. Actually, I did my first two years of undergrad there, and then when I decided to become a microbiologist, I knew I had to go to Ann Arbor at that point, because that's where all the research was being done.

VAN BENSCHOTEN: Let's talk a little bit about your family. If you could, maybe start with your grandparents.

CAMILLI: Okay. So, on my father's side, my grandfather, Peter [Camilli], is Italian. He came over in the early part of the 1900s. There's an interesting story there. He came over with his brother, and the brother hated it and went back. He stayed and then World War I hit, and they got separated. The other brother died soon thereafter back in Italy, unbeknownst to Peter, and then a few years into the twenties, Peter died, and we totally lost contact—names, addresses, nothing—we had nothing of who our Italian relatives were, so we totally lost that side of the family.

Then my grandmother [Mary Camilli], who married Peter, was Slovakian, so both immigrants, and she— I only have faint recollections of because she died when I was pretty young, too. So I never met my grandfather, of course, and my grandmother, I have some faint memories of her.

My mother was adopted, so the biological lineage ends right there. Her adoptive parents, the father, my grandfather [Gerard Reynolds] I never met. He died when my mother was very young, so the men, apparently, don't live long in this family. Then her adoptive mother [Helen Reynolds] just passed away maybe a decade ago, so she lived into her nineties.

VAN BENSCHOTEN: So you knew her best, then?

CAMILLI: I knew her best, yes.

VAN BENSCHOTEN: What was her name?

CAMILLI: Her name was Reynolds. I'm blanking out on her first name right now, but, you know, Grandma Reynolds, staunch Republican.

VAN BENSCHOTEN: What do you remember of her, other than her Republicanism?

CAMILLI: Well, I remember a couple of incidents. Like she was very protective of her— She had a couple of, I wouldn't call them heirlooms, but things that she really loved, including her lamps, and I remember breaking one of her lamps once. So I remember that incident. Some of the fonder memories are, she would always make us these hot dogs where you'd bake them inside the doughy bread.

VAN BENSCHOTEN: I remember those.

CAMILLI: When I think of Grandma Reynolds, I can smell that smell.

VAN BENSCHOTEN: That's funny.

CAMILLI: Dogs in a blanket, or I forget what she called those things.

VAN BENSCHOTEN: Oh, yeah. Pigs in a blanket.

CAMILLI: Pigs in a blanket. So she made an impression on me that way. The other thing is her apartment— There was a commuter rail maybe fifty yards behind her house, so I also remember her house, her apartment and her yard very well, because we'd go back there and play on the train tracks, against the adults' advice. So those are some of the things I remember. She was a very nice lady.

My older brother, Gerry [Gerard Camilli], he's the oldest in the family, was a staunch Democrat, growing up. The whole family is very— My parents and family are pretty much Democratic, very liberal, and I just remember my older brother, I wouldn't say taunting my grandmother, but he would say a few things just to get her ire up, and I just remember the arguments, the political arguments, because she had a big painting of Richard [M.] Nixon up there with her. I mean, that was just too much. My older brother understood a little more, and he's very political, so I remember those arguments. I was too young to really understand anything.

So, the way I know her is every summer we would drive from Michigan to New Jersey, which is where both my parents are from, Summit and New Providence, New Jersey. We would all pile into one station wagon, seven kids, two adults, and drive there, and that's when we would see the relatives there. We could only do that for a few years before we got too big to fit in the station wagon.

So the last time we went, I think I was maybe six or seven, and then that was it. Then I saw my grandmother maybe twice until she died. This is more than ten years ago, actually, maybe fifteen years ago that she passed away. In her last decade or so, she mentally was really not— Had no mental facilities whatsoever, so she didn't really know who you were. So we didn't visit her; the kids didn't visit her. My mother, obviously, visited her a lot. So, those are the grandparents.

VAN BENSCHOTEN: How did your grandmother take Watergate? It must have been a difficult period for her.

CAMILLI: Yes. So, post-Watergate was— Let me think. So this was post-Watergate that she still had that Nixon painting up.

VAN BENSCHOTEN: Oh, really.

CAMILLI: Yes. Still adored the man.

VAN BENSCHOTEN: A diehard.

CAMILLI: Yes. So, I don't know. I never heard arguments about that, like would she defend him or what.

VAN BENSCHOTEN: How about your mother and father?

CAMILLI: So, my father was, you know, the child of two immigrant parents, and he's also an identical twin [of Leonard Camilli]. He has an older sister, my aunt [Mary Ann Camilli], and a younger brother, my uncle [Philip Camilli], and so they're— My grandfather died when he was four, and so those kids were just devastated by that. It was the [Great] Depression. It was the end of the depression.

He [Camilli's grandfather] had a gas station, and they were doing all right, but when he died, the gas station— I forget what happened to the gas station, but basically they, at that point, became poor. He must not have had any insurance or anything. So I think losing— My father losing his own father when he was that young, and then growing up in the depression where his mother had to work all the time, was very hard on those kids.

So they became—and this is kind of, I guess, quite often happens—I'd say became overachievers. Especially my father and his twin brother really became overachievers. I mean, they were blessed with— Everyone in that family is, I would say, very smart, intellectuals, especially my father and his brother. So, intensive curiosity, always reading books, very smart, very athletic, just like super— From what I know of my dad, and from what I've heard from his siblings, etc., he was just like Mr. Everything, he and his brother. Straight As, scholarships, but overcompetitive, to this day.

VAN BENSCHOTEN: So he showed, then, his overachievement in terms of education and sports, it seems. Other ways?

CAMILLI: Yes. But I don't want to make him out to be, you know, Type A, and was a bad father. He had many talents, but his number one, I think, enjoyment in life is learning, continually learning, and being knowledgeable about a lot of different things. And he's driven by genuine interest, mostly, but there's something about his childhood and, I don't know, the pressures that he was under, that he's really an overachiever, and so that takes up— You know, growing up, it's hard to see your parents for anything other than the parents that you love without question, but now, after having a few arguments with him as an adult, suddenly you start to see the man, the person. That's true for my mother and my father.

So now I see him— He sometimes can be obnoxious because he's so knowledgeable about so many different things that you can just bring up some topic and off he goes. And sometimes it's almost like a passive aggression, his talking about things and overtaking conversations, but that's just how he is. He's just, you know, he's a brilliant person.

Another thing that always has had an impact on me, and probably has an impact on my career choices, is he always loved the sciences, but was a banker and hated it. He's, I would say, a staunch [Roman] Catholic, and as he got older, started looking back on his life and his career, and what his career basically boiled down to— he was a trust officer at a bank—what his career basically boiled down to was keeping rich people rich and making them richer, and then getting the fees for the bank. So for someone with so many outside interests, and someone who is very religious, when you turn forty and fifty and look back on your career, it's not very satisfying.

So he was always pushing us kids to develop our interests, and really, I wouldn't say steered us away from the business world, but anything, you know, like the sciences or athletics or things that he enjoyed most, he kind of pushed us toward. In a way he's largely responsible for my being a scientist, I would say, because growing up— The other thing about my father

that I always remember fondly is he would take us— There was a period there when maybe between the ages of ten and fifteen—I forget exactly when—on many weekends, not every weekend, but most of the weekends, we would go daily— Our Sunday thing to do is we'd go to church, then we'd go to the library, all of us kids running around the public library.

I spent a lot of time with him there where we'd go through stacks and he'd start telling me about different books and he'd suggest books for me to read, and so we had a real good rapport there, a real good teacher-student thing going, where he was, in a way, my librarian. He, as I said, was really into the sciences, all kinds of different sciences, and he would read a book on, say, astronomy, and then the next weekend he'd give the book to me. So I'd work my way through it as best I could, and then we'd talk. And we'd have these great conversations, talking about, you know, what the science was about, and I would just— You know, it was my dad, so I was just awed by spending quality time with him like that, which when you're one of seven is very important, but just, I remember, early on in my life developed this interest in the sciences. I just loved the stuff that we talked about.

VAN BENSCHOTEN: Is there any particular topic, or book, or subject that you remember?

CAMILLI: You know, oddly, it was really physics and astronomy. Those are the books I remember and the conversations I remember that fascinated me most, and to this day I immediately turn when there's some special on TV on the Hubble [Space] Telescope or I'm reading my Science or Nature magazine, and I spend most of my time skimming the latest in these other areas, as well as my own.

Reading about my own field is more work, because I know I have to read it. The other stuff is just enjoyment. So, early in my life it was other disciplines that fascinated me, but it was quite varied. Like we would go, many times would go rock hunting, so I learned a little bit about geology and fossils. We never had a telescope, but many of the books we read and talked about were things having to do with space and astronomy, origins of the universe and things like that.

Then as I got older, that was the obvious area of science to talk about in relation to some of the big questions about, you know, who are we, why are we here, and trying to make connections with my father's religion, which I don't share, but maybe that's something not appropriate for the tapes. So as I got older, that was always good fodder for conversations and arguments, was the religion-science dilemma, in my mind. There was never a dilemma in his mind.

VAN BENSCHOTEN: This is an interesting subject, because very often in these interviews we've carried out, we've dealt with this topic, and it's interesting, the sort of spectrum, different opinions, that you have on the thing. We won't get into the sort of knock-down drag-out fights, maybe, but first of all, what is his name?

CAMILLI: So, my father's name is John [P.] Camilli, and my mother is Maureen.

VAN BENSCHOTEN: And what religion was he a member of?

CAMILLI: So, they were both Roman Catholic, born and raised.

VAN BENSCHOTEN: Okay. I'm sorry, yes, you mentioned that.

CAMILLI: And we were all raised Roman Catholic, too. Went to church every Sunday and had our religion classes, but, you know, as each of us entered our teen years, we drifted away from the church, invariably, all seven of us kids.

VAN BENSCHOTEN: So he felt science and religion were completely compatible, I assume.

CAMILLI: Yep.

VAN BENSCHOTEN: What is your own view now?

CAMILLI: My view for a while was that they are incompatible, and that religion, Catholicism, is really totally faith based, and you either— There's no evidence, and there doesn't need to be evidence, as a good Catholic will argue. So for me, there's always been a disconnect. There's no evidence, so it's just— So the way I've seen it, and the way I see it now, is that religion is a creation of mankind.

I'm not saying that religions are a bad thing. I think they're a good thing. That's why they developed and then engrained in our species. Religions are apparently good for, you know, reproductive success. In the overall scheme of things, it must be good for societies and reproductive success of societies. And one can see why, when you think about religions. So I don't have a bad view of religions. It's just my own personal beliefs. There's no evidence; therefore I take it all with a grain of salt. So I see it as a creation of humans, for humans to use, and that's how I see it.

As far as science and religion not— My viewpoint now is that science doesn't need to be meshed with religion in any way, and vice versa. Religion doesn't need science to prove it. Science, in my own mind, religion is— I've put it off. It's totally faith based. I wouldn't even waste my time now thinking about what's the scientific evidence against religions—of course,

there's none—or for it; there's none.

VAN BENSCHOTEN: So, two separate realms?

CAMILLI: Yes, two separate realms.

VAN BENSCHOTEN: Do you believe, therefore, that science is not man made, is not a sort of cultural product?

CAMILLI: Science is something that's done by humans, but it's not— It's a natural outcome, I think, of any intelligent species. Science is a way to learn about the physical world through experimentation, and it's fact now that science is a good method for learning about the physical world and the biological world. And so far, we've yet to be thrown a curveball by nature. Science ultimately can always figure things out, things that have to do with our universe.

For example, now we're learning about the first few microseconds of the beginning of the universe. I mean, it's just incredible what science can learn about. So, is it a creation of humans? Yes, I mean, humans came up with science, but science, in a way, is like a natural outcome. Any intelligent species in this universe is going to use science to learn about the physical world.

VAN BENSCHOTEN: So it's a universal tool.

CAMILLI: Yes. It's like math. Math is just inherent, an inherent property of our universe. So I don't think it's a creation that's going to come and go, or going to have different flavors. It's just part of our universe.

VAN BENSCHOTEN: You might have mentioned this already, but I'll ask it. What was your father's education?

CAMILLI: He was well educated. He started college at Rutgers [University]. So, you know, he went through school with pretty much straight As. He and his twin brother went to Rutgers, and then they were both seventeen at that point, and enlisted in the air force. This was toward the end of the war [World War II], so they both went and got trained.

My father was trained in how to drop bombs. He would be the guy in the airplane who uses the charts and says exactly when to let go, when to release. He was trained to do that. He

was then being shipped across the country to go fight in the war with Japan, when the A bombs were dropped. So he spent a few months at a military field in Denver. I forget what that one's called [Buckley Field], but he was there for a couple of months, and then that was it. Then he was honorably discharged and went back to college. He finished college with funds from the government. I forget exactly what he got.

VAN BENSCHOTEN: Oh, the G.I. Bill.

CAMILLI: Yes, G.I. Bill. And finished up at Princeton [University], actually. He was an English major, and then he took some classes toward— Maybe he had a minor in some business thing; I forget exactly. But he ended up being a banker, at that point. But he was an English major. I think his brother might have been an English major as well. Yes, so he did, I think, two years at Rutgers and then two years at Princeton.

One of my favorite stories from him is he, one early morning at Princeton, saw Albert Einstein walking down the street with his dog, and he said across the street, "Good morning, Dr. Einstein."

Doctor Einstein said, "Good morning, young man," and kept going. So I just feel that's like— It's funny, because growing up as a kid, that's like, you know, he met my equivalent of a god. But Einstein, of course, was just a man, a very smart, creative man with his own personal problems.

But it's funny, because a few weeks, maybe two months ago, I was at a meeting at Cold Spring Harbor [Laboratory] and met Jim [James D.] Watson, and maybe twenty years from now my own children will think that I met, you know, the equivalent of a god, even though he's just a person.

VAN BENSCHOTEN: They'll have to keep that tradition going, the next god out there somewhere.

CAMILLI: Yes. The right place at the right time.

VAN BENSCHOTEN: Right. You said that he was very much interested in the sciences, and found himself a banker. Did he ever express regret about being a banker?

CAMILLI: Oh, yes, many times. Many times. And as I entered science, he gave me lots of encouragement, and occasionally would let slip his, "Boy, I wished I had done this." As I got into, for example, microbiology, he would, of course, shift his reading to try and learn some

stuff so we could have conversations. So he's fascinated by many different things. He also loves history, but science, I think, is special for him.

But now it's gotten to the point where he— He's in his seventies, and he's slowing down a bit, and he doesn't really try to keep up with my stuff anymore. I sent him the first few of my papers when I was a grad student and he tried to read them, and now he's pretty much given up. I brought him to a meeting and he heard me speak about science for the first time this summer, at the ASM meeting, American Society for Microbiology. I won an award and gave a big lecture there, and my dad was right there in the front row.

VAN BENSCHOTEN: That's great.

CAMILLI: He had a great time. What did he say? Speaking of proof of things, after my talk he said, "Andy, I knew you were a scientist, but today you actually proved it to me. I now have evidence that you're big stuff." Because he had never heard me talk before, so I think he was actually— I actually can give a pretty decent seminar on my work, and he was very impressed. I think he knew that I could do benchwork and run a lab, and then do good work, but to also be able to get up and present it well was, I think, an added bonus for him. I don't think he had any idea that I could get up and give a talk. I remember, he asked, "You must have had training, how to give a talk."

And I said, "Well, yeah, but jumping-into-the-water training. You just start giving talks. You're forced to give talks as graduate students, and Dad, I've given sixty talks by now. That's how I've learned. I didn't take any speech classes or anything like that."

So anyway, that was a real thrill for him and me, to attend a meeting and hear me speak this past summer.

VAN BENSCHOTEN: He had to be impressed, too, that recently you've won the American Society of Microbiology, the Eli Lilly Awards.

CAMILLI: Yes. That was the award talk he was at. So he got to meet a lot of my peers and the chair of our department, so he had a great time.

VAN BENSCHOTEN: Let's turn to your mother, and you've mentioned her. Tell us a little bit about her life.

CAMILLI: So, she was one of five children, all adopted. I think her father was a very difficult man. I think there were a lot of problems growing up. He died when she— I forget exactly how

old she was. She was maybe, I don't know, ten or eleven. She was still quite young when he died, and really did not— You know, I would say she has really had bad relationships with her siblings [Kevin Reynolds, Marsha McMurray, Robert Reynolds, Mary-Sheila Reynolds]. She was close to one, actually close to two of her siblings, one brother [Kevin Reynolds] and one sister [Marsha McMurray]. But the others [in her family], really problem relationships with them. Her family life was not the greatest, to put it mildly, so she, as a result, has some problems, you know, interacting with people.

So my mother is very practical. She's the opposite end of the spectrum as my father. My father's always incredibly intellectual and very bad at dealing with reality. My mother is wonderful at dealing with reality, but could care less about what's happening on some neutron star out there in the galaxy.

So my mother's very practical, has— She's actually epileptic, so I remember when I was maybe, you know, under ten—I forget how old I was—I remember we walked home from school one day, entered the house, and I have this vision just burned into my memory. There is my mother slumped over the phone, drool coming out. She had had a seizure, a grand mal seizure, and was unconscious, and that was the first seizure she had, the first grand mal seizure. There are these things called petit mal seizures, which are more of a— It's not a real seizure; it's more of you just lose— It's like a dizzy spell, a severe dizzy spell. And a grand mal is a seizure, you know, the shaking and everything.

So we came home from school and there's my mother slumped over the phone, drool and some blood coming out. So we ran to the neighbor's. She [my mother] was diagnosed with epilepsy. So then from that point on, she was on Dilantin, which I think still might be used. One of the things about my mother is, Dilantin is like— I don't know if you've ever had Benadryl. It knocks you out. It does its thing, whatever it's supposed to do. It definitely is good at stopping seizures, preventing seizures, but you're constantly drowsy, and so that was my mother, growing up, sleeping ungodly hours. She would go to bed at ten and wake up at eleven the next morning.

VAN BENSCHOTEN: Wow.

CAMILLI: By that point the older kids were already in their teens, and we were a self-sufficient bunch. But growing up, you know, she took care of the important things, but a lot of the little things we did ourselves. Like I would always make my own breakfast and lunch, as far back as I can remember. I would make my own breakfast and lunch, sometimes dinner. And our clothes— In my bedroom I'd have this huge pile of clothes, and if I wanted something clean to wear, I'd have to go wash my own laundry. That's how my childhood was, which, you know, when you grow up with this you don't think this is— You think this is normal. So then when I started going to other kids' houses, as I got into the upper years of elementary school or whatever, I started to realize that it's quite different in our house.

VAN BENSCHOTEN: So part of, then, your mother's condition, was to make the children more self-reliant, it seems, then.

CAMILLI: Oh yes. We were definitely self-reliant. You know, kids, you— I guess many people have kind of a "the grass is greener on the other side" response. So to this day now, I am like super-anal-retentive about things being clean and in order. So, that's something I got from my mother, and I thank her for it. So, my mother— She was very practical. She had a few passions. Like one of her passions was raising and showing collies, so she's like one of the best in the country. She has champion dogs and just loves that. That's been her life. As the kids in the family got older and went away to college or whatever, we were replaced by dogs. [mutual laughter]

VAN BENSCHOTEN: I've been replaced by a cat, by the way, so I understand your situation.

CAMILLI: She would travel all around, showing these dogs, so that's been her passion. And she's worked off and on, here and there. Like right now she's doing— My father's retired, and one of their new things they like to do is travel to Europe, but they don't really have much money, so she works part-time and earns that money just so they can do these trips every other year.

So my mother is just very— She can do what has to be done, like there's nothing that could stop her; very practical, can work very hard. But growing up, she was not— You know, I always thought my parents bit off a little more than they could chew, having seven kids. That's the way I feel about it, because my father, like I say, was not very good with practical stuff, and he worked a lot. He worked very long hours as a banker, and my mother, with her drowsiness— You know, I used to think of it as laziness back then, but really, it's not. She had physical impairments that kept her from being, you know, cheery and diligent about things.

VAN BENSCHOTEN: What kind of education did she have?

CAMILLI: So, she had one year of undergraduate, I think was going to be a nurse, and then married my father. I guess a lot of women did that back then. And then immediately started having children, so, did that for quite a number of years. Seven children in nine years, I think.

VAN BENSCHOTEN: Before we talk a little bit, too, about your siblings [Gerard, John Michael , Helen, Mary, Monica, and Peter Camilli], what traits do you believe that you inherited from your mom? What part of her do you see yourself as having received?

CAMILLI: Well, so, physical and mental, I got a lot from my mother. So, on the physical side I kind of have some of the features of my mother, like blue eyes. My mother, I told you, was adopted, but she looks kind of Irish-English. She's somewhere from the U.K. [United Kingdom], probably. We've never made the connect. I mean, my older brother's done some genealogy, but he's never dug there. So I've gotten some physical things from my mother, like my eyes and my light-colored hair. If I let my facial hair grow out, it takes on kind of a reddish tinge. It's very ugly.

I also have my mother's immune system. I told you she raises and shows collies, but she's— I'm allergic to dogs and cats, especially cats, and she's allergic to them, too. And two of my siblings are allergic to animals, too. I mean, it's really broad. Anything with fur I'm allergic to—rabbits, mice. If I do research that involves animals, I have to do it in a fume hood so I don't breathe it in, because I just get miserable. So my mother's allergic to these things, too, but she still loves the animals so much, but she's exposed to them so much that she's, in a way, tolerized.

But that's another impression. I'll tell you more about what I got from my mother, but my allergies were a big part of my childhood, because we had cats, growing up, and then later on, dogs. I was allergic as hell to these things and never knew it. So, growing up with cats I was— You know, I was sick a lot. I'd always have the sniffles, always have a sore throat, and I didn't know it until I went away to college. I went away to college [University of Michigan, Flint and Ann Arbor], and maybe two or three months went by, and then I went home for Christmas or whatever and just had the worst allergy attacks, to the animals in the house, and I put two and two together. I'm allergic to these things. So I attribute a lot of my childhood— You know, I wasn't deathly ill or anything, but I just remember always having some sniffles and being miserable with sneezing and whatever, and it was the damn cats and dogs, which I loved. I loved those animals, but at the same time was allergic to them. So I got that from my mother, too, because she's allergic.

Mentally, it's hard to distinguish what one gets genetically from the raising by the parents, but one thing I definitely got from my mother was practicality. My wife's [Kristen M. Camilli, née Auchter] very practical, too, but when it comes to being very practical in, say, an emergency situation, it's always me. I immediately know what to do. A tooth gets knocked loose. I'm the one that is calm and collected, and I actually put one back in. Well, it was actually knocked loose. My five-year-old [Adam O. Camilli]'s [tooth] knocked loose and was sticking out. We were on the Cape [Cape Cod], so we could not go to the dentist. It was the weekend, and my wife was just frantic, you know, calling the dentist at home, etc., and I just took him in the bathroom and straightened it out. Didn't hurt him at all. So I'm very— I've the practicality thing from my mother.

I would say she appreciates the biological world, very much so. With the raising of the collies, she does everything. She is there when the dogs have their litter, and she gives the animals their shots, and if one's born with some tragic defect, she'll put the animal down. She does everything. So she's like a nurse now, but for dogs. So she's always appreciated the

biology and knows a little bit about genetics because that's one of the problems with showing dogs; these dogs are inbred. But anyway, so I kind of got her admiration and love, I think, of the natural world, the biology.

VAN BENSCHOTEN: How about your father? You've already mentioned sort of his intellectual curiosity, so I would assume that you probably think you got some of that from him.

CAMILLI: I got that from him, and, you know, it was developed through my interactions with him, but also, I'm sure, some of it comes naturally, genetically. But I've always felt, and still feel, that I'm only a tenth the intellectual as my dad, and I don't know what it is. Maybe I just didn't get— You know, I just don't have the same brain structure as him. But he is such an avid reader, and his retention and understanding of things is just incredible. So I've always been jealous of that, so I feel inferior intellectually, compared to my dad, in an admirable and kind of a jealous way; I don't think bad of it, and I don't think bad of myself. I think I'm probably above average intelligence. I'm no genius. So I did get some intellectual curiosity from my dad, definitely, and some level of intelligence I got from him. Turns out the way I talk and converse, I got a little bit from him.

One thing I did not get from him—and I don't know where I got it from—is I was quite shy, growing up. That was probably more, I don't know, sibling rivalry, problems-at-home-type thing, rather than some natural inborn shyness, because my father definitely doesn't have it. I've learned to be less shy as the years have gone on, and now I can talk up a storm in many situations, again by just having to jump in the water.

It's funny. I mean, this almost surely would have come up in our interviews, but I'll just say it now while I'm thinking about it. One of the things that attracted me to microbiology was being at the bench and doing experiments, and not dealing with people. I'll be totally honest with you. That attracted me. I didn't really think consciously about it, but that was definitely a major thing that attracted me to it, and it turns out I deal with people all the time, ad nauseum. I've got, you know, seven people in the lab. I've got my colleagues. I've got the other students. It's like I'm immersed in people. And so I've, through the years, learned to cope with it and become social, to some extent.

Okay. So now back to my father. What else did I get from him? So, you know, I think I covered that. I also got some athleticism from him. Everyone in my family, we're pretty good athletes. You know, we're not big, by any means, but we're strong and wiry and fast, and so all of us were pretty decent athletes.

My second oldest brother, Johnny [John Michael Camilli], was by far and away the best athlete. He is just spectacular. I mean, he was like, could have been a world-class speed-skater, but didn't start speed-skating until he was an adult. When he was like, I don't know, twenty-five or whatever, he took up speed-skating, and by the time he was thirty, was one of the best in the country, and actually broke records. He had state and national records for his age group.

You know, no one cares about the over-thirties, but he had records, and he was just a tremendous athlete, pole-vaulter. I just always—I'll just say this, because this is, in a way, funny but true—[he was the] fastest guy I ever knew in my life. So fast, god, he just toys with you. Like if you tried to race him in a hundred-yard dash or whatever, he would just toy with you. He would let you get, you know, twenty yards in front of you, and then just turn it on. So he was incredible, an incredible athlete.

So anyway, my dad gave us some athleticism. I don't know what we got from my mom, because she wasn't really athletic.

VAN BENSCHOTEN: Hold it right there and I'll put this over.

[END OF TAPE 1, SIDE 1]

VAN BENSCHOTEN: All right. This is tape one, side B.

Let's turn now to your siblings [Gerard Camilli, John Michael Camilli, Helen Alvey, Mary Stroupe, Monica Camilli, and Peter Camilli]. You've got seven of them, and if I remember correctly, you were the second, or the second oldest?

CAMILLI: No, no, no. I'm one of seven, and I'm the fifth-born. So there are four boys and three girls, Gerry [Gerard], Johnny, Helen, Mary, me, Monica, Peter, all about a year apart. Peter was about a year and a half; no, no, he was about two years apart. I think there was a miscarriage in there. Could have been eight.

VAN BENSCHOTEN: Tell us a little bit about each one, maybe their full name and, if you could, what they do, their profession, their education.

CAMILLI: Okay. So the oldest is Gerry, Gerry Camilli. Gerard, named after my grandfather on my mother's side. He and I are the only two to go straight through, you know, graduate high school and go straight to undergrad[uate school] and graduate undergrad[uate school] with a degree. The others, several others, and I'll mention them as I go, did not go to college. Some have gone back in later years, and one of them has gotten another degree.

So anyway, Gerry went— He was the oldest born, the most responsible, always was quite clear-cut that he was going to be— Always loved history, and very politically minded and very interested in education, and never wavered. Went straight through, got a degree in history and teaching, and became a high school teacher of history. Now he teaches a lot of different things.

He lives in Denver now and has a wife [Camilla Camilli] and two boys [Andrew Thomas Camilli and Gian Gerard Camilli]. So Gerry, like I say, was very responsible, very good athlete. He was a very good runner and basketball player and was a very good athlete in high school.

He, as well as the oldest three, all went to public schools in Flint, and my older sister, Mary, myself, and the next two were put in Catholic schools, then, when we got to high school, because the results in public schools weren't turning out so well, as I'll tell you in a minute. So Gerry was— He made it through all right. He got a good education and is a fairly well adjusted person.

The next oldest is Johnny, John Peter Camilli. No, John Michael, sorry. John Michael Camilli. My father's John Peter [Camilli]. He finished up high school and that was it. My older brother Johnny ended up getting his girlfriend pregnant when they were both just out of high school, and married her and had two kids [Emily and John] with her. Then they got divorced about three years later. It ended up to be quite an ugly affair, and, you know, loves his kids dearly but lost them. So she went and didn't marry another person, but almost immediately moved in with another guy, and it just devastated my brother to lose his wife and his kids. He had visitation rights and would go see them occasionally, but they moved to another state, so it was very hard on him. That whole thing really took a lot out of him. I mean, he aged much more rapidly than he should have, I think, from just the sheer stress of it all.

He's had a number of jobs over the years, low-paying labor-type things. Tremendously, as I said, just tremendously strong and athletic, and he had a couple of high-paying jobs that were also high risk, like he worked on an oil rig, which is incredibly high paying, but very dangerous, one of the most dangerous jobs there is. So he did that for a while. Then he became a salesman. Did that for a while. Then he moved back to Michigan, actually, and has lived there most of his— For the past fifteen years he's lived in Michigan, where my parents live right now, and he's had a number of jobs. But the last five years he's had a steady job where he's moved up and he's making good money.

His passion has been speed-skating. So he was a very good speed-skater. He both trained younger people and competed himself, and has a few other things that he does, hobbies. He likes to go fishing. There's a lot of nice trout fishing in Michigan rivers and stuff, and he likes to do that. I played golf with him about a year ago, me and my father, and he told me one thing he's into right now in his spare time is he's read some stuff and is learning how to live off the land. So you just go hiking with, you know, bare essentials, and you try and survive for a week. So it's just typical of Johnny, you know. He's away from people and it's just him battling. He needs to fight against something, and in this case it's nature, to survive out there. And I believe he does this alone, which is stupid.

Enough bad things have happened to him that I think his— He's the type of person that is living very dangerously. So, who knows. You know, maybe he'll live to his nineties, but maybe he'll— We'll get a phone call and find out he's dead next week, from starving, starvation or broken leg or something like that [while living off the land]. So anyway, that's Johnny.

The next oldest is Helen. Helen suffers panic attacks when she's out in the public, so she's a little bit agoraphobic, afraid of people. Myself and my siblings and my parents, we've put a lot of things into place as the years have gone on, things that happened and ways we were when we were children. And one thing that she always suffered from and we didn't know until, you know, fairly recently, was she was suffering panic attacks when she was a little girl, you know, going out in public, going to school, or whatever, just hated it and was panic-stricken.

One outcome of her inability to cope with social situations is she was a mean little girl. I wouldn't say a bully. She didn't go out of her way to pick on kids, but you just never got in her face. She was very tough and would not think twice about just decking you if you were messing with her. She was three years older than me, and so, growing up, was always bigger, so I was always afraid of my sister Helen.

VAN BENSCHOTEN: Is that one way that she handled it then, this agoraphobia, or was that just simply a character trait that she had?

CAMILLI: It's hard to say if it was— As I'll tell you also, my younger sister, Monica, has epilepsy as well, and I've got to tell you, it's hard to tell. My immediate thought would be that these things are genetic, either inherited or there's been some, you know, mutation, some defect in some metabolic pathway or whatever that's the cause of these things. But I also have to tell you that some of the things that happened to them and myself, as children, I could easily see these things being outcomes of a bad environment.

Maybe she developed this agoraphobia and panic attack as a defense mechanism to just not get involved with people, not to deal with people. So in my mind it's not clear what's genetic and what's environment. Anyway, so that's been a major problem throughout her life. She did not graduate from high school. She later went back and got her GED [General Educational Development diploma] and, you know, she's very smart, taught herself to play the piano, has some talents, but just couldn't cope with society and the education system, you know, dealing with students and teachers. So she basically has lived on her own. She married, had three children [Michelle, Erik, and Kris], lives out in the suburbs almost like on a farm, and just is away from people. And she's safe and, you know, happy out there. Her kids are a little mixed up. I won't go there.

VAN BENSCHOTEN: Okay.

CAMILLI: So that's my sister Helen. She was the bully, but misunderstood, and could have been— You know, today, growing up around here where I'm living right now, she would have been diagnosed early—or many places in the country—would have been diagnosed early and treated and gotten therapy, and maybe would be better able to deal with society. So that's my sister Helen.

Then the fourth from the top is Mary. So she's one year older than me. Mary was always— She was the beautiful sibling, very pretty girl, you know, always had guys beating down the door, wanting to date her. She was very pretty, got some Italian looks. She has dark brown eyes, black hair. Good athlete, very good soccer player and runner, growing up, but very disinterested in education, so barely scratched by. And she did not graduate from high school. She also left school, I don't know, her junior year or whatever.

She also saw the light later on, after having some dead-end jobs, low-paying jobs, went back, got her GED. She has subsequently gone on and gotten a degree, like a two-year college degree in business, and actually has a very good job now. She has one daughter [Sarah], and husband [Rob Stroupe], and they're actually a very nice family. I like her husband a lot, and her daughter's wonderful.

I almost always go off to seminars and meetings alone, but last year I took my oldest son, Adam [O. Camilli]—he was four then—with me on a seminar visit to Texas, because I knew my sister lived there. So we stayed down there for three days, and I got to spend two days with my sister and her daughter and husband and my son Adam, and we had a great time. So, although she experienced the same things all the rest of us kids did growing up, she somehow came out of it better, much more socially intelligent and able to cope with things. She's a lot like my mother, very realistic and practical, and is able to deal with anything that comes at her. That's Mary.

Then comes myself, fifth from the top. Then a year younger than me is my sister Monica. Monica and I went to school together because in the second grade I got nailed by a car, was in a body cast, and got set back a year. So from the third grade, actually, second grade on, she and I were in the same classroom, same homeroom, whatever, growing up, so we went to school together.

Monica is very sweet, probably the best-natured of all seven of us, but like my sister Helen, the least able, the least capable to deal with society and people. Very poorly adjusted, and really affected her throughout her life. She left high school one semester— She had pretty decent grades. She and I weren't breaking any records. We were C, B, students, and she was one semester away from graduating high school when she quit school. It just shocked my parents, shocked me. I couldn't believe it. Why would you not finish out that last semester? But she had so many problems.

Things just came to a boil, and she left, packed her bags and moved to California with her boyfriend. She dumped the boyfriend within months of getting to California, and by sheer luck—if one were religious, one would say an act of God— the next-door neighbor in this apartment complex, she met, started dating, and he's just a terrific guy [David Prince]. He's my favorite— He's my second-favorite in-law. My older brother Jerry's wife, Cam, is just super. Her name is Camilla. Her last name is now Camilli, Camilla Camilli.

But anyway, so my sister Monica's husband, Dave, is just a fantastic guy. She got lucky, because he's helped her out a lot, and she's still unable to cope with reality, in my viewpoint,

but is engrained in doing things, working. She actually is helping children with problems, mental problems, and is into counseling. She also went back, got her GED, and she didn't go to a university, but she's got some type of post-high school training where she's learned how to help people, how to counsel people in certain problems. So that's Monica. She just had a daughter [Allison] about six months ago, so she's the last of the siblings to reproduce. We've all had children, the most being three. Two, two, three, one, two, one, two.

Now, the last of the siblings is my younger brother Peter, who is at the bottom of the barrel, got tons of abuse from all of us, being the youngest, and to this day I feel guilty about picking on him. And not so much picking on him, but also expecting too much of him. So he was two years younger than Monica, so he was quite young, but he and I hung out a lot, and I just remember asking too much of him physically. Like when we would play baseball or go play soccer or do whatever, I was always putting too much pressure on him, and so I personally feel responsible, but everyone in the family's responsible for just dumping on him too much.

So he also— He is not completely poorly adjusted. He is able to cope with some things. He also didn't experience some of the bad things that we older kids did, from Monica on up through Gerry, just because he was young enough that he just missed out on some things. I'll just leave it at that. But he still had some problems dealing with the real world, and has some problems interacting and maintaining relationships with his family. Like he and my parents have had some real, real arguments where they would go years without talking; religious arguments, some of them.

Peter, however, is probably the smartest of my family. Never studied during high school, didn't go to college, never is home reading anything. Just raw, natural intelligence. He just has a ton of it. My dad spent maybe two weeks showing him how to play chess, and he could cream all of us, you know, when he was a teenager. Just deep, deep mental capabilities in that boy.

Got a job as a salesman and has basically switched a couple of jobs here and there, selling things. At one point—this was about five years ago, so he's now in his thirties—he had enough of menial jobs and trying to sell things and decided to go back to college, or to go to college. So he entered a commuter college where he lives, and took a kind of a general set of classes and decided he wanted to go to be a microbiologist or a molecular biologist, or whatever. So I encouraged him. I said, "Hey, there are jobs." And he didn't stick to it. He did like a semester of that, and I think they ran into some money problems. Their daughter [Madelene] was born right about then, and he bagged it and went back to selling. So it's, in a way, sad. He didn't stick it out. Maybe he was not able to stick it out financially.

VAN BENSCHOTEN: Now, who were you closest to among these seven, growing up, or what siblings?

CAMILLI: So, I was closest to Peter, in terms of just doing stuff with. But it's just, you know, when I think back to the relationships, it's just sad. It was just sad. There was no— You know,

we love each other like siblings, and we did a lot of things together, but we also had very, I would just say, destructive relationships, growing up.

In a way it's because we were not supervised. My parents were not there. We were free to— My dad's working and my mother was whatever, sleeping or watching soap operas, whatever she was doing. So we had free rein. I mean, I always tell this to my friends and my wife [Kristen M. Camilli, née Auchter], for example, that when I was eight I could have gone on a train and gone to Canada for a month, and would have been fine. And I could have come back—

VAN BENSCHOTEN: Nobody would say a word.

CAMILLI: Complete and total freedom, but no supervision, and so we did some stupid things. We almost burnt the house down a couple of times. You know, I got nailed by a car, walking across streets I, as a second grader, shouldn't have been walking across. I told you, my reaction to some of my childhood things is that I'm now the opposite, like cleanliness, whatever, and it's the same when it comes to child safety.

Like yesterday I took my two boys [Adam O. and Ian M. Camilli] hiking. We have a very nice conservation area a mile away from our house, and I decided to take them up climbing this little rock cliff that I've climbed up before and biked up before. I took them up there, and I was just paranoid that they were going to do something stupid and fall off the edge. And so, you know, I said, "Okay." Adam walked close to that thing once and I said, "Adam, stop. Look down there. You do not want to fall down there."

He understood the whole thing. He probably could be up there by himself for hours and never have any problem, but just, I was just super-paranoid that he would get hurt. So what I'm saying is I'm overprotective. That's going to be something I'm going to have to battle with, raising my children, is I'm overprotective now. My wife and I are always watching those two. There's never a chance. They've never gotten within two feet of an outlet. And as we've met other couples with young kids and been over to their houses, I realize now—we're not overboard—but how different we are. Some might think it's overboard.

VAN BENSCHOTEN: [Laughs] You may be bringing up daredevils now.

CAMILLI: Well, they're— Adam's naturally a very safe kid. I mean, he's very curious. This kid's very brainy. He gravitates toward what's dangerous but wants to understand it, and once he understands it, he never messes with it. Like he knows about electricity and he knows about fire. He would never play with matches or turn on the stove or whatever, because he understands it. There's no mystery there anymore. So one thing I do is I demystify. I tell them about things. And he's incredibly curious about it and he'll ask a million questions over and

over again, and then he's on to something else. But he gravitates toward the dangerous and the daring, but not in a haphazard way. And then his younger brother just does whatever he does, so he'll be safe.

I forget where this started.

VAN BENSCHOTEN: Oh, we were talking about how, because you had been sort of a daredevil, had got into this accident as a young boy, that perhaps now you were overprotective.

CAMILLI: Oh, yes.

VAN BENSCHOTEN: By the way, what was the impact of—bad word to use—what was the impact of being hit by the car and being in a body cast? How long were you in a body cast?

CAMILLI: So, I broke up my left leg pretty bad and I broke some ribs, and I was in a body cast for about two months, and then went to just a regular cast for another month. It was right in the middle of school year, so that was it. Although for a while I had been keeping up with my studies, I kind of lapsed a little bit, and when I went back to school toward the end of the year, I fully expected to pass and go on, and the school said no. And I just remember that devastated me. I was so embarrassed to be set back.

So there've been a few moments from my childhood that were, you know, things I didn't have control over, and my parents didn't have control over, that really were just like a kick in the butt to me. And that was one of them, being set back. There were six of us [in the class] that got set back. The others got set back because they, you know, didn't apply themselves in any way whatsoever. Ironically, two of them I was friends with. We played sports together and they lived near me, but I just remember being embarrassed to be set back with and being in the same grade as my sister.

So, as a second grader, I suffered humiliation and embarrassment and anger over that whole thing. Now, I don't know what effect that's had on me, you know, the subsequent years, whatever. I'm just reporting what I remember.

VAN BENSCHOTEN: What other critical events? That was definitely one, but can you think of other— When you're, again, a young boy, fairly young boy, say between five and fourteen; were there other critical events that shaped you?

CAMILLI: Yes, there were. Before I get started about a few other things, that car accident, that getting hit by the car did something else. In a way, I was forced to not do anything athletically. I

was forced to sit and read in the bed and watch TV, but mostly read and think, and I think that changed my course, because prior to that I was a little second grade, first and second grade athlete. That's all I cared about, the only thing I got joy from. The only thing I got positive reinforcement back from my peers was I could do things [athletics].

Like I remember playing soccer. I got a soccer ball in kindergarten, and I remember first grade playing soccer during recess and whatever, I was a phenomenon. And, you know, self memories, self-aggrandizing memories maybe, but I just remember out there playing soccer with these kids my age and older kids, and I was a phenomenon. I could go all over the field, everyone chasing me. They could never get the ball. Getting hit by that car ruined what I think would have been something I would have stuck with, was playing soccer, because to this day this leg [indicates left leg] is not fully functional. I can't lift it beyond a certain thing where it has to start bending, so I took a hit, physically. And although I still enjoy playing soccer and am still fairly athletic, that thing stopped me in my track. Getting hit stopped me in my tracks, and I think changed my course to a more, maybe, educational. Who knows what would have happened, but anyway.

So one thing I'll tell you, and I'll just be honest, is— Well, I told you before that I was shy, growing up, and that was definitely true, and always had somewhat of an inferiority complex. I remember a few things, you know, clearly my upbringing and the sibling rivalries and the fights we had and other things all shaped that, and maybe naturally I'm shy as well. I don't know. But altogether that created an inferiority complex and a shy boy, afraid to venture, and no self-confidence. So that really hurt my education, because I didn't want to raise my hand. I didn't want to join the chess club. I didn't want to— You know, like one year my father realized I had a science interest and maybe some potential there, and he twisted my arm to join the science club, where you do some project and enter it into the science fair, whatever.

I told him I was going to that thing, and I never went to it. And then the science project was due to be entered into the science fair, and I went to the first, where you— It was a new group where there's someone that helps you prepare your science project for the show, and I showed up and I said, "I don't have a project." So I had lied to my father throughout that whole thing, because I just didn't want to have to go and deal with these people, and having to present in front of a group. So, I had this inferiority complex.

One incident I remember that was part of that, I'd say helped shaped me that way, is I remember in kindergarten the teacher was talking about astronomy, about our planet and our solar system and galaxies. She kept talking about the Milky Way, which is our galaxy that we're in, and she kept talking about it as if it were far away. And I remember—I'll never forget this—I raised my hand and I said, "Well, our planet and our solar system, we're part of the Milky Way."

And she said, "Oh, no, no. No, we're not."

And I said, "No, no. I'm pretty sure we are."

And she humiliated me. She said, “No. You’re wrong. We are not part of the Milky Way.”

So I have very few memories of kindergarten, but I just remember being humiliated and angry. I felt a little bit of rage. And when I think about my five-year-old who’s in kindergarten right now, I just can’t imagine a kid feeling like that, feeling humiliated and anger toward a teacher. So maybe, you know, I’m not the same child as my— I wasn’t the same child as my five-year-old, not to say that he’s not very emotional. So anyway, I remember that.

Never really got involved in education and after-school curriculum, and never had friends amongst my classmates. So when I went to high school, to Catholic school for high school, I never made friends with any— Throughout entire high school, never became friends with anybody in my class. My friends were my old schoolmates from public schools who I played sports with, you know, in city leagues. But I never did a single thing in high school, except senior year I played soccer. That was the first year that they had started up a varsity and junior varsity soccer team.

VAN BENSCHOTEN: And why are you in parochial? I mean, there are two other siblings, right, who were in parochial school as well. Why that split?

CAMILLI: Yes, the two younger ones. So, although my oldest brother studied and got a decent education, the next three were just disasters, barely passing each semester, never studied, several of them hooking up with the wrong crowd, and my parents— The rest of us were taken out of public schools and put into parochial school. So my sister Mary went to public schools up until her senior year, and she, along with the rest of us, were put into the Catholic school, and she hated it, started skipping classes, and dropped out. So it backfired with relation to her.

My sister [Monica] and I and my younger brother [Peter] kind of stuck it out, although my sister, like I told you, quit a semester before graduating.

But that was another thing that affected me, is my public school friends; I lost them. You know, I was suddenly put into a high school with a class who had all gone to school together in the parochial schools and all knew each other well, and had already played sports together, and here I was all of a sudden, stuck with a class of strangers. And for someone of my psyche, that was a very bad thing. That was a horrible experience. It just sent me the opposite direction.

VAN BENSCHOTEN: Made you turn more inward, it appears.

CAMILLI: Turn more inward and totally shut myself out of any involvement in that school, including classes. So I never studied. Got by. Occasionally would study the night before an exam so I could pass the course. But in a way I got lucky. It’s the one thing I have, the one thing

that's going to be a recurring theme is there were a lot of fortuitous moments in my life that got me into this career which I love.

One of them was my junior year we had our first exchange student [Jens Nielsen]. The school had never been part of any exchange program before, but we got a kid from Sweden, and he was a soccer player. Oh no, actually, that's the start of my senior year. He was an exchange student senior year. So the start of the senior year, here's this new kid, this Swedish kid, and they just announced they were going to start up varsity and junior varsity soccer.

I knew how to play soccer, so I went out to the first day of practice. This was actually a few weeks before school started, and here was this Swedish kid who was a great soccer player, and I was a good soccer player. He and I and this one other kid were the only ones that really knew how to play well, so we immediately formed a bond and became really good friends, became best friends. And this is, in a way, ironic, because here I was this kid who was totally uninvolved, had no friends, just moped around like a mouse from class to class, and here was this six-foot-three, good-looking Swedish kid who was an immediate star, totally popular, and he and I were best friends.

By the end of that year, he made a major impact in dragging me out from my self-imposed trench and got me involved in things. So we played soccer together. He, within a month of the senior year starting, just nailed me to the wall about what's my future plans. He said, "What are you going to do? You have no plan? What are you going to do in eight months when school's over?"

So he just drilled it into me, and it's amazing, because my parents didn't do that. They didn't pin me to the wall and say, "What are you going to do in one year?" They were so busy dealing with all the other siblings and the problems in their work, that it just, you know. So my parents often failed me as parents, and failed the rest of us. And again, it's this thing where they were not good parents. They were not well adjusted themselves, and they bit off more than they could chew with seven.

So anyway, this Swedish kid [Jens Nielsen] became my mentor, in a way. He got me thinking about my future, and so I studied, my senior year, and got— I think I got all As and one B my senior year, and the reason is when he forced me to think about my future, I said, "Aha! What am I going to do?"

And I went home and I sat down with my— Actually, I went downtown where my dad worked. I called my dad up and I said, "Dad, I need to have lunch with you." Something I'd never done before was go to see Dad at work.

And he said, "Okay. This sounds important."

So we went and had lunch, and I said, "Dad, what am I going to do when school ends this year?"

And he said, "You're absolutely right. What are you going to do?" And we talked about my options. He had some friends at the University of Michigan, and he'd made some calls. He came back to me. He said, "Admission to get into the University of Michigan, Flint, here, is you need a GPA [grade point average] of 2.7 or higher just to get in." My GPA was like 2.5, and so I realized I had to get good grades.

And I talked with my Swedish friend, Jens, and he was a straight-A student. Between all that, I decided to hit the books that year, and I got As and one B, and got my GPA— I ended at like 2.81 or something, and just got— So that was a major success for me, a major victory, and a rare thing in my life at that point. So then I had a plan. I was going to get my GPA up, and I did it, and go into college, and I did.

VAN BENSCHOTEN: Before we get you into college, because we'll definitely want to talk about that, you said that for various reasons that you tended to be sort of an introverted child. What were you doing? Were you reading? Were you collecting things? How did you spend your time in elementary school?

CAMILLI: That's a good question. So, I read, but I wasn't a prodigious reader. I read the usual classics, and I read a lot— So, the one area that I really delved into, which I said before, was science-type books, Popular Science-type books, and some that went a little bit deeper. So that I read a lot about, and some autobiography and biography stuff about scientists.

But other areas, never really read in any depth, and so I feel in a way I'm not well-read at all. In fact, I'm sure that I'm not a well-read person, well-rounded. So, reading, a little bit, but not all my time. I always went to school. I never skipped or played hooky. So I really— You know, I wouldn't say it was self-torture, but I did stick it out. I did go to school. Maybe it was because I didn't have any alternative.

VAN BENSCHOTEN: When you're reading science, these popular journals and books, what was your conception of science? And did you have any inkling that, you know, "I'm going to be a scientist"? Or is it just that science is a fascinating subject?

CAMILLI: The latter. Science just was a fascinating subject. In retrospect, it was an escape for me. You know, reading about that and dreaming about that stuff was an escape. And to this day I can still envision myself on a little space capsule going to explore something where I'm away from this world, so it definitely was an escape.

My other escape was sports, so I was always playing some sports. Not usually organized sports, but, well, a lot of organized sports, but never high school organized sports, for example. I would play in city leagues and would go play with my friends, you know, pickup soccer or basketball. So a lot of my extracurricular time was sports. Some reading at home. I did a lot of

things, like I shoveled walks in the winter. Growing up in Michigan, that was a big part of the winter, earn a little money. So I just have a lot of memories of shoveling walks on snow days when there should have been a machine doing it, just shoveling until my arms were ready to drop.

VAN BENSCHOTEN: Did you have any special teachers, mentors that were helping you in school?

CAMILLI: Nope. There were a few favorite teachers who I just remember fondly. It wasn't reciprocated because I was so disinvolved. There was never really an opportunity for a teacher to form a special relationship with me, where they would, you know, help me get through things, and teach me. So I'd never had that, not until college, really. So there was no major force in my life from any teacher. My father was the major driving force in my becoming educated, and my Swedish friend.

VAN BENSCHOTEN: What was the name of your Swedish friend, so we have this on record.

CAMILLI: His name is Jens Nielsen.

VAN BENSCHOTEN: And are you still in touch with him?

CAMILLI: Yes. Yes. We see each other every five or so years, basically. After high school, the year after high school, he came back and we spent the summer together. We drove around the country and stopped in L.A. for the Olympics, went to a lot of events together, and that was a great summer.

Then a few years after that, I went to Sweden for a week, and went to his wedding. Then we were there a second time. I forget what the second time was for. I guess it was just vacation. And then he was in the [United] States about a year ago. And in between all those years, we e-mail occasionally, send Christmas cards with photos, and stuff like that. He turned out to be an engineer. So he was a good friend. He is my best friend of my life, I would say, although I have a pretty good best friend now. But childhood and school-age time, he was my best friend.

VAN BENSCHOTEN: And were you dating at all when you were in high school?

CAMILLI: Very rarely. Very rarely. I, you know, occasionally would be smitten with some girl that I would have a crush on, but rarely, you know, would be bold enough to act on it. I had

a few girlfriends. When I was in the public schools, like in junior high school, I had a couple of girlfriends who— I think one of them lasted several months that we were boyfriend and girlfriend. But in high school, no girlfriends. Went to one or two dances.

One girl I really had a crush on asked me to a Sadie Hawkins [dance], and that was like a big moment for me. This girl who I just thought was beautiful actually asked me to a dance, so that was a big moment. That was my senior year, also. Everything happened my senior year. So, high school, I don't believe I'd had any— I did zero dating in high school, other than for a period with that one girl.

Now, it's funny, because after high school— So, after you graduate, that summer there's a lot of going to parties at other high school kids'—I forget what you call that—but anyway, all of a sudden there was like three or four girls who were classmates of mine who were very interested in me. So it's a weird thing. In a way, I was (a) confused, and (b) almost a little angry at these women, who I'd known and talked to all throughout high school, who all of a sudden were about to enter the world and now expressed an interest in me. So I was a little bit bitter. One of them I did see, though. We dated for a few months during that summer, but then everyone went away to college, and that was that. So, no dating; almost zero dating.

[END OF TAPE 1, SIDE 2]

VAN BENSCHOTEN: Okay. This is tape two, side A.

We got you up to high school. I want to sort of circle back to religion, to ask, were you a family that attended church fairly regularly?

CAMILLI: Yes. Growing up, we went to church just about every Sunday. It wasn't until my teen years that the kids started— As each kid got fourteen and fifteen and sixteen, we started finding excuses not to go. My father [John P. Camilli] tried his best, but wouldn't really force us. It wasn't mandatory. So, you know, probably by the time I was sixteen, I just stopped going, and then ever since then, occasionally would go. Like if I was home on the weekend from college [University of Michigan, Flint and Ann Arbor] and they were going, I would go.

But at that point I had an understanding with myself. I was at peace with my lack of religion, and would go just to be nice, and just for, you know, the tradition of it. So, like going [to] Christmas mass, whatever, I did that for years and years and years. [It's] not until I permanently moved away to grad[uate] school and did my postdoc[toral fellowship] and all that, that I am totally removed from religion. But even to this day, if I'm home, like if my wife and kids, if we go home for Christmas, which we do occasionally, we'll go to the Christmas mass, and I have no problem saying all the prayers and shaking people's hands and going up to receive the body of Christ, whatever. I have no problem with it.

When it's time to pray, I'll even say a little prayer. I usually find some way to entertain myself, to do a prayer. Like perhaps I'll say, "Well, just on the outside chance, this is what I'd like to say." And then sometimes if I'm in a bad mood, it'll be a very sarcastic prayer, things like that, or I'll just think about whatever. But so occasionally now I will go to church, but growing up, yes, almost every Sunday. We had a few traditions growing up. That was one of them.

VAN BENSCHOTEN: How about jobs during junior high school and say the summer, and college, even, or in college?

CAMILLI: When I was a child I had a lot of, you know, mowing, shoveling-snow jobs. Actually, some of the positive figures in my life were old ladies that lived next door who I would shovel or do gardening for. Like I remember in particular there was this one lady [Janet Hines] who was, you know— Her husband had died years before. She was in her seventies, but very outgoing, golfed all the time. She turned out to be a good role model for me, so I did a lot of work for her.

VAN BENSCHOTEN: A good role model in what way?

CAMILLI: She was a good role model in that we would have adult conversations. You know, I started doing stuff for her when I was maybe twelve or thirteen, and we'd go garden. We'd plant flowers every spring, and she'd be out there working with me and we'd have conversations. She would ask me the types of questions that a child should be asked. "What are your interests? Oh, you play soccer. Oh, who do you play with? Are you good? Oh, you like astronomy." She was a very good conversationalist, and very sweet.

And at the time, you know, I also wanted the money, so that would be my spending money. We didn't get allowances. Then in my freshman year of high school, I think, that's when I really started getting a real job with a paycheck, and I started doing things. I was a busboy. At several restaurants, I would be the busboy. Those jobs I always look back fondly on because of the food, the free food.

But some of them were just awful. Like I would go to school and then I'd get on a bus and then I'd exchange to another bus. I'd go out to, you know, the shopping mall out in the suburbs and I'd be busboy until ten o'clock, and then I'd go home. It was crazy for a high schooler to be doing this, just crazy. And I saw the part of adult life that I should not have seen, you know, being a busboy in a restaurant with all these young adult waitresses and waiters and cooks. So anyway, a young, freshman in high school, I should not have a job like that.

But anyway, I had a couple of busboy-type jobs, waited tables for a little bit, and then as I started getting— I started getting really good grades, and got a couple of scholarships, and that

was it. I stopped doing jobs. Although that was later; that was in college. So for high school I had a couple of jobs. Actually, one summer worked at the church, our local church, scrubbing floors and stuff like that. Yes, always had some job during the summers; during the school year, rare jobs.

VAN BENSCHOTEN: And when you become accepted at University of Michigan at Flint, what direction did you think that your life would take?

CAMILLI: Although I knew, having had some jobs that were not very enjoyable, that [with] a higher education, that I could, if I was lucky, end up with a job that was enjoyable. I was cognizant of that. I didn't really know what I was— I didn't know specifically what I might like to do for a career, although I knew it was in the sciences somewhere. So actually, I started my freshman year of undergraduate as a computer science major, having never sat down and computed before, but having read a few things, and a little bit of guidance from my father about computer science is the big thing.

VAN BENSCHOTEN: It was taking off at that time.

CAMILLI: Yes.

VAN BENSCHOTEN: Still is, in many ways.

CAMILLI: Yes, still is. It would have been a great career. So anyway, I was a computer science major for one full year. I did a lot of the general courses, but also I had my computer science main courses that I took, and I did well in them. I got As and Bs in them, but I was bored to tears. I just hated it. I mean, computer science, it was like doing calculus. At that point, for me, it was like doing calculus. We were learning the languages, the programming languages, and it was just rote memorization, and only rarely did we have any fun with it. Like one time they said, "Okay. Go design a board game. Write a program to play a board game," checkers, chess, whatever. And that was a challenge, and I love that, but most of it I hated.

So, start of my second year I was taking more of the general requirements, and I took a biology course. It was actually called Human Genetics, and we learned about human genetics and some of the diseases that one inherits, and I just remember that that just hit me as something (a) important, (b) I was totally absorbed by it. So I knew at that point that I wanted to study some type of aspect of biology and do research.

VAN BENSCHOTEN: What was it that absorbed you?

CAMILLI: It was two things. It was learning about how our bodies work and how we reproduce, and how [for] all of the life forms on this planet, the blueprint is the DNA. That's what we learned in that course. And then, as if that weren't interesting enough, we learned about mistakes in the DNA that caused diseases, and so just that idea that a change in this single base in gene X leads to a person who can't drink Coca Cola with certain amino acids in it or they'll die was just fascinating to me.

But also, the practicality of it. That was very hot at that point, too—genetics. And I learned that, you know. The teacher told me that. My father told me that; that if you become educated in this and do genetic research, that's a good job. So it just all hit me right then and there that I want to be a biologist. I didn't know exactly what type of biology to do. That came the next year. So, basically I was bored by the other courses. You know, I did well in the other courses, so my freshman year I got like a 3.8 or whatever, and got a scholarship for the next year. That was pretty cool. So I was getting positive feedback that encouraged me to keep going and study hard.

VAN BENSCHOTEN: Were you still in touch with your Swedish friend [Jens Nielsen]?

CAMILLI: Yes. We would exchange letters, and he was just thrilled that I was sticking to the books, and is very proud of himself, which he should be, that he saved someone from mediocrity and menial labor and god knows what else. Yes, so I stuck to it.

I suppose you want to save more of my development into this area for later.

VAN BENSCHOTEN: Yes. Okay. Let's leave it there. I'll begin it again tomorrow.

[END OF TAPE 2, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Andrew Camilli**

INTERVIEWER: **William Van Benschoten**

LOCATION: **Tufts University Medical School**
Boston, Massachusetts

DATE: **8 October 2002**

VAN BENSCHOTEN: This is tape three, side A. I'm with Andrew Camilli.

We left off with your sophomore year, I think, sophomore year, actually, in college [University of Michigan at Flint]. You had talked a little bit about that first year and your fascination with especially human genetics, DNA, but you didn't know quite where you were going to go. What was the next step in that? I know that at some point you leave the University of Michigan at Flint and you go to [University of Michigan at] Ann Arbor. Was that critical in choosing your subject and moving at the same time?

CAMILLI: Choosing the subject came first. So the human genetics course really turned me on to biology and genetics. I remember speaking with my instructor, as well as with another professor who taught another course that I took, called limnology. That's the biological study of freshwater systems, like ponds and rivers. I may have taken that the same semester, or the following semester, as the human genetics.

But anyway, in talking to both those professors, I got some advice as to what are hot fields right now, and it came up repeatedly, molecular biology. And if you can go study human diseases, for example, that would be— That's very exciting and important stuff. But at that point I hadn't really decided what, but I thought molecular biology, working with, doing new types of genetic analyses, things like that, would really excite me.

So with that I also got advice from them and also my father: that you need to go to a place where there's cutting-edge research being done, because you have to get research experience. So the obvious choice was to transfer to the Ann Arbor campus, which is the main campus of Michigan, and there's where most of the research is going on, both medical research and other biological research.

I remember getting some information and just being amazed at how much, all the different types of labs and work, that's going on down there, and so I transferred. I got a scholarship for my first year there, so that was then my junior year. Although it turns out, all my credits transferred. But as it turned out, I was missing a few classes, so it took me five years total, two years in Flint, and then three years at Michigan. So anyway, it took me five years to get my undergrad[uate] degree. I may have that backwards. Maybe I did three years in Flint and

two years at Michigan, but anyway.

So I knew I wanted to get into research, so I decided to go to Ann Arbor. I went there, and I, my first semester, did not choose a program, a degree program yet, and just took a bunch of different courses. And that semester—so this was my junior year—I took a course called Epidemiology 101, or something like that. It was an advanced undergraduate course, and what it talked— The course was misnamed. It wasn't really epidemiology; it should have been called bacterial pathogenesis or something like that, because that's all it was. We talked about different types of bacterial pathogens of humans and learned their major virulence factors, and also a little bit about the research that was producing this information about these pathogens. And that was the course that got me set on my current track of being a microbiologist.

There are many different names you could use to describe my field. There's molecular microbiology. There's medical microbiology. I'm also a molecular biologist. But probably the best descriptive term is a molecular microbiologist, and that's what that course talked about.

I remember my professor. His name was Carl [F.] Marrs, with Carl with a C, Marrs with two Rs. He was from this field. So one thing I'm going to try and impress upon you is this field I entered is like a big family, and everyone knows each other, and there've been some strong laboratories that have trained a lot of the people in the field. And Carl Marrs came from perhaps the most famous of the labs in this field, which is Stanley Falkow's lab. Stanley Falkow's at Stanford [University], and he's like— Many consider him to be the father of our modern molecular microbiology field, or bacterial pathogenesis field.

So anyway, Carl came from that lab, so he was also involved in research. So I spoke with him after class a couple of times and said, "This stuff really seems interesting to me. I would like to do some research." Now, he was on the medical campus at [University of] Michigan [Ann Arbor], which is separated from the main campus, so all of my other courses were on the main campus. So he suggested that I switch my major to microbiology, which I did. Toward the end of that semester I switched to microbiology, and then had to take a bunch of different courses to get my degree, to fulfill the degree requirements, and almost all those courses were on the main campus.

There's a microbiology department on the main campus, so I went, I took some of those courses, and met— And actually, the professor [Robert B. Helling] who taught the microbiology on the main campus, I went and spoke to him after class one day and said, "I would like to do research." And he, as any good professor would in an undergraduates' campus who has room in the lab, said, "Okay. You can come to my lab."

This is actually a funny story. I went into his office. It was still toward the beginning of the course. I didn't really know what I was talking about, but I went into his office afterward, set up an appointment sat down, and I said, "I would like to get in the lab and try my hand at research. Don't want credit for it, just come in after hours and do research after classes."

And he said, "Very good. Do you have any idea of what area of research you'd like to

do?"

And I said, "I want to clone something. I want to clone a gene."

And he just chuckled. He said, "Well, cloning is just a technique that we use to study some biological problem."

So I was very naïve walking in there. I just wanted to do the technology. And so he was patient with me, and he said, "Well, let me tell you what's going on in my lab, and see if that might be something that interests you." So, he was studying E. [Escherichia] coli and more classical microbiology stuff, where he was using transduction and using phages to move DNA around, and using various markers. He wasn't doing the most modern, cutting-edge stuff, but he convinced me that I needed— You would need a background in classical genetics in order to then proceed on and work on bacteria with newer techniques.

So I went to work in his lab. His name was Robert [B.] Helling, Helling with two Ls. He recently retired, I think this year. So anyway, he set me up to start doing a project after my courses were over, throughout the semester, and then in the summer I worked full-time. He worked very closely with another professor, Julian Adams, who's right now the chair, actually, of the microbiology department at Michigan. So I worked kind of for both of them. They were both my mentors, as an undergraduate, and I learned some basic techniques.

But it was still that epidemiology course, that bacterial pathogenesis course, that was what I wanted to do. So what I was doing in Helling's and in Adams's labs, wasn't that, but I was learning techniques that I could apply toward studying bacterial pathogens, so I was quite happy with it.

I ended up continuing to do research for Robert Helling and Julian Adams until I graduated a year and a half later. Those two professors were very instrumental—and also Carl Marrs—in suggesting graduate programs, and where are the good places to go to learn how to do cutting-edge research on bacterial pathogens. And there were a few obvious choices at that point, and I took the advice of them and went to one of those places.

Actually, another funny story is Carl Marrs— So he was the one with experience in the bacterial pathogenesis field, and he suggested some schools. And at the top of his list—well, near the top of his list—was Washington University in St. Louis, which has a very large group of people studying bacterial pathogens. So that was one of my five or six schools, I forget, that I applied to for graduate programs.

So, meantime I'd taken GRE [Graduate Record Examination], got a decent score. I'd done research, I had good letters, and I was marketable. I could get into these grad programs. So I got into all but Harvard [University]. I got into [University of California,] Berkeley, got into University of Washington, Seattle, got into Washington University, got into a couple of others.

So I went on interviews to these places, and at Washington University there were five or

six professors studying bacterial pathogens. So that had the greatest concentration of labs studying that, so that was at the top of my list. And I interviewed with this young, new professor, this guy named Daniel [A.] Portnoy, who— I'll tell you later—turns out to be my Ph.D. mentor, and he just really had, I thought, very exciting research going on. He was incredibly enthusiastic. He was new, so I knew he would be— I would get lots of attention from him. So Washington University and he were at the top of my list for where to go and train to get my graduate education.

So I went back to Michigan after my interviews and I went and talked to Carl Marrs, and I told him about all the places I went. He asked, "Who did you meet with?" I told him about Washington and Dan Portnoy, and he overlapped with Dan Portnoy in his postdoc[toral fellowship] lab and despised Dan. So he said, "Washington University is a good school, but don't— I do not recommend you go to work with Dan Portnoy." And at that point I was just applying to these schools, so I took his advice. I said, "Okay. I'll keep that in mind."

And I ended up going to Wash U. [Washington University, St Louis], and first day went to Dan's lab to do a rotation, and ended up going into his lab. So it was funny that Carl, although he's the one that got me into this field with his exciting course and gave me lots of advice, ended up telling me to not go where I ended up going Which is more of a personal thing.

Dan Portnoy, it turns out, he's the type of person, you either love him or hate him. He can be very obnoxious, brilliant, but very obnoxious. And Carl Marrs is kind of— I could easily foresee Dan having a passion for teasing him and giving him a hard time. So anyway, those two obviously didn't get along well.

So anyway, that really sums up my undergraduate education. I ended up working in those labs, Julian Adams and Robert Helling, for about a year and a half, including a whole summer.

VAN BENSCHOTEN: The work that you did in those labs, was it mostly, then, sort of a technical education then, or were you studying particular—

CAMILLI: That's a good question. It was mostly technical, but I did get a foundation in something very important, and that's called population biology. Julian Adams is a population biologist, and they were doing a very interesting set of experiments when I joined the lab and did some work. What they would do is they'd take a single bacterial cell of E. coli and inoculate what's called a chemostat. A chemostat is basically a big glass flask, but there's a very slow influx of nutrients and water, and an outflux with filters, so new nutrients were coming in and at the other end of this chamber it's going out, and so the main body of the chemostat is a kind of a constant environment of nutrients coming and going out. You inoculate that with a single cell, which starts to multiply, and it sets up its own little microcosm. The only difference between the natural environment and this is it's one bacterial cell started the population of cells in that chemostat.

Then you maintain this thing running for hundreds, sometimes even thousands of generation times for the bacteria. So they studied population biology and were asking some fundamental questions. How do the bacteria change over time? Because you started out with single cell, a single clone. And it was very fascinating, because at the end of, say, 300 generation times, the bacteria would establish an equilibrium population that was different from the starting strain.

Another very interesting thing is that the final equilibrium state usually had a couple of variants that were now coexisting together in a mutualistic way, a beneficial way. So this single cell would give rise to multiple strains who differed in their utilization of nutrients and what byproducts they spit out, and by working together, they were able to make better use of the environment.

So anyway, that was a very interesting lesson. I learned some very interesting things about population biology that, to this day, still are in the back of my mind and help me in my thinking, because what we do today is, are constantly growing up the bacteria, starting from single cells which form a colony on a plate, and we'll pick that colony and do things with it. So now I feel I have a very strong intuition for the types of genetic variations that can happen in that colony, or when we grow it up in a glass flask in a liquid media, how the bacteria can vary.

That's a fascinating thing about bacteria; they're incredibly dynamic. They will change, they will undergo mutations, and evolution will act on that, and you'll end up with something different, if you're not careful of what can happen. So you have to be very careful of what selective pressures you're placing on the bacteria, because they will respond by adapting, by undergoing mutations. And that's critical, because in microbiology you like to know what you're working with. You inoculate a culture with a single cell, and you hope that the end product is identical to the start, because you know what the start was.

So anyway, I did learn a lot of population biology and learned tons of techniques, like how to transform bacteria, how to put in new DNA, and just how to work with them. And sterile technique, that's the other thing that was important. Having no background, though, when I first showed up in the lab, I was put with a graduate student who I worked with for a while, and my first week in the lab was so bewildering. I didn't understand.

All the bottles of reagents were marked with a numerical system, so a bottle of sodium chloride would be marked 10X, and another bottle of some other compound would be marked 100X. And all these bottles had these numbers and an X, and I couldn't for the life of me figure it out, and I was a little too embarrassed to ask. So finally, after a couple of days it was my turn to start making up solutions, and I was told, "Okay. Go make up a 100X stock of sodium chloride."

So finally at that point— I was very frustrated, didn't want to— Wanted to figure it out on my own, but ultimately couldn't. It's laughable now, because X is just whatever you want it to be. So 100X is 100 concentrated solution for what you would use in some experiments. X is

user-defined. It's whatever you want it to be. I was perplexed by this X system, but I got over that.

So I was quite naïve, starting off, and I learned. This grad student was very patient with me. I only worked with him for a month or so and then he left, so I don't even remember his name. I could probably find out.

So, you know, as I got into the science, I have to say in retrospect, something was driving me forward, because as I told you yesterday, I was a shy person growing up, lacked self-confidence in a number of ways, especially socially, didn't really study up until my senior year of high school, so I wasn't well read, wasn't a good writer, yet still something kept driving me forward. There was some bit of self-confidence back there that kept me going.

When I learned about this research that was being done, and [when] you work in the lab and you're looking under the microscope and you're doing experiments, that really appealed to me. And there must, in the back of me, [have] been some self-confidence that said, "You can do this. This is something you would enjoy, and if you enjoy it, you have a good work ethic, you're going to make it." So I think that kept driving me forward over these stumbling blocks.

Like going in to see the professor and asking him to do research was a major thing for me, to get up my nerve to go ask that. Because, you know, there are 30,000 undergrads at Michigan. He could have easily said, "Sorry. No way. There are lots of other more qualified students that want to do research." So anyway, I got by and, you know, figured out what I wanted to do, and I'm still doing it, so in a way I was lucky.

VAN BENSCHOTEN: You eventually, as you know, in '87 you go to Washington University. I don't know where I picked this up when I was reading in the paperwork here. Did you work in the Roy Curtiss [III] lab at Washington U.?

CAMILLI: Yes, I did, but that was a rotation. A first-year grad student does three or four rotations through various labs. I should say, prior to starting grad school I worked at Upjohn in Kalamazoo, Michigan, for that summer, in a molecular biology lab. That was advice from my father, my future father-in-law [Richard Auchter]. I told him what I wanted to do and I said, "I may want to go into industry at some point after getting my Ph.D."

And he said, "Oh, well you should go do an internship. You should go see what it's like." And it was great advice. So I went to Upjohn, worked in a molecular biology lab, and I saw the advantages and disadvantages of industry, and was quite sure after that summer that I wanted to stay in academia, not fully being aware of what awaited me in academia, which also can be quite stressful.

VAN BENSCHOTEN: What were the pros and cons of working at Upjohn?

CAMILLI: So, incredibly well-funded research. I mean, whatever you want, you got to move your project forward. The company life was nice, like there was— They had intramural sports. I mean, that's something you don't expect to have in your job. So I played soccer that summer with— Our department had their own team and we'd play other departments. So they had great social activities.

And the pay, of course. The pay is much higher than one can get in academia. And I couldn't believe how much I was getting paid that summer. And I knew the numbers. I had done some research. The advantages are: you have a lot more time for family, but you get paid a lot, and the job is pretty enjoyable in terms of, I guess, the social life in a company. Well, it probably varies by the company. So there were some advantages.

The bad news, what really struck me was during the summer I was there: the higher-ups told the lab to drop what they were doing, which is something they'd been working on for a couple of years, and totally switch to something different. And that just hit me as— And I could see kind of a depressed look in people's faces in the lab. They had put a lot of their time and effort into this project, and then the rug was pulled out from under them and they were moved on to something else. So I thought, so the bottom line is, you're not your own boss.

Upjohn's a big company. The jobs were pretty stable there. But another negative, I learned through the years, is that like startups and small places, you could easily lose your job. So that type of uncertainty was very unappealing to me. So those were the negatives.

VAN BENSCHOTEN: Now, your father-in-law suggested this. That suggests to me, too, that you've met your wife-to-be [Kristen M. Camilli], I guess, at this particular point?

CAMILLI: No, actually, prior to that. We'd been dating for a number of years before that. We went to separate universities our first three years, and then— Well, her first two years, my first three years. Then my last two years and her last two years happened to be— She transferred to Ann Arbor, for two reasons. One, for her business— She was getting a bachelor's in business, and Michigan was a good place for it, but also, I was there.

So we had a long-distance relationship and then we were in the same city for two years, and that was great. Then after that, I went away to grad school in St. Louis. Oh, she had one more year. She had one more year after that. Then, as I'm going to tell you, I had to transfer to [University of] Penn[sylvania], and she then got her bachelor's in business administration degree, moved to Philadelphia, and got a job. We lived separately, but in the same city, for the rest of my graduate education, and then we got married my last year of grad school there.

VAN BENSCHOTEN: So that would be about, what, '92?

CAMILLI: Oh, so I'm wrong, then. We got married in '90. Let's see, '87, '88. Yeah, we got married midway through my years at Penn.

So how did I get to Penn? Okay. So I went to Washington University, was taking classes, rotated with Dan Portnoy [in my] first lab rotation. You do about an eight-week stint in each of the labs of your choosing to see if that's a good match for you.

Then I did a rotation with Bill [William L.] Goldman. I'm pretty sure it was Bill Goldman. Actually, I can't remember who I did my middle rotation with. But then my third rotation I did with Roy Curtiss [III]. He's one of the reasons I went to Wash U. I'd met him and he was doing great work. But right when I started the rotation in his lab, Dan came and told me the news: he was moving to Penn. He was moving mostly for personal reasons. His wife was an artist from New York City, and was just suffocating in St. Louis, so to save his marriage, he had to move back to the East Coast, and so he got a job at Penn. He also was somewhat unhappy with the department there, so it was a good move for him.

So anyway, he came and told me, "I'm leaving in two months. I'm going to Penn, and I'd give anything for you to come, transfer and come to my lab." And he really made a strong commitment to me. He said, "I will not— If you transfer to Penn and do your thesis in my lab, I will not let you down." Those were his words.

VAN BENSCHOTEN: Wow.

CAMILLI: So he really wanted me to come. It turned out the rotation project was just awesome, so it was a clear choice in my mind. So anyway, I decided to stay and finish my rotation with Roy and finish my first year, finish my courses, which I did. As a result, it turned out that I did a six-month rotation with Roy. It's only supposed to be a two-month, but I wanted to stay and finish off my classes, yet not choose a thesis lab, because I knew I was going to be transferring. So I decided to stay and finish my rotation, finish my classes.

I did a really long rotation in Roy's lab, and got some stuff done, got some things to work, and I actually ended up getting two papers out of that rotation. Actually, I tell my grad students this today. I got a paper out of every rotation I did, which is like— It was mostly sheer luck, but I did work hard. Because nowadays, to get a paper out of a rotation is almost unheard of.

But anyway, so I worked for a long time in Roy's lab and got to know him well and got to know the lab well, and to this day he and I are really good friends, and he's written me several good letters of recommendation, apparently good ones.

VAN BENSCHOTEN: What was your working relationship with him then, when you first began that rotation? Did you sit down together and say, "Okay, I have a couple of ideas," and throw them out? How did that work?

CAMILLI: Basically, the way it worked and the way it normally works is you show up and you talk to the professor and they propose a couple of projects. I had the gist of what they were doing in their lab but didn't know the details. And typically, you want to get a hot project, something that's doable and is right off the press, so you can only get that from talking to the professor.

So I went in and we talked and he told me about a couple of projects, and usually you steer the student toward one project that you think is going to be best for them. So he steered me toward a project working with streptococcus species. So I worked on that project and got things— Did a screen, found some interesting genes, and got some good data out of that. Now, he obviously knew that Dan Portnoy was leaving. From right up front, he knew it. What he didn't know was that I was strongly convinced— I had strongly convinced myself I was going to be going, yet I stayed and did my rotation. I was open to it, and I told Dan that, too. I said, "I'm going to stay and finish my classes. I'm going to finish my rotation, and then make my decision."

And he said, "Fine." So I didn't really tell Roy until about two months or so into the rotation. I sat down and I told him that I may be transferring to Dan's lab. And he, you know, he made an argument for me to stay, and he wasn't— You know, didn't twist my arm too much. So then I finished the rotation, got some stuff done.

He was quite happy with my rotation and said, "I'd love to have you stay and do your thesis project here."

So I had to bite the bullet, though, and I told him right then that "I'm 99 percent certain I'm going to transfer and go to Dan's lab."

So he said, "Well, I'm disappointed, but Dan's doing great work right now. I can't argue with your choice."

So then off I went.

VAN BENSCHOTEN: That was in, I believe, '88, '89.

CAMILLI: Yes. And I had to transfer. I couldn't go directly into Dan's lab. I couldn't get my degree from Wash U. [Washington University], obviously. I still had four, five more years of grad school to go. But the way Penn handled it, and the way most places would handle it, is I had to transfer into the program, so I had to go and interview. I got credit for my courses that I

had taken back at Wash U., but the other stipulation is I had to do three lab rotations. I had to at least make it appear on the surface that I was open to going into one of the other labs, because why else would they want me, other than to appease Dan?

But that was all fine, and that's the way it's usually done. So I went, I did some rotations, very short rotations, and really did rotations based on what I wanted to learn for my graduate work with Dan. Everyone knew that I was going to his lab, so that's the way it turned out.

VAN BENSCHOTEN: Before we get to that work, if you could talk to me a little bit about Dan Portnoy. You've already mentioned a little bit about his personality, but what is his mentoring style?

CAMILLI: He was a fabulous mentor, and I believe he's still the same today. First of all, he's brilliant. He's got near photographic memory, so he reads all the papers and remembers every little fact.

The building up of a foundation of facts and knowledge is critical in our field, because there are so much data out there, and you— Most successful people build a sort of intuition, a sixth sense over where to go with a biological problem that they're studying. But in order to make the right choices, to have the right intuition, you've got to have a lot of information stuffed into your head, and Dan has a tremendous bank of knowledge and great instincts.

So he, as an advisor—you know, seeing him every day and talking about your work and getting suggestions on where to go—was just the best. He's the best I've seen. I was his first graduate student, so I had his full attention, which was also great. I've learned there's something special about the first student. There's something special about that interaction and the training that first student gets, and it happened to me in my lab. My first student turned out to be incredibly successful in his graduate work, and it's because I— Mostly it's because of our relationship.

So Dan and I had a good relationship. He annoyed me sometimes, like he would always be in— He lived very close to the lab. He was always the first in in the morning. He would always go look at my plates, interpret the experiment, and have the next set up experiments ready to go. So I'd come in and here would be this burst of words from Dan, because he's very excited, very enthusiastic. And so this went on for a while, and one day I told him, "I don't want you to do this anymore." I said, "You've got to let me come in, interpret my experiment and come up with some ideas, and then I come to you." And he agreed that that would be better. It would be better for my education if I was forced to think.

VAN BENSCHOTEN: And how large— I mean, you're the first grad student. How large did that lab get while you were there, before you left for your postdoc?

CAMILLI: It got to maybe seven or eight people by the time I finished, so he grew. He always had a couple of undergrads, and even a high school student who's now his wife, actually. When I first entered Dan's lab at Wash U., there was a high school student who came to do research in his lab. It was a totally platonic thing, back then. She came in, she was very smart, got some work done, and then off she went.

Then Dan went to Penn, and as you'll see, several years later went to Berkeley, where he is now. When he was at Berkeley, this former high school student that learned to do research in Dan's lab was about to go to medical school, and she went and talked to Dan to get advice. And I don't know exactly what happened, but they ended up hitting it off and ended up getting married, and now they have kids and are quite happy with each other.

So he always had a few undergrads, and even a couple of high school students, and a technician. That was really the start of his lab. I was his first student. Then, very shortly after I arrived he got a postdoc, the next year got another grad student, and it kept building. So by the time in my last year or two, he had two or three postdocs and two or three grad students. So I overlapped with a couple, mostly postdocs, actually.

Dan's personality is such, as I said— He has a very biting personality. Very, very incredible sense of humor, almost like in a Howard Stern-ish kind of way. Very funny but also he can make a comment that is almost a direct challenge of you sometimes. And why he does that, I don't know. So one thing I have joked about Dan is he's—oh, what's the word—subclinical Tourette's Syndrome. That's what I like to say, because often when he first meets you or knows you, whatever, he'll often say something that is something almost like a person with Tourette's Syndrome would say, something that cuts right to the chase and is offensive. That's how he is.

So a lot of people could not get along with him, and so maybe that affected— He didn't get too many grad students starting off, and maybe that's why. For example, the first time I met Dan— I told you, when I went to Wash U., my first day there I went to see him in his lab and say, "Can I do my first rotation with you?" Wanted to beat the other grad students there.

So I go into his lab and he looks up and sees me, and he's just happy. He knows that we hit it off during my interview, and he says, "Come back here. Let me show you what I'm working on." So, he's the only one in the lab that day, and he's spinning down some— He's infecting some eukaryotic cells with some bacteria and looking at it under the microscope, and it's very exciting stuff. So he brings me back there and shows me what he's doing in the back of the lab, and then he gets something spinning in a centrifuge and there's a moment of small talk. This is within ten minutes of us meeting, except for the interview, which was the prior year. So he looks up at me and he says, "Okay. That's got to spin for ten minutes." And he says, "You're probably Catholic, aren't you?"

I said, "Yeah, I was raised Roman Catholic."

And he said, “I’m Jewish.”

And I said, “Yeah, I kind of figured that out.”

And he looked at me and he said, “You know, we killed Jesus Christ once, and we’d do it again.”

VAN BENSCHOTEN: [Laughs] Oh my god.

CAMILLI: That was his first bit of small talk with me. I just looked at him and just started laughing, because it was just— So I don’t know if he was testing me out to see if I was going to be compatible with him or what. But I just laughed. I thought it was funny. I guess some Catholics—or Christians—would have been horrified, but I just laughed.

So anyway, I’ve always gotten along with Dan, and his sometimes crude, sometimes evil sense of humor is fine with me. It’s funny because he, underneath it all, is an incredibly kind person, very perceptive and very generous, yet he has this outward obnoxious, offensive—

VAN BENSCHOTEN: Sort of in your face, kind of gruff—

CAMILLI: Yes. So I’ve often thought it’s almost like a defense for him. It’s a way to keep people from hurting him. Whatever. But anyway, so Dan and I got along well.

VAN BENSCHOTEN: It’s while you were in his lab that you published your first paper. And you have fairly— I mean, from reading your recommendations, too, that were supplied to the Pew [Scholars Program in the Biomedical Sciences], most people noted just how productive you were as a grad student. You had four first-author papers. I was wondering if you would maybe describe the evolution of the projects that you take in Dan’s lab.

CAMILLI: Okay. So, that first day when I went in to see him, he was working with a bacterial pathogen called *Listeria monocytogenes*. This is a pathogen that people ingest accidentally, eating contaminated cheese or dairy products, and occasionally, if you get a big enough dose or your immune system is compromised in some way, this can cause very deadly disease, so it can spread systemically in your body.

What this bacteria does is it goes inside of your cells and lives on the inside of them like a virus would. So it hides from your antibodies and other parts of your immune system, and these cells float around through your bloodstream and it disseminates the bacteria, and ultimately the bacteria invade the liver and the spleen and start growing. If you’re pregnant, it’ll

cross into the uterus and cause a septic abortion. So, a very deadly pathogen. And at that time was becoming a major concern in the United States and Europe, because there were a number of outbreaks traced to contaminated food products.

The thing about this bacteria is, it not only survives at four degrees refrigeration temperature, but actually grows, so if you have a contaminated product and you have it in the fridge, the bacteria are going to continue to grow, and then when you eat it, you're going to get a heavy dose of it.

So anyway, Dan was going to figure out what virulence determinants enable this bacteria to invade into your human cells and grow inside of them. That was his concept of his lab, and why I got in there early on in the project. He had already made some discoveries when I got there. He had learned that the bacteria invade into macrophages. Macrophages are part of your immune system, and these are cells whose job it normally is to eat bacteria and kill them. So here was this bacteria [that] had turned the tables. It actually targeted macrophages to trigger the macrophage to eat them, and then it grew inside the macrophage and [ultimately kill] the macrophage.

My project when I first got in the lab was to— He'd already discovered one very important virulence factor that allowed the bacteria to do this, and so my first project was to devise a screen or a selection to identify more genes that were essential for this process. What the process is, is the bacteria will come in contact with the macrophage, and the macrophage will eat the bacteria up. It'll ingest it, and when it first eats the bacteria and internalizes the bacteria, the bacteria are inside something called a phagosome, also called a vacuole. Basically, it's the bacterial cell surrounded by a membrane, a bag that's a membrane, and that's floating around inside the cell.

Normally, the macrophage would then inject very toxic noxious chemicals into that phagosome and kill the bacteria. That's its job. But what *Listeria* does is it quickly breaks open that bag, that phagosome, and escapes into the cytoplasm of the host cells, and that cytoplasm is actually a very nutrient-rich, fairly stable environment. The bacteria grow quite happily in the cytoplasm. So that's what *Listeria* does.

At that time he had discovered one major virulence factor important for breaking open the phagosome. So my project was to screen for additional factors that were required for that process, and so I'm working very closely with Dan. I mean, it's hard— You know, I would say that early on, virtually every idea was his, and I was the hands. I made things work. And so, one of the early ideas that I did was to develop a selection to identify mutant strains which had mutations in particular genes whose identities we don't know about, identify mutants that could not break out of the phagosome and escape into the cytoplasm.

The technical description of how that works is probably— I don't know if you want to hear it. It's a little bit complicated.

VAN BENSCHOTEN: Can we have the three-minute version?

CAMILLI: Sure. The three-minute version. So, when *Listeria* gets inside the cytoplasm, breaks out of the phagosome into the cytoplasm, it starts to multiply, and one bacteria turns into two, two into four, and in a few hours the macrophage is filled with a hundred bacteria. That requires bacterial growth.

You can treat the bacteria with certain antibiotics that kill growing bacteria; only growing bacteria. If the bacteria have stopped growing, they're resistant to the antibiotic. An example of that is penicillin. Penicillin only kills growing, multiplying bacteria. So we reasoned that if we— So we mutagenized the bacteria. We created random mutations using a piece of DNA that inserts randomly, called a transposon. We inserted the transposon randomly in the 4,000 or so genes, and then took that collection of mutant bacteria, infected macrophages, then added penicillin.

So the normal virulent *Listeria* would break out of the phagosome and grow, and get killed by the penicillin. The only thing that would survive would be the mutant bacteria that are unable to grow for some reason. Those are the only thing that stay alive, and so at the end of the experiment we just lyse open the macrophages, collect the bacteria and grow them up, and those are mutants that were unable to grow. They're able to survive but not grow, and those were a very interesting class of mutant, because those were mutants that couldn't— Some of them were mutants that couldn't break out of the phagosome. They just sat there in the phagosome, stayed alive, but weren't growing. Others broke out of the phagosome, got to the cytoplasm, but failed to grow there. So we call that a genetic selection.

VAN BENSCHOTEN: Okay. Let me flip over the tape. We'll begin it there.

[END OF TAPE 3, SIDE 1]

VAN BENSCHOTEN: All right. This is tape three, side B now.

CAMILLI: So that idea was really a brilliant idea, and it's totally Dan's [Daniel A. Portnoy] idea. He came up with that idea and I made it work.

It's hard to tell how that influenced other people in the field, but the way I see it, when I look back on the publication record, I think that paper [A. Camilli et al., 1989. Intracellular methicillin selection of *Listeria monocytogenes* mutants unable to replicate in a macrophage cell line. Proceedings of the National Academy of Sciences 86:5522-26] spawned a lot of related ideas in other people's minds, because that was the first of a decade-long period of new genetic screens and selections that were able to reveal things about bacteria during an infection, and

that's what we had done.

We had come up with a genetic selection to identify virulence factors that are important during a real infection in a macrophage. Subsequently, other labs have come up with related things, and I like to think, and I think this is true in a few cases for sure, but I would like to think that this spawned— People saw this paper and started thinking about, "Isn't that interesting? They're actually doing genetics. They're actually learning about their bacteria during a real infection." "Real" in this case is infection of macrophages in a tissue culture plate. Nowadays I would say a real infection is an animal, an intact animal.

But anyway, it turns out that that was very instrumental in my future, because I started thinking about—and have continued to think about—how do we study the bacteria during real infections, and that's basically what we do right now. I came up with some other methods, came up with my own ideas later on that have turned out to work well for looking at, studying the bacteria during real infections. So that was a very important thing for me early in my education, was to work with Dan to get that thing published.

Now, from there we moved on to other projects. The next project really formed the meat and potatoes of my thesis. The next project was again to mutagenize the bacteria with a transposon. So the way that works is you just add the transposon to the bacteria and it will randomly insert somewhere in the genome, and where it inserts, if it inserts right in the middle of a gene, it knocks the gene out. The gene's not functional. That's a mutation, transposon insertion mutation.

So the next project I did was to do a transposon mutagenesis screen. Now, let me see if I can get this right. Oh, yes, okay. As I told you before, we infect macrophages. This new project we did a transposon mutagenesis of Listeria, and then added them to what we call a monolayer of cells. In this case these were epithelial cells, and a monolayer means they grow as a single, uniform layer on the surface of a plastic dish, and overlaying them is a nutrient-rich media that keeps the human epithelial cells alive and growing.

We add the bacteria to these cells, and occasionally one of the *Listeria* will invade into a cell and multiply and kill that cell, and the bacteria released from that dead cell will infect the neighboring cells, and what you get is an expanding circle of death of cells in that tissue culture plate. You can then, later on, stain the monolayer and actually see these little holes where the bacteria have caused a little focus of infection, we call it.

So the wildtype bacteria, when you add them, and you'll see these little circles. They are growing out and so they get bigger as the days go by, as it kills more and more cells. So the diameter of the circle is a measure of the virulence of the strain that's causing that focus of infection. We actually call these things plaques. These holes in the monolayer are called plaques.

So we did a screen for small plaque-forming mutants. So we mutagenized the Listeria with this transposon, infected monolayers of epithelial cells, let the bacteria infect for three days

or so. Then we stain the monolayer and we look at the plaques. Now, most of these plaques, these circular holes, were about the same size. Wildtype virulent *Listeria* would cause a plaque of, say—I'll just make up a number—say four millimeters in diameter.

Rarely, maybe one in a hundred to one in a thousand plaques would be slightly smaller. Some would be even very tiny. Those are mutant strains which grew very slowly. They suffer some defect in multiplication or spreading from one cell to another. So we did that screen. That's called a screen because you have to look at, you know, thousands and thousands of plaques to find mutants. And then you just take a toothpick and touch that right around the edge of that small plaque, and you recover the bacteria, grow it up, and do experiments on it.

So we did a small plaque-forming mutant screen, and got— That was a goldmine. We got lots of interesting mutants. Interestingly, the rest of my thesis project resolved around a mutant that formed a slightly smaller plaque. It wasn't a tiny, tiny plaque. It was just a little bit smaller than the wildtype size. So this was a gene that either played just— It wasn't an essential function, but played somewhat of an important role, but not essential in virulence by this assay.

So, the other thing I didn't tell you is once we have a mutant strain with an interesting phenotype, we can then go in and use some genetic tricks to find out exactly what gene was mutated. We can go in to find exactly where the transposon landed, because we know the sequence of the transposon. So to make a long story short, we'd go and find what gene was mutated, and for that particular strain the transposon had landed in an enzyme, the gene that encodes an enzyme, and this enzyme is called a phospholipase. This is an enzyme that degrades lipids. Lipids are the central component of membranes.

So this immediately struck us as, "Ah, this is interesting." Here's a lipid-degrading enzyme, and *Listeria* needs to degrade the lipid membrane that makes up the phagosome in order to escape into the cytoplasm. So it turned out this was a very interesting gene, and the gene lay in a set of genes that were all involved in virulence, and that was the rest of my thesis project, was characterizing that gene and other genes.

To make a long story short, this phospholipase lay in what we now call a pathogenicity island. So, most of the genes in any bacteria are just what we call housekeeping genes, just needed for replication and growth. Genes that are specifically involved in virulence, sometimes they're grouped together in this big array of genes—back-to-back genes—all involved in virulence, and we call those pathogenicity islands. So the gene I found lay in a pathogenicity island.

I mentioned earlier, the major virulence factor that Dan had found was a protein needed to break out of the phagosome. Turns out there're three proteins that function together to break out of the phagosome. There's the major one, called a hemolysin, because if you add it to red blood cells it lyses them, so, hemolysin. That's the major one. That's absolutely essential to break out of the phagosome.

Then there are two lipases, the one I found and another one that lay close by in this

pathogenicity island. Those two facilitate the process, speed up the process. They help Listeria get out of the phagosome faster and more efficiently, and actually that's critical during a real infection. The tissue culture cells that we grow up on a plate are very wimpy macrophages. They barely can kill nonpathogenic *E.* [*Escherichia*] *coli*. In our body, when a macrophage eats a bacteria, that macrophage is very— We call them angry macrophages. They rapidly will kill the bacteria. So *Listeria* in the real situation has to get out of that phagosome fast.

VAN BENSCHOTEN: Before the toxins descend.

CAMILLI: Before the vacuole becomes acidified and before degradative proteins are injected in there and kill it. It literally has a matter of minutes to get out of there, so the faster it gets out, the better. So these phospholipases function to increase the speed with which they break out of the phagosome.

I did some pretty cool experiments characterizing this phospholipase, and I should mention that we worked with a cell biologist called Lou [Louis G.] Tilney, who was very important in my work as well as Dan's work. He was a genius with microscopy. In our case, we worked together and did electron microscopy. So you can actually see the bacteria inside the phagosomal membrane when you look under the electron microscope, and then you can see the bacteria that are free in the cytoplasm, and you can measure those two. So, working with Lou Tilney's group, I was able to do statistically robust sort of measurements of the wildtype *Listeria* and my phospholipase mutant, and that's how I was able to determine what I just said, that it facilitates escape from the phagosome. So we could measure a difference, a real difference between the wildtype and our mutant.

Then another test one always does—if one has a good model for it—is infect an animal and see how does your mutation affect the virulence of the pathogen in an animal model. This phospholipase mutant was a couple orders of magnitude more avirulent than the wildtype. So like I say, in a real animal, getting out of the phagosome fast is critical, so this mutant had a very strong phenotype in an animal.

So that was that project, and that really was the end of my— Most of my thesis came from that gene and the surrounding genes. So, next to that gene was a regulator of transcription of all the other virulence genes in that locus, and I characterized that regulator. To this day, one of the major things I do is study virulence gene regulation, because it turns out that these virulence factor— Bacteria are very smart. They don't express genes if they don't need them. So if you're growing up *Listeria*, say, in a test tube out in the lab, it's not making any of these virulence factors. They're hidden from you. You don't know they exist.

What the bacteria do is, during a real infection they sense, they receive signals that say, "Ah, you're in the host. You're in the bloodstream," or whatever. "You're in a macrophage. Turn on X, Y, and Z genes." So, studying virulence gene regulation is very important. Part of my thesis was to study a virulence gene regulator, which lay right next door, actually, to the

phospholipase gene, and that was needed to turn on expression of the phospholipase. So those were the successful things.

There were a lot of unsuccessful things, like at the end— So, a few genes away from my phospholipase turned out to be a very, very important virulence factor that turned out to be a tremendous find later on for another group. I was just sequencing around my phospholipase gene and actually started sequencing into that gene, and one day I brought Dan my sequence and I said, "Look at this. Here's another gene next door, very interesting sequence." It had a repeated amino acid sequence, and I said, "I'd like to continue sequencing into this and see what this thing is, and maybe make a mutation."

And Dan said, "No. We're not going to waste money just sequencing willy-nilly." Turns out that this gene is a very critical virulence factor, and then we missed it, just by chance. So, not everything we did was successful, and we had other things that failed as well.

VAN BENSCHOTEN: How did you finish up, though, in the Portnoy lab? Because I know you have an important brainstorm near the end of that.

CAMILLI: So, you may have to remind me what that is. Let's see, what did we do at the end? Oh, with reference to my postdoc[toral] project.

VAN BENSCHOTEN: Right. Because when reading some literature on you, I have, in one account it was something that came to you—

CAMILLI: Right.

VAN BENSCHOTEN: [Unclear] like in ten minutes, and you scribbled it down quickly and it became—

CAMILLI: I'll tell that story. I'd say about a year away from finishing up, I had to start thinking about what I was going to do next. Typically what one does is go into a postdoc, go do research in another lab working on a different system. And so Dan came and said, "You need to start thinking about your postdoc."

And I said, "Yeah, yeah, I know." So I started doing some reading and it wasn't— It was still quite early then. It was more like a year and half before I finished, so I hadn't really identified any groups that I wanted to work with yet. But anyway, that summer I went to a meeting. So, that's one thing that was great about Dan, is he sent me to meetings.

My first year in his lab, when I had no right to be giving talks, he sent me to give a talk at the ASM [American Society of Microbiology] meeting. He was supposed to give the talk, and like two weeks before he said, "I can't go. Why don't you go give my talk." I was frightened to death, but I went and gave it, and just, you know, was pushed right into the water. So, anyway, he sent me to lots of meetings, and that was great because I got to meet people.

So that summer I went to a Gordon Conference. There's a Gordon Conference that's really one of the most popular meetings for people in my field. The Gordon— There are hundreds of different top conferences on various things, but there's one specifically on pathogenesis called toxins and microbial pathogenesis, I think. So I've gone to that virtually every year since I've been a graduate student, and that summer it was the third time I'd been to it, and Dan started introducing me to some potential postdoc lab people, and he introduced me to John [J.] Mekalanos.

Oh, I'm telling the story wrong. A few months prior to going to that meeting, John Mekalanos, who will turn out to be my postdoc mentor, advisor, came to give a seminar at [University of]Penn[sylvania]. Dan invited him down. So John Mekalanos is at Harvard [University] Med[ical] School. He came down and gave a seminar on Vibrio cholerae, and I thought, "This is cool stuff." And Dan was John's host.

John Mekalanos likes to have a good time, so he said, "Dan, how far—." I was in Dan's office. "How far away are we from Atlantic City?"

Dan said, "Oh, I think just forty-five minutes."

He said, "Let's go. Let's go spend my honorarium. Let's go gamble it away."

And Dan said, "Okay. I'll go get Phil [Phillip] Youngman next door, and we'll go."

So he came back and said, "Okay, Phil Youngman's in, but we need a designated driver."

And so Dan turned to me and he said, "Want to go to Atlantic City?"

And I said, "Yes. I'll be—."

And they asked, "Will you be designated driver? No drinking for you."

And I said, "Sure. I'll do it, but I would like to get a little money from each of you, gas money and maybe a little gambling money." So they each gave me forty bucks.

VAN BENSCHOTEN: Tough bargainer here.

CAMILLI: Yes. So I had a hundred and twenty bucks, minus twenty for gas. I lost it all in like

ten minutes, gambling. But anyway, we had a great time. The drive over and back was fun, hearing those guys chatter, and Dan and John were good friends. So anyway, that was the first time I met John. I was this grad[uate] student.

Then the following summer, saw John again at this Gordon Conference, and we hit it off. He came to see my poster on my work and thought I was doing good stuff, and invited me right then and there. He said, "You want to come to my lab? Come do the proper interview, give a seminar, etc., etc."

And so a few months later I went and gave a talk, and we clicked. Then I still had another eight months or so before I would show up to John's lab, and wanted to come up with a project idea. John gave me some ideas, but I wanted to come up with my own, and typically that happens. I wanted to write fellowships prior to my arriving so that when I showed up on day one I had my own money. So I started brainstorming on ideas.

Now, the day I'd visited and given a seminar there [Harvard University Medical School], I heard about a very exciting project being done by two postdocs in his lab, and it was unpublished at that point. They had come up with an idea which is now called IVET, in vivo expression technology. This goes back to that selection that Dan and I had invented three or four years earlier, studying the bacteria during a real infection.

So these two postdocs in John Mekalanos's lab—their names are Mike [Michael] Mahan and Jim [James M.] Slauch—those two had come up with this idea with John. The three of them came up with this idea called IVET, and what it is, is a genetic selection similar to the one Dan and I had invented, but its goal—its objective—is to identify bacterial genes that are turned on during the infection. So they told me about their work.

You know, we had like a half hour and I was at their bench, and they were both talking to me and telling me about their, on paper, their idea, and I just thought it was beautiful. I appreciated it immediately, because I had never thought about how do you find genes based on their transcription, genes that are turned on during an infection. I'll elaborate on that in a minute, but anyway, they told me about their idea and I had my first brainstorm right at that moment.

Well, I guess I have to describe it right now. So, what they came up with is a positive selection. What they do is they have a gene— No. Let me go back. They create a mutation in Salmonella that makes it so that the bacteria can no longer synthesize one of the DNA bases. So, obviously, the strain can't grow. It can't make DNA. But they can add the gene back in and complement the defect—it's called complementation—and then the strain can grow. So they have a mutation in this critical gene and then they can add the gene back and get complementation.

So, what they reasoned is they could take this gene, this complementing gene, and delete its promoter region. The promoter is the segment that says when to turn on the gene, turn on transcription of the gene. They deleted the promoter, so if they add the gene back, it doesn't complement, because it's not expressed; it's not turned on. They take this gene and they insert it

randomly in the chromosome of the Salmonella.

Occasionally they'll insert just downstream of some other gene, and it'll then be expressed at the same time as the gene it landed next to. If that gene is turned on during the infection, it'll then turn on the complementing gene that was inserted right next to it, and the bacteria will survive and multiply. If the complementing gene lands somewhere else that is not expressed during the infection, the strain dies.

So again, this is a genetic selection. You infect animals with a bank of strains where this thing is inserted randomly in the genome, let the infection go, and at the end of the infection collect the bacteria back out and most of the bacteria have died. Only the strains where the complementing gene inserted downstream of another gene that's on during infection survive.

So, many of those genes are just genes that are on all the time, and we don't care about those. They were after genes that are off when you grow them up in the laboratory, but are then turned on in response to the host. The premise, the idea here is that those are probably important for the infection, because I told you earlier bacteria don't express genes when they don't need them. So, that was their project that they had come up with.

And as they were telling me this, I said, "Well, in order to complement, the gene has to be expressed pretty much throughout the whole infection, and at a fairly high level in order to produce enough of the gene to allow growth."

And they said, "Yeah, you're right, and that's actually a problem."

So I said right then and there, "What about another idea? What about finding genes that might be turned on only at one stage of infection?" So, transiently expressed genes, or maybe genes that are turned on throughout the infection, but at a low level of expression. Because it was obvious that those two classes are also going to be very important, potentially very important virulence factors.

And they said, "Yeah, that would be nice."

And I said, "What about a genetic switch reporter?" So, this would be a reporter gene that you just need a little bit of expression and then it causes a permanent change, heritable change, so this would allow you to find transiently expressed genes, or genes that are expressed at a low level. And they both looked at me when I said that, and I know in retrospect why they gave me that look. It wasn't, "Oh, isn't that a brilliant idea?" look. It was a, "How did you know that we're actually thinking of doing that in the lab?"

So then that was, I guess, early summer. No, that was in the winter, and then the following— A few months later I was at another meeting, and this was a meeting that I didn't care too much about. I forget what it was, but— No, I did care. I did care what it was about.

I started to develop this idea of a genetic switch reporter of gene expression. It was just

an idea, and I was thinking about it. I remember taking a very long walk at this meeting. Usually these meetings are very social. You're either at talks or at posters, or you're drinking and eating with other scientists, and seeing your friends and making new friends, etc. But that meeting, I decided to take a long walk, and I walked for like three hours, just thinking, brainstorming. And I came up with an idea of this. I really developed this genetic switch reporter idea.

That same evening I went back to the meeting. I was at the poster sessions and I ran into my former neighbor, Doug [Douglas] Berg. He was in the lab right next door to Dan Portnoy's lab at Washington University, so I only knew him from the rotation I did in Dan's lab there. Dan and he actually didn't get along so well, so there was a little bit of a strange feeling in the air.

But I met him and talked a couple of times with him while I was there, and I knew what he did. He was like one of the world's experts on transposons and DNA recombination. So I—just fortuitously—saw him at the meeting that evening, and I, over a beer, said, "Doug, I have this idea, genetic switch reporter. My idea is to, instead of having a promoterless complementing gene, have a promoterless site-specific DNA recombinase, so that when it gets turned on, it goes and causes a permanent change in the DNA. So it marks the cell and its descendants, and reports that the gene had been turned on." I'll try and describe that better in a minute.

But I didn't really know much about DNA recombinases, and I said, "Doug, here's my idea. Do you have any suggestions for— Is there a recombinase that fits the bill here?"

And he immediately said, "Oh, yeah. You want to use a site-specific DNA resolvase." That's a particular type of recombinase enzyme.

I said, "A resolvase?"

And he reminded me, because I'd learned, probably from him in a course at Wash U. [University of Washington, St. Louis], what a resolvase does, what its role is during transposons hopping. Bottom line is: just an enzyme that recognizes a specific DNA sequence that's duplicated, and it causes a recombination reaction between the duplicated sequences and excises out the intervening DNA, gets rid of it.

So I went, after the meeting, went back, read some papers on resolvases, and said, "He's absolutely right. This is going to work. Here's my idea." I formulated it. A month later, I went to talk to John [Mekalanos] about specifically what project to write about for my fellowship applications, and sat down, started telling him, very excited about my idea. And I just saw the look on his face. He was like— You could see the excitement welling up, and he just wanted to butt in and butt in, but he waited and he let me finish.

Then he said, "That's a great idea." He stood up, grabbed a grant that he was writing off of his thing, and said, "We have the exact same idea." There were some slight differences in what we chose to use, but he said, "I have the exact same idea, and not only that, I have two grad students in the labs who, we've just sent away for some vectors and we're going to start

developing this thing." And he said, "Perfect. You write about it for your fellowship."

So I wrote about it. I actually got a fellowship. I got a Damon Runyon-Walter Winchell Cancer [Fund Postdoctoral Fellowship Award] Research Fellowship. Obviously I'm not studying cancer, but they fund a few other projects that aren't directly related to cancer.

So anyway, that was all fine. So I show up to the lab maybe four or five months later. No, it was longer than that, maybe six months. And sure enough, John had a senior grad student [David Beattie] who was helping a new grad student [Su Chiang] to work on this. It was kind of a back-burner project. She was working on another project, but she had done some stuff, gotten these vectors in, and started creating constructs to get this idea working, and so I was very fortunate that they gladly relinquished the project to me, said, "Okay. We're getting some things done here, but it's not really flying. Take it. It needs someone's full-time attention." That was Su Chiang. She was the new grad student working on it, and David Beattie was the senior grad student, and so I have to credit them. They both, you know—John's lab, maybe it came right from John's head, probably—came up with this idea independently, and he and these students actually got it going. They got stuff going ahead of time.

So I came in with vectors and stuff already there. I just had to assemble a few things together and make it work. And it did; it worked right off the bat. I should also say that this exact same idea arose a third time in another lab, totally independently, so it was not— I wouldn't say it was an obvious idea, but based on these prior things like my and Dan's paper on the selection for *Listeria* genes *in vivo—in vivo* means during infection, when I use it—as well as this brand-new IVET technique from John's lab, this got people thinking about these things, so in a way it was kind of obvious to think about a recombinase reporter system.

So I jumped in and started up on this project. Now, I don't know if my description, my multiple attempts at describing that were clear. Do you have—

VAN BENSCHOTEN: I think I do, but I also have just recently read a description of it on your lab, and then also the paper itself [A. Camilli et al., 1994. Use of genetic recombination as a reporter of gene expression. Proceedings of the National Academy of Sciences 91:2634-38.] which I believe comes about in '94. Is that when all of this sort of comes to fruition?

CAMILLI: Yes.

VAN BENSCHOTEN: You published a paper in PNAS [Proceedings of the National Academy of Sciences]. So I think I do. I think I have a pretty good idea.

CAMILLI: Okay.

VAN BENSCHOTEN: It's a technique, though, that as you say is going to be sort of used in one form or another, then, in a lot of different other fields, isn't it also?

CAMILLI: Yes. So, even before I had my idea and John had his idea, resolvase enzymes, these DNA recombinases were being used in the development field, eukaryotic development. People realized that here's an enzyme that's going to specifically go and chop out whatever piece of DNA you want. All you have to do is flank that DNA with the recognition sequence for the resolvase enzyme. So people were already doing this in another field.

I had no idea, you know; I don't read any differentiation papers. But people are already doing this, using this enzyme system in another field to specifically knock out genes when and where they want, in a cell lineage-specific manner. So, I guess, had people in my field been reading differentiation stuff, they might have known about this years before and come up with the idea themselves.

But anyway. So the third person that came up with it in our field was Russell Mauer at Case Western [Reserve University]. He came up with the same idea and used a different resolvase enzyme and recognition sequence, but set it up as well. I learned that when I was a second-year postdoc, went to a meeting and first day walked into the poster session. First poster by the door was a graduate student of Russell Mauer's, with this recombinase-based IVET technique, right out there in black and white, the exact thing that I was doing, but in a different bug, in a different pathogen. As I read this, I experienced a bout of paranoia and talked to the student, who was very nice. And I said, "Where are you guys at with this?

And he said, "Well, we've just gotten these data. We haven't even started writing anything up."

I had already written mine up and submitted it. So I tend to be quite honest, and maybe too honest sometimes for my own good, but I told, you know, as typical of me I said, "Oh, well, I'll be honest with you, I'm doing the exact same thing, and we already have a paper submitted to PNAS." Then it was his turn for his heart to drop out. He knew he was about to get scooped. And I said, you know, "Sorry about that. Apparently you've come up with this idea independently."

We talked for a long time, and as is often the case, it's very hard to really scoop somebody in our field. He was using a different system, doing it in a different bacteria, and will come up with obviously different genes, different bacteria. So we both agreed, no one's really scooping anybody. We've come up with this independently, and so it was fine.

VAN BENSCHOTEN: How much oversight did John Mekalanos have on this project and other projects that you did during your postdoc?

CAMILLI: So, he was much less hands-on than Dan, but he had a much bigger lab. When I was in the lab, he started up a company, so he was— I have to say that I was always surprised at how much interaction there was with John, considering things he was involved in. You'd spend maybe two hours with him a week, or he would be available say two hours a week, on average. So you'd go in and spend a half hour with him every week or so, and tell him your latest data.

John also, a brilliant mind. He knows all the projects that are going on in his lab. He's constantly thinking about them as he reads other things and learns about other science, so when you come in his office, he's right on top of it very quickly. You tell him your results and he incorporates it and spits out brilliant ideas, often too many ideas. So one trick that pretty much everyone learns in the lab is to let him give all these ideas, then you go back to the lab and decide which one you're going to work on. Don't commit to anything. If you're not able to do that, if you do everything he says, you'll just be lost.

So, yes, John, very brilliant guy, great ideas. So I would say, you know, clearly, making the project happen was totally me. I did everything. Other than getting some constructs at the beginning and getting a few things made by these two grad students, I did basically everything by myself.

VAN BENSCHOTEN: What was sort of a typical routine for you? I should have asked this earlier, too, when we talked about the Portnoy lab. But when did you usually go in? When did you usually leave?

CAMILLI: Yes, I'm not a morning person. I would generally get in, say, nine-ish every day. One of the great things about this field is you, there's no— You're not punching in a clock. You really set your own time, and if you're not interested in what you're doing, you're not going to work many hours. I was very interested in what I was doing, so I was working probably sixty hour weeks, seventy hour weeks as a grad student.

I'd go in at nine and generally stay until seven or eight, and almost always would go in on weekends. When my girlfriend [now wife Kristen M. Camilli] moved to Penn, I started cutting out weekend hours. I typically would go in one day, either Saturday or Sunday. So I cut down on my hours when she came out, because we'd generally spend one day together. But then when I got back to the postdoc, I worked really hard. Again, I was working like sixty-hour weeks, and typically the same thing, going in at nine, staying till seven or eight, and almost always going in on weekends.

There was one difference between John's lab and Dan's lab. Dan's lab, when I would go in, I basically would see him, during the early years. In the later years more people were in the lab and there was a little bit of a group there I socialized with. But John's lab was incredible in terms of the group of people that were there, a big group of very bright people coming from all over the country, and just socially I had a great time. Maybe the best time of my life was those

three years in John's lab. I made some really lifelong friends, and really cemented my— I mean, really I was committed at that point to this field, but really cemented into my head that this is a great group of people in this field; not just the lab, but other people I've met, and were doing incredibly interesting stuff. You know, you're going to be happy, and I have been.

VAN BENSCHOTEN: How do you finish up at the Mekalanos lab, and then how do you make that transition, then, to your own lab in '95?

CAMILLI: You know, I was lucky. It's rare to do a three-year postdoc and get a job.

VAN BENSCHOTEN: It is.

CAMILLI: So I got things working right away, and got some papers.

VAN BENSCHOTEN: Did it surprise you that you got this first project to work the first time around?

CAMILLI: Yes, it did.

VAN BENSCHOTEN: I would imagine. It's fairly complex.

CAMILLI: It surprised other people in the lab, too. You know, when I showed up to lab meeting with data and said, "Look. This works," people were shocked. And John was very excited. He was very excited about this. So, yes, I was very happy. So I got a couple of papers, and with a couple of papers one can technically, theoretically get a job.

So I have to say that by my third year I realized— After two years were up, I realized, although I'm having a great time here, I'm really not going to learn much else from John. And I thought, "I want to start looking for jobs." My reasoning was that— There are two things. One, I had a three-year fellowship, so I just thought, why not, you know, the day my paycheck ends, have a new paycheck from another place. So in other words, not hit John up for money. Even though he had probably tons of money, that was my thought, "My fellowship's up. It's time to get a job."

The second reason is I realized I'm really not going to learn much else from John. He gave me tons of attention, lots of ideas, but he had gotten busier and busier, and it was to the point where I really didn't think, you know, I'm really not going to learn much else. So I

thought, time to start looking for jobs. And if I don't— In any one year there may only be a couple of jobs available, so you may take multiple years looking to find something that's suitable.

So I thought, well, let me start looking for jobs this year, and if I don't find anything suitable, I'll look again next year. So I was willing to do another, fourth year in John's lab, and he said he would be happy to have me stay. He agreed with my plan. Other people, including Dan Portnoy, very strongly argued to not start looking for a job yet, to stay another year and just develop my system, make it as strong as possible so that I'd come out with blazing guns; get a grant right away, have lots of preliminary data. And I argued back, "No. I want to start looking for a job." And Dan was very miffed at me, I think, for doing that, because as you become assistant professor and then associate, you look back fondly on your postdoc years as a great time where you have no responsibilities other than doing research. So they're right; that is the time to really build up your system and have a running start when you start competing for grants.

But I started looking for jobs and got a good offer here, at Tufts [University School of Medicine], and my wife had a good job around here, so it made perfect sense. And there was definitely no guarantee that I would have gotten a job the next year, here, somewhere in Boston. Also, this department made, I think, a special effort to get me. One of the professors here, Ralph Isberg, knew me well, and really made a strong play to get me here. They made me a very good offer. He's a great colleague, so it made perfect sense.

So in a way, he went out and got someone a little premature, and knew he was getting someone a little premature, but was willing to take the gamble and have someone start off a little bit slowly and build up their system. And it's all worked out. His vision was correct. So, yes, I left after three years in John's lab, and started up here. And basically, John let me take my entire project with me, and there are some stories in there that we can get to.

VAN BENSCHOTEN: Good.

[END OF TAPE 3, SIDE 2]

VAN BENSCHOTEN: This is tape four, side A.

What were the other places that you applied for a position?

CAMILLI: I applied at Emory [University] in Atlanta. I applied at University of Alabama at Birmingham, which is kind of a weird place for two northerners like my wife and I, but actually they have a very strong microbiology department down there. I applied here at Tufts University School of Medicine.

I applied at the University of Hawaii, and I actually got a job offer out there. That was one place my wife [Kristen M. Camilli] said, "No way. I'm not living out here on an island in the middle of the Pacific." And I had no idea. Really, I mean, the conditions out there for doing research were horrible. I mean, everything's super-expensive to get it there. It takes a long time. The lab was not in that good condition, so it turned out to be a definitely, absolutely not.

So let's see, where else? I applied at a few other places, like U Mass [University of Massachusetts], Worcester, and University of Michigan [Ann Arbor], which was where I got my undergraduate degree. I was kind of hoping to get a job there, because my wife and my family are from there. So I did not get a job offer from them; I was their second choice.

VAN BENSCHOTEN: In spite of Carl [F.] Marrs.

CAMILLI: In spite of Carl Marrs, yes. I don't know if he was pulling for me or not at that time. As I was told, they wanted a cell biologist and they had a very good—There was a very good cell biologist applicant that year. They knew that I would be a good fit in terms of having family there, and that I would take their job offer. Usually that's a very strong thing, but I did not get an offer from them.

So, anyway, it boiled down to Emory and Tufts. Emory had a pretty good graduate program, good lab situation down there, and Atlanta was a pretty nice city, but my wife clearly wanted to stay here with her job and didn't really want to live in Atlanta; much prefers Boston.

VAN BENSCHOTEN: And what was the startup package that they gave you here at Tufts?

CAMILLI: It was similar to the Emory. They kind of matched Emory, which back seven years ago—actually, eight years ago when the offer was made—was about, if I can remember it, it was like $250,000 startup. I could be off on that. I actually have a documentation with that on it. So the offers were equivalent.

It was roughly you were going to be expected to get at least 50 percent of your salary from grants, although, you know, we'll give you a couple year leeway till you get grants. First year, no teaching, which is a very critical thing, and then the startup package, which were roughly equivalent. You know, I think I'm wrong on the 250. I think it was lower than that. I think it was like 150 or 175 back then. It's odd that I don't remember something as important as that. But the other thing is the lab space. That was critical. Emory had a really nice lab space, and Tufts was good, but didn't match Emory.

But the other factors made it a clear choice. Tufts matched them in terms of my salary and startup package. The salary was like 55,000, something like that. It was pretty decent. It was

the going rate back then. There's really an inflation in startup packages, so when I hear about what people get now, I just can't believe it. And apparently that's always been the case. But anyway.

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

CAMILLI: There was nothing really hard. It all came pretty quickly, and I think I was very resourceful and very determined to get it going, like I had experiments going within a week of getting here. Other people would have dilly-dallied and taken their time.

I started in the end of July, I think, and when September rolled around, I had experiments going, and I asked to have rotating students start their rotations so I can get a student the first year. Some of the faculty were a little concerned that I didn't— You know, a lot of my equipment hadn't come in yet, and so we agreed that when the rest of my equipment comes in, I can start getting rotators. So I ended up getting rotators and got a student that year. So that was a major concern for me: I don't want to be stuck in this lab with me and a technician for, you know, another twelve months. I want a grad[udate] student. And so I was a little disappointed in my colleagues for even suggesting that I wait for some stupid piece of equipment to come in. And to this day, I think I'm right.

Anyway, I ended up getting a student, Sang Ho Lee. He was my first graduate student, and he was tremendous. Actually, I had two students interested in coming to my lab that first year. So one thing I feel I've been very fortunate with is getting good students, and always, each year, students wanting to come into my lab. I think being young and, you know, with it, whatever, is part of it, like they know I was trained in the latest techniques or whatever. But I think personality, I'm young; they can identify with me more. But I always have lots of students who want to come to my lab, so that's always good, because you've got to get students. There are many more labs than there are students, so there's competition going on there.

So actually I had two students interested in my lab that first year, but one of them didn't particularly like the other student and said, "Well, I think you're going to have to choose between us." That made my decision easier; it was the other student. My biggest concern was getting a student. And then the research started rolling. So I, again, am fortunate to be working in a field where you can do experiments in twenty-four hours and get the results. The bacteria grow very rapidly, very fast, so it's not like if I were in a cancer lab or something where each experiment takes months. The turnaround is tremendous, and that's good for my personality, because I need stuff coming in. And I'm not particularly careful. I'm not very diligent in— You know, I don't have perfect hands. I tend to do experiments a couple of times until I get them to work. Often I'm spinning my wheels, but I can do it because the research is such that you can just keep going, repeating it, and improving the experiment each time. But anyway, so, things started working right away.

So, my other big concern was getting grant money right off the bat. My first grant attempt

failed, so that has ruled my— When I think about negatives of being a professor, it always boils down to that: the competition for grants. No matter what your thinking is, you always have to go back to the math. One in five grants gets funded, on average, so there's no way those other four grants are ridiculous ideas. Many of them are very good grants. So, there's a lot of politics that come into it; there's a lot of proving yourself. You've got to have papers. My first grant didn't get funded, and one of the comments was, "Well, he hasn't proven himself yet. He has all these papers with John [J.] Mekalanos's name on it."

And I submitted the grant like two months after getting here in my lab, so I just thought that was just the most ridiculous thing there is. But I realize in retrospect that in a way I'm now at the end of the queue. I have to wait my turn. No one's going to give me a grant first time in, when they know I just got a startup package, etc. Unless my research was like, you know, so fantastic that they couldn't justify not giving it to me, and it wasn't like that.

So, I didn't get that first grant and was really pissed off about that. So my second time in, I had a very strong— So, when you resubmit, you can put in a three-page response to criticisms, and I put in a very strong thing that, I was very polite and—what's the word—respectful, but at the same time challenged some of these things. Like the thing about John being an author on my papers is just ridiculous, so I got it the second time, and was then quite happy.

Unfortunately, that grant, the type of grant I got was a small grant specifically for new people. It's called a First Award or an R29, NIH [National Institutes of Health] grant, and they've done away with them because they're basically useless. It's only enough money for one student. Back then it was, you know, half my salary and a student, but nowadays it's worthless, so they did away with it about two years ago. So my R29, my First Award, just ended about a year ago, and it was 70,000 a year in direct costs, and then some, you know, 60 percent over and above that goes to the university.

So I, at that point, started applying for a lot of different things, and the next year got the Pew [Scholars Program in the Biomedical Sciences] grant. And that was perfect, because the Pew [Scholars Program in the Biomedical Sciences] grant is also not much money, but together with the R29 it made it like a real amount of money. I had enough to run the lab, to take another student or two.

VAN BENSCHOTEN: It came without strings, right?

CAMILLI: It came without strings. Which is very important. So then I was rolling, and got a couple more students. I think I took two students the next year, and got a postdoc [toral fellow].

VAN BENSCHOTEN: So you're really picking up speed at that point.

CAMILLI: Yes, picking up speed because we had lots of ideas but no hands to do these things. Then I think another two years after that, I got my first big grant, an RO1, which is, at that point it was 175,000 per year in direct costs, so then you can have a full lab. So for a while there I had lots of money to do our research. I had the RO1 and I had the R29 and I had the Pew [Scholars Program in the Biomedical Sciences grant].

The Pew [Scholars Program in the Biomedical Sciences] was also very instrumental in that, as you know, the meetings they have are fantastic. Meeting my peers, some in my field but mostly in other fields, but meeting with them and hearing about their research, because they're all new, just getting going, very exciting ideas, those meetings were just fantastic. Best meetings I've been to in my life were those Pew [Scholars Program in the Biomedical Sciences] meetings.

It really, in addition to actually being a learning experience, it was also very confidence-boosting, going to those Pew [Scholars Program in the Biomedical Sciences] meetings, because you'd see other people who maybe you've seen that they had a Nature paper or some big paper. Then you meet them and talk to them and you realize they're at the same level you are. They're new, worried about getting grants, learning how to mentor students in the lab, and it just made me feel like I was in there, you know, a scientist. I'm doing work that's, you know, good. So anyway, those meetings were great.

VAN BENSCHOTEN: To sort of get back to funding, how much are you concerned, in your day-to-day interaction in the lab, with funding issues? Sort of like on a scale of one to ten, I mean, how much do they really [unclear]?

CAMILLI: Well, it's gone in phases. I can tell you that my first year it was a six or a seven. I was very concerned. Even though I wasn't expected to get anything right away, and could have gone two, maybe even three years before I got a grant, I still felt I've got to get one as fast as possible. So that was a concern my first year. Then the next six years, a one or a two. It was no concern. I knew how much I had. I knew my lab personnel and I was constantly growing by one person a year, and I knew I had the money for it, so it was not a concern.

Then a year ago my Pew [Scholars Program in the Biomedical Sciences grant] ended and my R29 ended, and a year prior to that, I had written two new grants. I knew that when those two grants ended I would only have one grant left and I would be running a deficit. So I would say two years ago, roughly, I submitted two new applications, and then funding was looking over my shoulder. I was concerned about it because both grants were new projects, new grants. What are my odds of getting them? Probably pretty slim on the first shot, first try, and sure enough I did not get either of the two grants that first time in. Then my Pew [Scholars Program in the Biomedical Sciences grant] and First Award ended and then—boom—deficit. So then I would say concern over funding jumped back up to like a seven.

I went and explored my options with my chair and the department manager, and that's

one thing, also, is this department's fantastic for that, the support. You know, they knew this was coming. They knew what grants I'd submitted, and when I got my scores I went in and we sat down and figured out some things. So the department has given me money to keep going, so I've needed a little bit. I was running a deficit when it came to salaries and supplies.

What they did is they put one of my students on a training grant and helped me out that way, and shuffled some money around in some other way so that my internal bills weren't so high. So I was running without a deficit, it turns out, until I made something, until I could revise these applications and send them back and then get one. Those revised applications are going to be reviewed in a month, so I still need one of those.

In the meantime I got a one-year bridge award from NIH, so as of a month ago my number-seven-level concern has gone back down to like a two, because now we have lots of money. Now the student's been taken off the training grant; my internal bills are back to what they should be; but I have lots of money. And this one-year bridge award from NIH is a fantastic thing because you can roll over unspent, so it's actually going to keep me flush for two years. I'll have no concern for two years, so even if my second submissions fail, I can go back in a third time and get them. I'll have the chance to go in a third time.

If by that point I haven't gotten either, then I'm back to a seven or an eight, because I should have, at my point in my career, I should be able to get another grant. I feel my odds are pretty strong of getting one or both this time in, because we've answered— We've waited our turn. We've answered the criticisms. We have lots of new data.

I'm trying to break into a new field, a new area that's unrelated to anything I've done in the past, and to do that, there are two things: You've got to have a lot of preliminary data, and then you've got to get your foot in the door. These people don't particularly want you competing for funding with them, and it's not a very friendly field, this one I've entered, but I'm determined. We've done some good work and we have some good ideas to— I think this field needs us in it, to speak unabashedly. Is that the phrase?

VAN BENSCHOTEN: Maybe this is a good point then to do sort a brief overview of the research that you're doing now in your lab that these grants are tied to.

CAMILLI: Okay. The first few years in the lab I took and developed further that project that I came up with and that John Mekalanos came up with, and that we both did together, and that's this recombinase-based IVET [in vivo expression technique] method. And this is really my own doing. I came up with a couple of ideas of how to extend this. First, I asked the biological question, how can we learn about this? And then I came up with in my own mind a way to adapt this methodology to answer these questions. So what I'll tell you with this, what we've done— What I did in my postdoc and the first year or so here was use this recombinase fusion technique to identify *Vibrio cholerae* genes—this was the bacteria that causes cholera—to find genes of this pathogen that are turned on during infection.

Now, this bacteria only causes cholera in humans, so we have an animal model. We have a five-day-old mouse model where we anesthetize the mice, inject the bacteria into their stomach via tubing, and the bacteria go into the small intestine and multiply and cause cholera disease. Then we can euthanize the mouse, dissect out the small intestine, grind it up, and do things with the bacteria. We can try and figure out what's going on in there.

I refer to that—the host pathogen interactions and the infection—as a black box, because we can't see what's going on in there. We can only interrupt the infection and try and find something out, and when you do that, you ruin the interactions. So there's a principle in physics called "the uncertainty principle," and this is the way I like to think about it. You can't know the momentum and the position of any atom or molecule. If you go to make the measurement, you change the results, in other words. It's kind of hard to describe it, but by making the measurement, you force the atom to choose a position, and that's called "the uncertainty principle."

The same thing happens with our black box host-pathogen interaction. The bacteria are in the small intestine and they're sensing the environment and turning on certain virulence factors, and they're interacting with the host in very complex ways that we want to understand. But it's easy to perturb the system. So as soon as we sacrifice the animal, for example, blood supply stops, oxygen content starts going down. But further, we have to then take out the intestine and grind it up in order to do anything, and you've completely ruined the system, so you can no longer ask what was happening. It's a microbiological uncertainty principle.

So this recombinase IVET is a way to find things out, find out what's going on, because the recombinase tells us if a gene was turned on during the infection and the infection's run in its natural course, because the recombinase doesn't interfere with anything. It just goes and makes a small change in the DNA that doesn't affect virulence. Then we collect the bacteria back out and say, "Well, did the DNA recombinase recombine its DNA?" Thus we knew it was turned on by the neighboring gene. Thus we say the neighboring gene was turned on during infection.

Okay, so we did that and we found about a dozen or so genes that are specifically turned on during the infection. So the next question we were wondering about is, well, how do we find out when they're turned on? Are these transient? For example, turned on at only one point during infection, and then maybe turned off. So I realized, well, the recombinase system is going to be great for that. Just infect a group of animals and at different time points after the infection, collect the bacteria back out and ask, did the recombination event happen? That way we've got temporal information. When is the gene turned on?

We also can check the bacteria in different segments of the intestine and say, "Well, where is the gene being turned on?" So that was kind of the first extension of the technology, to get temporal and spatial information about these genes that are being turned on during infection. So we've done that and we continue to do some of that work, to this day, asking when is a gene turned on.

The other thing we did was ask ourselves, why is a gene turned on? So, for example, we've learned that the genes for cholera toxin, the major toxin that this pathogen makes that causes the diarrhea, we found out that this gene is turned on at about four hours after the bacteria were put into the animal. And I remember before we did that experiment I asked the people in the lab, "Well, when do you think it's going to be turned on?"

And most people said, "Immediately. As soon as the bacteria get in the intestine, they're going to turn this on." And everyone was wrong. It was turned on at four hours, so there was a delay.

So then the question became, why? What signaling is going on in the bacteria? What are they sensing? How are they sensing it? And what determines this timing of expression of cholera toxin? So again, I came up with an idea using the recombinase system. We can use it to identify other bacterial factors that are required for sensing and making the decision to turn on the cholera toxin genes. This is also technical, so I'll give you the three-minute version of that, the two-minute version.

VAN BENSCHOTEN: Okay.

CAMILLI: This reporter system works by— The recombinase gets made at some point during infection, and then goes and excises out a segment of DNA that's flanked by the recombinase recognition sequences. The intervening DNA is usually an antibiotic resistance gene, so the bacteria start off being resistant to the antibiotic. During infection, when the gene is turned on and makes the recombinase, the recombinase goes and excises out that antibiotic resistance gene from the chromosome, and then that cell and its descendant daughter cells become sensitive to the antibiotic.

So for the cholera toxin reporter strain, we have the cholera toxin genes fused to the recombinase. Those genes get activated at four hours and the bacteria then become antibiotic-sensitive, sensitive to the antibiotic. So what we did was mutagenize the bacteria with a transposon, hoping to knock out regulators, the regulators responsible for turning on cholera toxin. If we knock out an essential regulator, cholera toxin will never turn on at four hours. They'll keep the antibiotic resistance gene, and they'll be resistant to antibiotic.

So all we did was let the infection go, collect all the bacteria back out, and select only those strains still resistant to the antibiotic. Those strains never turned on cholera toxin when they should have, because we have transposon mutations in required regulatory genes, genes that make proteins needed to sense host signals and turn on cholera toxin.

That is, if I had to pick one thing, one, the most exciting and important thing that I've come up with on my own is that, and we used it to identify some really neat and novel regulators of cholera toxin, because it's one thing— We did some cool things by determining

that cholera toxin is turned on at four hours. No one's ever done that before.

And then we've gone one step further and are starting to figure out why is it turned on; what are the other players? So, in other words, we're deriving information about what the pathogen's doing during the infection without ever tampering with the infection, and I think that's a very cool thing. It's a way around "the uncertainty principle," and it's really a change in the experimental approach.

Now, there are limitations to that technology, which we can talk about later. You can only derive so much information using this *ex post facto* reporter system. So that's what we're working on now, but I didn't tell you about the new project.

This new project, which I have a grant submitted on, is to study another human pathogen called *Streptococcus pneumoniae*. It's also called the pneumococcus. And this is a major killer, second leading bacterial killer in the world. It causes pneumonia. It also causes middle ear infections in children, which is one of the reasons I started studying it, because my kids just boom, boom, boom, ear infection after ear infection. And it can cause meningitis. It's a Gram-positive [bacteria].

I worked on Gram positives during my Ph.D. work, including Streptococcus, so I did a lot of reading and decided I'm going to start working on a second project on *Streptococcus pneumoniae*, and this is a pathogen that kills people in the United States. Cholera doesn't. So in a way I'm hedging my bets that continue to get funding but still working on something that I'm going to like.

So in a way, this funding thing has influenced what I'm working on, but I wouldn't say that I sold my soul in any way. I always knew I'd work on a second project, but the choice to work on something that kills people in the United States, I admit was influenced by funding, but still, it's a very interesting bug. I'm happy to work on it. So we have some new stuff going on with that that I can tell you about.

VAN BENSCHOTEN: Okay. We can pick that up again tomorrow.

CAMILLI: Okay.

[END OF TAPE 4, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Andrew Camilli**

INTERVIEWER: **William Van Benschoten**

LOCATION: **Tufts University Medical School**
Boston, Massachusetts

DATE: **10 October 2002**

VAN BENSCHOTEN: It is tape five, side A.

You wanted to talk a little bit more, I think, about the *Streptococcus* project that you have.

CAMILLI: Yes. That's a new project that we started about five years ago. So the project got a slow start, and [*Streptococcus*] *pneumo*[*niae*] has been difficult to work with, but basically what my objective was, was to learn about virulence gene regulation for this pathogen, because zero is known about regulation of virulence factors in this organism. And as I was thinking about studying this pathogen, I talked to a couple of people in the field, that studied pneumococcus. The general feeling was that, well, you know, who cares. This is a strict human pathogen. It doesn't live in the environment. There are not two phases where it lives, so why would it regulate virulence genes? Why would it need to turn them on and off? It's always inside your body. So their feeling was (a) there wouldn't be much regulation, and so, (b) then why study it?

But I knew this pathogen was going to regulate its virulence factors in response to different tissues in the host; just intuitively, I knew it was going to. And in fact, that turned out to be the case. So we . . . . I then got a graduate student, Dave [David L.] Hava, and he's been fabulous. He's been, in many ways, my best graduate student. Works very hard at the bench, is very smart, and has really made the project fly.

Now I have three people working on this *Streptococcus*, so ultimately I want half my lab to be working on that, and half on *Vibrio* [*cholerae*], and then have two RO1 grants to maintain. The project's gone very well in the last three years under Dave's work and some of the newer folks in the lab. So, what we started off by doing was a large screen for essential virulence genes. These are genes that are required for pneumococcus to infect the lungs of mice, adult mice. So we anesthetize them, [add] a couple of drops of bacteria into their nose, and they actually breathe it down into their lung, and will get, ultimately, a lethal pneumonia, a lethal infection of the lungs, and we can recover the bacteria back out of the lungs.

We did a new type of technique invented by a scientist named David Holden, who's in England. He invented what we call a negative screen. So we make a transposon mutant library of *Streptococcus pneumoniae*, and each transposon is uniquely tagged with a short DNA

sequence of about forty base pairs. It's the equivalent of a barcode on objects in the supermarket. Each item has a different barcode. Each transposon has a unique barcode, which is a short DNA sequence, and we can keep track of these tags, as we call them.

So, we infect mice with this pool of mutant, of transposon insertion strains, let the infection run its course, collect the bacteria back out, and ask, who is missing? What mutant strain did not survive and multiply in the lung? And we know that it's missing because its tag is missing. We then go back to an archive of the input strains, which are frozen away in the freezer, and we pull out that particular mutant strain via its tag sequence. We go in and we can easily find it, and then we know that the transposon disrupted an essential virulence gene.

So we call that a negative screen. That's David Holden's technique, and it's a very popular, really cutting-edge technique right now for finding essential virulence genes. So we did a very large screen, the largest done to date by anybody in the world. Most people will screen about a thousand and maybe two thousand transposon mutant strains. We did six thousand, and we found over three hundred different genes that are essential for infection in the lung, which is just fabulous. I mean, it has been a goldmine.

So now, amongst those three hundred genes are about a dozen genes that we predict are regulators of transcription of other genes. So these are transcription factors, transcriptional regulators, and this is exactly what I'd hoped for, because here we have found regulatory genes that are essential for virulence. Thus, these things must be turning on and off genes that are critical for the infectious process.

So virulence genes are regulated in pneumococcus, and that makes perfect sense because this bacteria normally lives in our nasopharynx. We're asymptomatic; 5 or 10 percent of us have this bacteria in us with no disease. Only rarely does it get down into the lung or invade the bloodstream and cause disease, and that's a different environment in our body. The bacteria should regulate genes, turn certain genes on and other genes off, to be able to survive and grow in this new environment. Okay.

So anyway, we did that. We found a bunch of virulence genes, a bunch of regulatory factors, and we're now focusing on a few of these genes. We also found a pathogenicity island in our screen, and we're now studying those [genes]. So that project's flying, and I have a revised grant application that's going to be reviewed in a month, and I hope—I think I have a pretty good chance of getting it, because the prior work was—I had no grant for it.

I spent a lot of my Pew [Scholars Program in the Biomedical Sciences] award to fund the initial years of research on that, but the Pew [Scholars Program in the Biomedical Sciences award] ended a year ago. So this past year I've basically been taking money from my cholera grant to pay for the pneumo research, which is— You're not supposed to do it, but everybody does it. It's the only way to get started on a new project, is to take a little money from other sources. So that's that project, and it's been a lot of fun, and it's a great project. Who knows, maybe it'll take over the lab down the road, and we'll only work on this, depending on how exciting a set of findings we make.

VAN BENSCHOTEN: What are some of the potential long- and short-term applications of the work that you do? I came across an article for a potential vaccine that might come out of your work.

CAMILLI: So both for our cholera work and for the *Streptococcus* work, the ultimate goal and what's driving the research in the fields are better vaccines. For cholera there's really no vaccine, so any vaccine that would work would be welcome in the cholera field. The pneumococcus field— There is a vaccine right now.

Streptococcus pneumoniae has over a hundred different serotypes— So these bacteria have a capsule. They're surrounded by a thick layer of polysaccharides called a capsule—some people call it a slime layer—and it endows the bacteria with resistance to phagocytosis. So macrophages will try and eat the pneumococcus and kill it, but then the capsule protects them from being eaten by macrophages, and it's the number-one virulence factor. If you knock out the genes for making the capsule, these bacteria can no longer cause disease in the lung or bacteremia or meningitis.

Anyway, the different serotypes are based on the type of sugars in this capsule. There are over a hundred different serotypes, and about a dozen or two dozen are frequent human pathogens. The current vaccine only protects against six of those serotypes. There used to be one that protected against twenty-three serotypes, but that's been replaced by a newer better vaccine that protects against the six dominant, most prevalent serotype strains. But other serotypes cause disease, so this vaccine is not optimal.

Some of the serotypes, some of the capsules, are not good antigens. They're not good in a vaccine. They don't work that well. So one of the goals in the field is to create new types of vaccines that instead of targeting the polysaccharides, or the sugars in these capsules, target surface proteins that are more conserved amongst all the serotypes. So, in other words, a vaccine that could protect against all the different serotypes is one of the goals.

So, in order to do that, we need to know what proteins are on the surface of the bacteria, and are these essential for virulence. You don't want to target something that's not essential, because the bacteria will just mutate the gene and not make that antigen anymore. Then the vaccine is no good, because vaccines place a selective pressure on the bacteria in the human population. So anyway, vaccines are the ultimate goal.

Right now, the vaccine field—and it's pretty much from A to Z—is basically empirical. There are no intelligent strategies for making vaccines, and the reason for that is the past successes were done empirically. You take, for example, the protectant against smallpox. A vaccine for that was discovered, actually, I think, [hundreds] of years ago. There was a correlation made between having been infected with another, related virus that would give you protection to smallpox, and that was known for [hundreds] of years. But anyway, that was

purely, you know, empirical. It wasn't an intelligent strategy to come up with a vaccine.

And even to this day, vaccines— Basically, the attempts that are made are you take the virulent strain, you knock out some genes that encode toxins, for example, cholera toxin for cholera, and then you hope that this new attenuated strain would be a good vaccine. And so that's been done over and over again, and repeatedly failed, so we need smarter strategies. So one idea that I have in relation to the Streptococcus work is, if we can learn what virulence factors are turned on during the infection, so these are regulated, we can then get a handle on what proteins are made during the infection and thus try and come up with a new vaccine where we include all of those proteins. Because when you grow the bacteria up in the lab, they're not expressing these things, so you don't know about them.

That's the other thing, what I mean about empirical. So there's also a wholesale kill vaccine for cholera, and what they do is they grow the bacteria up in a flask, and they kill the bacteria. Then they have people just drink these dead bacteria, and they get— Your body makes antibodies to the surface factors, the surface proteins. But the problem is, is that the surface of the bacteria isn't the same surface as the ones that are causing an infection.

So, that's what I mean by we need to learn which virulence factors are turned on during the infection, because those are what you want to target with a vaccine. So that's the premise. That's why I'm interested in virulence gene regulation, or at least that's the application of it. I'm also interested because this is like exploring. Learning about these pathogens and how they regulate genes and what the function of these various genes and their protein products are is fascinating to me. They're incredibly complex organisms, and have evolved over, you know, thousands, sometimes millions of years, with humans. And so the beauty of their complexity and how they circumvent and overcome our immune system is fascinating, I think.

So, that's one thing I wanted to make clear is that I really equate my job with being an explorer, because I feel the need to explore. I've always felt that, since I was a kid. Like going out on hiking and fishing trips, discovering a new pool in a river, it's just always been— Goes right to the core of me and gives me satisfaction. There's really not much left to explore in the world, at least not in the Northeast of the United States. And you can't make money at it, so I've become a molecular explorer, which is slightly less satisfying than the type of exploration that I feel— Well, one of the problems is you don't see anything that we explore. It's all numbers on paper, or if you're lucky, you have the images from an electron microscope or some other type of microscopy. But these virulence factors we work with, we never see them. We only have a readout of their effect, and so in a way it's less satisfying that you don't see them, you don't feel them, but nevertheless, it is exploring.

VAN BENSCHOTEN: You have several duties as a PI [principal investigator], so we'll change gears a little bit here, and talk about those duties. What do you spend the bulk of your time doing as a PI [principal investigator]?

CAMILLI: You want to talk right now?

VAN BENSCHOTEN: Yes.

CAMILLI: Because it's constantly evolving. Currently, I would say over the past six months, the majority of my time, 60 or 70 percent of my time, has nothing to do with the research going on in my lab, and that irks me, and I'm constantly scheming and planning how to fight back.

One of my problems is, I'm not good at saying no. Well, I just got tenure and promotion a few months ago. Prior to that, I really couldn't say no. You can't say no to your chair. You can't say no to your colleagues. You can't really say no to anything when you're an assistant professor. You can, but—

VAN BENSCHOTEN: It's risky.

CAMILLI: —it's risky. So now that I'm tenured and promoted to associate, I feel I should—Actually, I have been starting to say no to things, and I need to do it more and more, to carve out more, get back more time with the lab, because as my lab's grown, I'm needed more and more. I need to interact with the people in the lab more and more, and yet my time available for that has gone down.

So I'm going the way that all professors go, to a paper shuffler and a writer and a reader, and not doing work at the bench anymore. So the bulk of my time now is doing things like going to meetings, either faculty meetings that have absolutely nothing to do with research, or going to meetings, for example, thesis committee meetings. I'm on eighteen students' thesis committees, so they have to meet every six months, and it's an hour and a half, sometimes two hours long, where they present updates of their research. In a way that's a learning experience for me, because I learn and am involved in other students' research, but often it has nothing to do with the research going on in my lab.

There are seminars. There's teaching. There's various paperwork, like I do all the ordering for my lab. So a good 10 percent of my time has to do with ordering and dealing with the billing and all that.

So one of my problems is, I've never had a technician or a secretary to do that type of thing. I've had money to do it, and I have money right now, actually, to do it, but there's no room in my lab. I've made the choice three times in the past seven years to take an additional researcher instead of hiring a technician/secretary. I would like a technician who would half-time do paperwork, ordering, and other stuff, and half-time do research. But I've consciously made the decision three times to say no to that.

I even interviewed people one time and decided instead to take a graduate student, because more research will get done in my lab, because my lab's not that big. I don't feel sorry for myself. I've made that decision in the past and I have to live with it. So I spend about 10 percent of my time ordering and dealing with billing. Maybe another 10 percent of my time is dealing with thesis committees and other departmental research activities. About 20 percent of my time is dealing with other types of paperwork, either reviewing papers—I'm on a couple of editorial boards now—working on drafts of papers in the lab, dealing with the types of grants, various types of grants, my own grants and kind of project-type grants. Maybe 10 percent of my time I just do my own thing. Like I'll be e-mailing a friend or be dealing with something about my kids or my wife, like, I don't know, finding a gift online for someone's birthday, something like that. So 10 percent of my time is doing that.

I'd say a good 20 percent of my time, I'll call it, is directly involved with research going on in my lab. That's either having lab meetings in my office, which we have like every Friday at noon, or out there in the lab talking with people about their work, or the students come in. My door is almost always open, except for now, for them to come in and talk about their research. It's become a struggle over the last couple of years to keep track of everyone's project and have good ideas about it, because I'm less and less involved.

Ideally, I would be out there in the lab doing experiments a couple of hours a day, to keep track of what's going on, but I actually lost my bench six months ago. My lab has seven people and there's really only bench space for six, so I lost my bench. So when I go out there to do an experiment now— See, the past week, actually the past two weeks, I've been out there a couple of hours a week doing an experiment, and I basically have to look around and see who's missing and go to their bench and steal some of their solutions, and it's an ugly situation. But that's what I'm forced to do right now.

Anyway, in about seven months, roughly, I'll have a new lab with space for twelve people, and assuming I get one of these two grants coming up in November, I will hire a technician, and my life will change. I predict I'm going to get back another 10 percent immediately of research time, after a few months of training this person. Just taking the ordering out of my hands will be a tremendous boon to me.

I feel my level of involvement in departmental and university [Tufts University School of Medicine] activities has peaked. I really feel that I'm going to fight back on that, because right now I'm on the Curriculum Committee, which is a hellish committee. They meet frequently and it's serious. I have to show up; I have to be involved, because it's the total med[ical] school curriculum. So I feel I've reached my peak in terms of involvement.

I'm also the director of our kitchen facility, dishwashing and media preparation, and that's a big job. So I'm maxed out in terms of my responsibilities with the university. In my book, I'm maxed out. I'm not going to take on any more there, including having to say no.

I get about an average of two papers a week to review. This is a very interesting thing for a scientist. I only publish maybe three or four papers a year. You would think that I would then

only review three or four papers a year, to give a full payback. However, only about one in three papers gets accepted, so maybe I need to do three times that amount. So let's say one a month would be my equal payback, yet I'm doing— Recently it's been about two papers a week. If you really average the last year, it's maybe three or four papers a month, so I'm paying back manyfold more than I'm doing, and I've been told that that's how it is if you're going to be a respected scientist doing cutting-edge research, that you're expected to review more because you're now an expert. So, I agree with that. That's how it should be.

So I've bitten the bullet and I take— I basically say yes to every paper that editors send to me. If I have two or three on my desk, I say no, can't do it, because I do give them back in a timely fashion. Occasionally I'll give a paper to someone in the lab to read if it's something right up their alley, and a lot of people do that. You're not supposed to, but I know people do it, because what you do is you give it to them to review. It's good for their education to go through the process of reviewing, and usually they do a much better, more thorough job than me, and I'll bet that's true of other professors.

But anyway, they then give the review back to me. I read the paper. I read their review and modify it or veto it or agree with it, or [say] whether or not I agree with it, with some minor modifications. So occasionally I have people out there review. So that's another big responsibility, reviewing papers.

VAN BENSCHOTEN: You mentioned teaching. How much teaching do you have?

CAMILLI: I have a very light load. I cannot complain about it. I give about six lectures a year, which I do take very seriously, so there's a lot of prep time involved in those. But having given each of them before, it's a lot easier now, maybe a couple of hours [of] prep time for each lecture. And they all come together in the fall. So the past two months I've given all my six lectures, so I'm done with lectures.

But I'm the course director of the medical microbiology course, and that's a big job. So six weeks, I'm a 50 percent-time teacher for six weeks of the year, which is not bad in the grand scheme of things. I go to most of the lectures. I deal with all of the paperwork in getting a course syllabus together. I deal with the exams. Like right now, I have a midterm exam Friday morning, so I'm rereading and rereading and proofreading and making up new questions for the exam. About half the questions I get from the lecturers in the course; the other half I make.

So anyway, for six weeks, medical microbiology, 50 percent of my time. Every other year is a graduate course that I co-teach with another faculty member, and it's the bacterial pathogenesis course for the graduate students. And that's it. So, a very light load.

VAN BENSCHOTEN: How about study sections?

CAMILLI: I have not been on study section yet, and I would say no to being on study section until they give me more grants. I've only gotten one RO1 grant, so if they were to ask me to go and review, to be on study section, I would say no. I'm also protected by being— Well, I should serve on study section, and I hope to eventually do it and will do it. I feel (a) it's my duty, (b) you really learn how to write grants by being on study section. But (c) I'm protected by being a white male in the Northeast, because there's a regionality to who's on the committees. So there are so many male professors in the Northeast that I'm just one amongst a huge school of fish, and they're not so— So in a way I'm fortunate that I have not been asked yet.

I was asked to go down to NIH [National Institutes of Health] and review postdoc[toral] fellowships, and that was right prior to my time to teach, and I said, "No, I can't do it this time." Ultimately I expect to be asked to do it in a year or two, after I've gotten one of these other grants, is what my speculation is, and I'll go and do it. So that's the short answer to that. I have not been on study section yet.

VAN BENSCHOTEN: Congratulations on your tenure. I didn't know that you have tenure. What does tenure mean here?

CAMILLI: It's the full definition of tenure. My job is guaranteed till retirement, barring any unethical behavior and other types of screw-ups. Now, I still am supposed to get— And that means my salary is guaranteed at its current level. If tomorrow I stop getting grants altogether, as at any place in this university, I would ultimately lose my lab if my research didn't continue to flourish and get grants. I would lose my lab, and my salary would basically stay static. So I have to continue to do research, and, of course, I will, and continue to get grants.

VAN BENSCHOTEN: So your salary's guaranteed. Is that all of it? Because I remember when you said when you began it was 50 percent of your salary.

CAMILLI: Right. I'm expected to get 50 percent of my salary. There are periods when people don't get 50 percent and the university picks it up. My understanding is that when one receives tenure, their full salary is guaranteed at that level. Again, if I lose grants, I may stop getting raises, but I don't expect to lose grants. So I'm pretty sure that the total salary is guaranteed, not 50 percent.

So anyway, some people's research does go down the tubes and they become more heavily involved in teaching and other activities, so everyone earns their pay. Most people earn their pay when they're tenured, and so I will, too. One can lose their grant if their research falters— I mean, can lose their labs if they stop getting grants— so that occasionally happens.

My department has had a great, fantastic history. Everyone's always been funded and had

productive labs, and that's always been the case and probably will continue to be the case because we have a small, really fantastic department. Promotion to associate has much less meaning. Basically it meant I got a raise and a little more responsibility, but that's basically it.

VAN BENSCHOTEN: Another duty that you have is writing articles for journals. What is the process in your lab for doing that?

CAMILLI: As I was taught by Dan [Daniel A.] Portnoy, my Ph.D. mentor—and this was not always shared by professors—the students and the postdocs would write the paper. It's part of their education. They have to learn to write well. So they'll write papers over and over again. I'll critique them, give them back, and we'll go through this ad nauseum. But it always happens, and students vary in their writing abilities. Occasionally I'll write a paper, but rarely now.

In reviews, it's the same thing. If it's my review, I'm the only author, I'll write it. Oh, every paper that leaves my lab has been seen by multiple people, multiple colleagues, because I'm not the best writer in the world and I really rely on papers getting seen by multiple people. It's a little bit painful and slow, but the writing's usually quite good by the end. So I feel that every paper I've ever published was well written because of that process. So that's the writing. That's what happens with papers in the lab.

VAN BENSCHOTEN: You've described in the earlier session the lab management styles of [Daniel A.] Portnoy and [John J.] Mekalanos. What is your own lab management style? What kind of boss are you?

CAMILLI: I'm probably different than any faculty member I know of, because I feel I'm just like a grad[uate] student, still. I feel I can relate to the students on the usual levels that they would have with a peer. And so even though I just turned thirty-nine, I've just been promoted, I still feel like I'm a grad student.

I mean, they treat me with respect. They know I'm their boss, their mentor, but at the same time I feel fortunate to enjoy a more relaxed atmosphere with the students, like we can joke about things. We eat lunch together sometimes. We occasionally will go out to a sports bar and play pool, and I have fun with my students. Now, a couple of times that's not been perceived well by people, that I should not be socializing with my students, or hanging out with them.

For example, one of my first students, [D. Scott] Scotty Merrell, who's now doing a postdoc at Stanford [University], he actually became pretty good friends with myself and my family. I was very careful to not give him any more attention or time in the lab than anybody else, but a problem arose. Scotty was a very aggressive student, and was always coming in with his research. So, in fact, he did get more of my, I'd say, quality time in the lab than the other

students, and the students, some of the students got together and came and talked to me and said, “This isn’t right. He’s getting more of your attention because he’s a good friend of your family’s.”

And it came to a head that neither the students nor I could convince the other of their case. My claim was that Scotty, being a friend of myself and my family, had nothing to do with my interaction with him in the lab, that even if I couldn’t stand him outside the lab, he still would be coming in, just because he was very aggressive and career-driven and research-driven. He would just— Students vary in their, I don’t want to say aggressiveness, but their coming in and using me, getting information from me and showing me their data. And the more you do that, the more ideas I can give them.

So anyway, that got me into a little bit of trouble with the students, and it was never really resolved. So now I feel like I really shouldn’t— And I have to say that some of my colleagues felt the same way, that you should never become friends with your students. And I, to this day, believe, yes, you can become— The rare student who, for whatever reason, becomes a good friend, is fine. And I’ll always stick to that, as long as one can maintain their professionalism in the lab, that’s fine.

So my style is pretty laid-back. I have fun with the students and the postdocs. We’ll have a gin and tonic to celebrate someone’s birthday or whatever, and you know, we’ll have conversations that I guarantee you no other faculty member has with their students and postdocs. Occasionally I’ll stop myself and I’ll say, “You know what? I should not be having this conversation with you.”

VAN BENSCHOTEN: [Laughs] “It’s not happening.”

CAMILLI: And so it’s kind of funny. But the students enjoy that, that I’m, I guess “cool,” they would say; at least that’s my feeling. I don’t know if they talk behind my back, but I feel I have a more realistic, less of a generation-gap-type relationship with my students. And so the ultimate result of that, and this was a conscious effort on my part, is to make the lab as comfortable as possible, that the students want to be here, because I’ve seen labs where students didn’t like the PI, weren’t comfortable in the lab environment, either personalitywise or just it was a dull, boring, “no one talked” lab. It was more like a nine-to-fiver job.

So I want my students to feel comfortable, and I’ve been successful in that. Students have wanted to come to my lab for their thesis work. They often work long hours because they (a) can get their work done, but (b) it’s kind of a comfortable, fun place to be. I mean, other professors had the same lab in the past, had this little room outside here which I have as a lunch room, was research space. Their students had to go out and sit on the porch and the street to eat their lunch. So that’s a lunch room, with refrigerator, and they can hang out in there.

So anyway, that’s part of my management style, to make the lab as attractive a place to

be in, because even if you're in there eating your lunch or we're goofing off having a gin and tonic, whatever, you're there and you can have something going out there [in the lab], because there's a lot of wait time in between experiments. And [one can read a] journal, or whatever. So that's my strategy.

My new lab design, I've gone to great efforts to make, create space, livability space in my lab. I was originally told, "No, you cannot do this," by the architects and by the chair of our department. Why should I have a little room that's a little lunch room, and no one else has that? And I fought. I fought and won, and my lab now has a nice little lunch room with a little alcove with a sink and refrigerator, and so I just feel that a place has to be comfortable.

VAN BENSCHOTEN: Well, too, I want to say, the first session that we had, when I came into your office, there was a grad student at the other end of the settle here, who was eating pizza on a paper plate. It's little things like that, I think, that tell you volumes about the PI in a way. I mean, you don't often see that, so obviously they feel comfortable enough that they'll even occupy your office sometimes, but definitely use this outside room. That's for sure.

CAMILLI: I think that's valuable on a number of levels, but one is that I think students, postdocs, and their mentor have to be honest with each other, because if they're not, if they're hiding things from each other and ill feelings are building, that can be disastrous, not only for the education of the student and people's feelings, but the lab always knows about these types of things, bad feelings, and it creates tension in the lab, and that's just bad.

People get a bad feeling in their stomach when they think about going into work if things like that are going on, so, you know, occasionally one of my students will come in and just be in my face and say, "You know, this isn't right." And so I welcome that. Having had six siblings, I can take just about anything, any type of teasing. I dish it back, and in a way I get some enjoyment out of it. Maybe it's a little sick. But occasionally some of my students, in particular a few, can be critical or even sarcastic, and I actually enjoy it. So anyway.

I was going to add something else. Oh, yes. So, another thing that's important about my style of mentoring is, they can come in and say anything, speak their mind, but they feel comfortable to come in and talk about their research. Now, I've had a few— I have a few students that are a little bit shyer than the others and don't come in enough, telling me about their work, and so I'm on their backs saying, you know, I say, "You do not come in here often enough. I cannot keep track of your work, so every time you come in, we have to spend ten minutes of you refreshing my memory of what's happened in the past." Then I say, "You've got to be more frequent in your coming in."

And they say, "Yes, you're right." Then they'll go through a period where they do come in every week or a couple of times a week, and then it fades off again. So, students—everyone—has a different personality. My style isn't the best of styles with some certain people, because I'm not— What I should do for that type of student is say, "Okay. We're going to

designate a time. Twice a week you come in and tell me what you're doing." But I don't do that, so my style does— I wouldn't say fails, but it's not optimal for certain students.

Another part of my style that's bad is the occasional student that isn't working very hard, isn't really— There's something wrong, either a lack of self-confidence or quite comfortable with where they're at in life; don't really want to move on. I'm very bad at dealing with that, and moving, you know, getting on these students' backs and really forcing them. I rarely give ultimatums. I have. When it's gotten bad enough, I have done that, so I'm not a total failure in that respect, but I just can't really— It just causes me too much pain to be on someone's back all the time and saying, "Well, you're not working hard enough. You need to do more experiments every day." I just don't like doing that.

Part of that is I don't like confrontation. That's part of my childhood. Part of my upbringing is avoiding confrontation. I'm sure it has to do with sibling rivalries. Growing up with my siblings, you never— You didn't want to show weakness, so one way is to avoid arguments and avoid people's really spilling out their feelings and arguing. So I don't often go all the way with that when I should. Anyway, I don't know if that made sense.

VAN BENSCHOTEN: Oh, yes, I think it does. Let's talk a little bit about, you have a family. You have two children. How do you negotiate the demands of both work and family life?

CAMILLI: So, I've worked pretty long hours as a grad student and postdoc, and my wife [Kristen M. Camilli] was fully supportive of me, but from day one, before we got married, she asked me quite bluntly a couple of times, "You realize that when we have children later on, you can't work these hours."

And I always said, "You're right. I will cut my hours down and take care of responsibilities with family." And I've done it. So now I work forty-hour weeks. I bring home stuff to read, which I don't count as work hours, but my wife does. [mutual laughter] But I am a family man. I'm juggling the two. And the honest fact, the honest-to-god truth is, I enjoy playing with my kids and doing things with them more than anything in the world, and so it's been easy for me.

If my wife and I had a bad relationship and we were always fighting, or I turned out not to be very loving with my children, I don't know what would have happened, but I enjoy my kids tremendously. They're at that age where, you know, everything's new and they're learning how to speak. Like my five-year-old [Adam O. Camilli], I'm teaching him how to play chess right now, and I just sit there in fascination at some of the things they say, and when they have a new realization. So, I have no problem spending time at home. But still, I come in. You know, I leave at eight and I get home at six-thirty, so I could be home an hour earlier and be home right when they get home, but I don't do it. We get a good two hours of playtime every night.

The first interview day we had in the afternoon because I had to be at kindergarten for the

first time while my son was there. And I've got to tell you, I walked— Half my body wasn't through that door when Adam just said, "Daddy!" and ran across the room, and he was just ear-to-ear grin that his daddy was there, so he could show me his friends and his homeroom. I've got to tell you, I never experienced— I do not have a memory of childhood of my parents ever coming to my school, ever having that moment of being proud and— I don't know what good it's going to do for him, but I never experienced that. Or I've forgotten it, if I did. But after that I realized that, hey, if I have to cut out a half of a day every month to do something at school, I'm going to do it.

VAN BENSCHOTEN: It's important.

CAMILLI: Yes.

VAN BENSCHOTEN: Okay. Let me flip this over.

[END OF TAPE 5, SIDE 1]

VAN BENSCHOTEN: Tape five, side B.

You mentioned when you got up and when you leave, when you come to work, when you leave work. If you would, maybe describe a typical workday, from the time that you get up to the time that you put your head on the pillow and go to bed.

CAMILLI: So, recently, my son Adam [O. Camilli] started kindergarten, so his bus picks him up at a quarter of eight. So I'll get up. My wife's [Kristen M. Camilli] already up, showered, takes a good half hour putting makeup on or whatever she does in there. Right when she finishes and comes in and dresses, I wake up and go take a shower. By that time the youngest [Ian M. Camilli] is usually up. He's an early riser. And then I'll finish my shower, get dressed, and they're already downstairs having breakfast, whatever.

I'll eat real quick, go get Adam dressed. He always has to be roused out of bed; get him dressed. Both my boys are quite emotional and can be quite whiney in the morning, fighting over their, you know, "I don't want to wear that shirt." So there are always these little whining episodes that we have to deal with, and so we get through the morning. Then we all go out and wait for Adam's bus to come, which, thank god, picks up right at the end of our driveway. It's a perfect situation. Picks him up right at the end of the driveway, so we put him on the bus and then my wife and Ian leave. She takes him down to daycare in Rhode Island where she works.

And I go back in the house for about ten minutes and grab my books and stuff like that,

my book bag, and then take off for the train, which is a five-minute walk from my house, the commuter train—we're the last one-car family in America—and take the train in. I sit down; I get to read, maybe go over a paper or whatever on the train. Then I get into town, hop on the subway, and so it's literally thirty-five minutes door to door. So that's a really good, easy commute.

Get into lab about nine-ish, and on days when I teach, I probably come in before— I don't see the kids in the morning. I have to come in at eight, be here by eight, and so those mornings are— This part of the year, the fall, maybe once a week I have to do that. So then I get in at nine, turn on my computer, check my e-mail, often go out and make an espresso, or if I didn't have time to have breakfast, I'll make something, toast or whatever, and have a coffee.

Then I usually tackle some paperwork for a few hours in the morning, do some ordering, or tackle the pile of papers that pile up on my desk, and lately I've been playing catch-up. The last few months I've been playing catch-up. Things have piled up. Do some e-mail, check my e-mails and try and go through and answer the critical ones. My e-mail's piling up a little bit, too.

Then usually by ten or eleven I'll have interacted with one or two students who've come in, and either some problem or they want to show me their research. So that's always good; always enjoy that. Then usually at noon there's either a seminar in our department or another department that I'll want to go to, or there's some type of meeting, so almost every noon is gone. On Fridays we have our lab meeting. So usually I'll put together a quick sandwich and take an apple or something with me to seminar, and eat while I'm listening.

And as I told you off tape, ever since my second son was born, I've gotten less sleep each night, because the three-year-old [Ian], oh, about every other night, wakes up in the night crying and has to use the potty or whatever. It's always something. Or he tries to crawl in bed with us. So I get less sleep now and I started dozing off at seminars. I wouldn't start snoring, but my head would start bobbing, and I never had a problem staying awake in seminars before. So I started drinking coffee about a year ago. Now I'm addicted, have to have my coffee. It's funny, but after a few months of drinking coffee, on weekends I'd get these headaches and I kept thinking, "I'm sick. I'm coming down with something."

And after the third weekend of that, my wife said, "You idiot. It's the caffeine withdrawal. You need your caffeine." So then I started drinking coffee on the weekends. Now I'm fully addicted, and I have to say that I never liked the taste of coffee, but I also, seeing these people, you know, go into the coffee shops in the morning, I saw it as an addiction, and it really is an addiction. And I always assumed in my head that there's no free lunch in life. You're losing something by drinking this coffee, and who knows what it is. Maybe you're going to take a few weeks off your life, or a few years off your life in the end, or maybe your memory's not as good, or something. But now that I'm drinking coffee, I feel it is a free lunch. You get something more than you give, than you ultimately will give back physically.

VAN BENSCHOTEN: You truly are addicted now. [Laughs] That's mind control, as well.

CAMILLI: I rationalize my addiction to coffee.

Okay. So then lunch is usually some type of seminar or whatever. Then I come back. There's always a hurried part of my day now, the last few months. There are always a few things that have to get done that day that I will try and tackle, and so there is a fair part of my day is moving as fast as possible.

I'm like a wicked fast walker through the hallways, and just about once a day almost collide with somebody, just moving quickly, getting things done quickly. Certain days I'll be amazingly efficient, and burn through a lot of responsibilities and things I had to get turned in, like proofing a paper, sending in the proofs, or sending strains to somebody. I'm an excellent strain sender. I'm famous in the field for, if somebody requests a strain, I say, "Okay. You need this, this, and this." Boom. They get it the next week. Because usually people either never send it, [. . .] or say no, or say yeah, and then you wait months, and have to remind them again. But anyway, so I spend I'd say an hour a week sending strains to people.

I try to get some work done, like lately I'm doing experiments and I'm out there working with mice or something, so I move through that quickly. Talk some more with students during the afternoon. Usually two or three times a week there'll be some meeting, Curriculum Committee meeting or something more formal that I have to prepare for and then go away to that meeting. A lot of teaching these days, so I'm going back and forth to the lecture hall or to the laboratory part of the course. And toward the end to the evening, the meetings, the e-mails stop.

Between like four-thirty and five-thirty, things slow down and I can catch up on a few things or go out into the lab and talk some research with people. Like if I've decided I'm done with paperwork for the day, I may go out and either do some experiment real quick at the end of the day or talk science with people. So I enjoy the end of the day. And then I often will either leave at 5:30 or at 6:10, depending on what train I'm going to catch.

And one of my student's is on the same train line as me, and about once or twice a week we'll walk together to the train. Ironically, this is one of the students who thought that one should not have any type of social relationship outside of lab with their student, and he and I walk to the train now and shoot the breeze about things. And I've teased him about that.

The other thing I do, one of my hobbies is I play soccer. [The] past few months I've been playing twice a week, and I actually— This is also something that some people would look on with disdain, is I play with grad[uate] students and postdoc[toral fellow]s, and then a second time a week I'm playing with medical students who I am teaching. Now, is that a conflict of interest? Who knows. But I enjoy playing soccer, so we go out and have pickup games on the Boston Common.

The med[ical] students are better players. They bring nets. It's really organized, and I

really enjoy playing with them because it's fun and good exercise. The grad students and postdocs from the department are less skilled players, but I still have fun playing. So soccer is one of my passions, and I feel if I have a vice, soccer is my vice, because I'll often take off at four o'clock from the lab and go play, definitely once a week. I'd say I average once a week, take off and go play soccer on the Boston Common, and then I'm late coming home. I get home at 7:30 or 8:00. In my mind it's not a vice. It's one of the things I really enjoy in life, and actually it's one of the few things I'm really good at. Like for an American born and raised, I'm really an excellent soccer player.

At the start of the interview [you asked], "What were some of the positive reinforcing things in your childhood?" My ability to play soccer really well and just immensely enjoy playing was one of those things, and to this day it's a positive reinforcing thing. It's one thing I can go do that I'm really skilled at.

VAN BENSCHOTEN: That you lose yourself in.

CAMILLI: That I lose myself in, yes. There's no thinking. There's some thinking but it's not— Yes, you lose yourself in, it's incredibly fun, and I will always be able to justify it for staying in shape. I've gone through a month or two where I stopped playing and stopped running. I also jog once or twice a week, and ran the Boston Marathon a few years ago. I've gone through periods a couple of times where a month or two months go by, usually January, February, when there's snow out there, and my mental faculties suffer when I've stopped exercising. I can't explain it, and I've heard people say "sound body, sound mind," but it's really true for me. My body undergoes changes. I think it undergoes some type of stress when I'm not getting exercise. I suppose if I stopped cold turkey, my body would [eventually] adapt to the new nonexercise mode.

But the other thing is I think I have to exercise for health reasons. My grandfathers [Peter Camilli and Gerard Reynolds] died when they were very young. My father [John P. Camilli] was diagnosed with colon cancer, and his identical twin brother [Leonard Camilli]— So we got a bad gene here or there, and I feel I have to exercise and eat well and rely on modern medicine to get me into old age, based on the history of the grandfathers.

Well, my mother's father was— She was adopted, so the genetics don't— There's nothing there. But my grandfather on my father's side [Peter Camilli] died of, they say, stomach cancer, back in the twenties. It could have been colon cancer. Who knows. My father and his twin brother both have colon cancer, so clearly there's a genetic defect for colon cancer susceptibility.

So I've actually already gone and had a colonoscopy. Usually you wait till you're fifty, but I've already had one, and I was fine. So I'll have those every ten years till I'm fifty, then every five years. The genetics of that susceptibility are very complex. I could either have a fifty-fifty chance of having that susceptibility gene, or a lesser chance. But I'm going with the fifty-

fifty chance, and thinking that I'll be battling this later in life.

VAN BENSCHOTEN: So you said the soccer then is sort of a therapeutic, is a medicine, sort of, if that's a possibility.

CAMILLI: Yes. Exercise— I enjoy eating, and if I'm eating a lot and not exercising, I'm not burning those calories. I'm not using that food fully, so it's passing through my digestive system. Colon cancer, one of the precipitating factors is the flora in your gut digesting the food, and in the process producing these toxic oxidative radicals and things like that that are causing mutations in your cells, in the cells lining your intestine. So there is a correlation between nutrition, and getting fiber and exercise, and cancer.

I tend to take with a grain of salt any nutritional effects on one's health because I think a lot of that stuff's funded by the food companies, to be honest. But intuitively it makes sense, that if you're exercising and you're eating well, it can only be good for you. So I give it the benefit of the doubt. But the bottom line is, I enjoy playing soccer so much, that that's why I do it.

VAN BENSCHOTEN: A typical day, you usually get home, what, around 6:00 or 6:30, depending on the train?

CAMILLI: I get home at 6:15 about three times out of five of the week, and two of those days I get home later, 7:30, sometimes 8:30, usually because of soccer or some late meeting. The Curriculum Committee meetings go to 6:10.

VAN BENSCHOTEN: And what is the routine usually when you get home? What do you do?

CAMILLI: So I get home. My wife's been home for about fifteen or twenty minutes before me. The boys are playing. She usually has some type of dinner prepped. If she's been doing it a number of nights in a row, or we've had some argument recently, I'll find myself making my own dinner, which I don't mind doing.

Occasionally, I would say one time a week, I'll make the dinner, like if she hasn't gotten started, I'll go in and make something. But I do my fair share. To give an honest estimate, I'd say I do a third of all the housekeeping stuff, like making meals, cleaning dishes, emptying the dishwasher, whatever. I do a good third. I should be doing 50 percent, but I'm not home as much as my wife. She is a nine-to-fiver, and she's happy to do more than I do.

And then we each have our certain things, like I mow the lawn once a week. She never

does that. Put out the garbage, the usual stuff. So I get home, help with dinner if help's needed, and then we all sit down in our kitchen and eat. Usually the boys don't like what they're eating and there's some conflict. We're both disciplinarians, but when push comes to shove, it's me giving the boys a timeout and doing whatever.

So we usually eat, and that's a good time to sit down and all— The boys are usually in a trance when they're eating. It's hard to get them to talk, but occasionally we'll talk. Then the boys are off playing and it's cleanup time. Kris [Kristen M. Camilli] will go up and change into some normal clothes, and Adam and I will play chess, or we'll play a computer game, or we'll go down and watch a movie in the basement.

I set up a little, on the cheap, a little home theater down in the basement, and we'll go down and watch Star Wars or something. The boys love their movies. They'll watch the same thing over and over and over, thirty or forty times, until they're on to something else. Then we put the boys— There's a big routine to put them to bed. At 8:30 we head upstairs. Every other night's a shower, a bath. Read them a story— Put them in their PJs, read them a story, brush the teeth, use the potty, and then to bed. That's a good half-hour routine every night.

And then they're off to bed and then usually Kris and I go down and watch TV, or if I have homework, which I have about half— I'd say average, every other night I have something that has to be done before I can go in the next morning, either finishing the proofs of a paper that has to be turned in the next day, or preparing for a lecture or something. So I'll sit at the dining room table and do that. Kris always watches TV at the end of the day. She needs an hour or two of just zoning-out there, watching TV. And then it's off to bed.

Two, roughly one or two nights a week I'll finish my homework, Kris will go to bed, and I just have to watch a movie or something. I just have to have something fun, some alone time. And really that— Kris and I both, the only alone time we get is if one of stays up and everyone else has gone to bed, and we either read or watch a movie or write letters to relatives, or whatever. So we each have to have our alone time once or twice a week. So that's my day.

VAN BENSCHOTEN: Okay. A question about your work. If you would, assess your efforts so far in achieving your professional goals. Are you where you want to be right about now?

CAMILLI: Yes. I would say my professional goals, for the most part, I've done as well as I could have hoped, and what I had planned. I've achieved what I've planned.

I must say that I have a healthy dose of self-doubt. Like I feel my research— So, there's been a little bit of a dilemma or a— Yes, a dilemma with me. I've personally felt that my research could be better. We haven't made tremendous discoveries. I don't feel I'm out there doing work myself enough. Some of my students' work hasn't gone as well as it could. So in a way, on one side of it I feel we haven't achieved, haven't made the types of discoveries that we're capable of, so a little bit of a letdown.

But on the other hand, I've been getting these awards. I've been getting these special recognition things, like the Eli Lilly [and Company Research Award], that tell me, "Well, you are doing okay. You are doing fantastic." And so I'm a little confused.

To be honest with you, I feel that I've trained in the best labs, like Dan [Daniel A.] Portnoy is one of the best, and I went and worked with John [J.] Mekalanos. He's one of the best. I've been in the top labs, so I've gotten the best training possible, I think, but having been in those labs and getting letters of recommendation from them is very important. They've propelled me forward as much as my own work has. Had I been in more unknown people's labs, there's no way I would have gotten the recognition I have.

So in a way I feel it's a little unearned, some of the recognition I've gotten. But on the other hand, this is the game. This is how it's done. So, had I been in these people's labs and not been good at getting work done and publishing papers, I wouldn't have gotten recognition. I also come back the other side and say, "Well, you have achieved a certain level of things. You've trained students well."

My first two students have graduated and gone into really good labs for their postdocs, so I feel really proud about that, and my students get papers. The bottom line for— There are two things for a thesis. They learn how to do work themselves. They learn how to do research themselves. But you need evidence of that, and that's papers, original research written up and accepted, peer reviewed papers. And my students have gotten papers. Scotty [D. Scott Merrell] has six first-author papers. Sang [Ho Lee] had four first-author papers. That's fabulous. I'm doing much better than anybody else in my department in relation to that.

That's another one of the reasons I have no problem getting students wanting to come into my lab each year, is the students— Also, my two students were the first to graduate in their class, so I feel proud and happy. We're really cooking in this lab.

But in terms of the quality of the discoveries we're making, the importance of the discoveries we're making, I'm a little disappointed there. And I feel hopeful that we're— You know, we're always onto something and we're waiting for things to come out. We have had two major scores, two major discoveries that were fantastic, and we got really good papers in high-profile journals with those, but they don't come often enough, to my—

VAN BENSCHOTEN: Satisfaction.

CAMILLI: To my satisfaction, yes. But, you know what? And then the third thing is that if I felt differently, that would be a very bad thing. If I was totally happy with where I was at, I'm probably going to slow down or be less demanding of myself or whatever. And that's actually a common knowledge among scientists. If you're not hungry for more success, if you're not self-doubting, you're probably not going to achieve; you're probably not going to go to the heights

that you really would like to achieve.

I've always heard—had conversations and heard things from my mentors— about what makes the type of person that's like that. I mean, that type of person has a problem, who's never happy with their achievement and who's always wanting more. And there are a couple of different types of personalities that have that. There's the egomaniac, the Type A, who's just driven to be better than anybody else. Then there's the type who is insecure, and is driven forward by not wanting to look like an idiot.

I feel I have a little bit of that. I don't want to fail. I'm not the type of person that is driven by wanting power and success and to be better than other people. I really am not like that. I'm driven by a little bit of insecurity, but also by a heavy dose of really enjoying what I do. And I realized that early on, as an undergraduate. When I was in one of the laboratory courses, I realized one day I was the only kid in this class who was fascinated by what I was seeing under the microscope. And one day another kid said to me—this was toward the end of my undergraduate career—I said to him— We were looking through the microscope and I said, "How come I feel like the only one that really likes what I'm seeing here?"

And he said, "Well, you probably are, and that's why you're going to graduate school."

So I do enjoy the adventure and the discovery mode of it. That's the ultimate thing that's driving me, and that I'm proud of. I have a little bit of pure scientist in me, and a little bit of insecurity, not wanting to fail. And I get positive feedback when I do get a nice paper and Dan Portnoy or my chair [Catherine L. Squires] says, "Oh, good job." I get satisfaction from that.

And I have no doubt that that comes back from not getting a lot of positive feedback as a child. Like it really impacts me when someone says something nice about me, in a way that it—I don't know. Perhaps everyone should be like that, regardless of what their childhood was like, but I feel that really getting positive feedback from colleagues and my boss really means something. I say "my boss," but I'm my own boss. But the chair of the department.

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

CAMILLI: Well, we have some stuff in the works right now that is a mixture of can't-fail type of work. This is more the type of stuff one has to do to get papers and grants, so, can't-fail stuff, but that is not so exciting. It's not going to be any great advance, you know, it's not going to be a new paradigm, whatever. So we have a large amount of that going on. It's also driven by the students' need to do experiments and get positive results and get papers, so it a way that drives us a little bit, doing the ordinary.

We also have some things in the works that are more risky, that we could discover something really nice, and those, you never know how those are going to turn out. My hope, and

what I think we have a fair chance of doing, is making some big discoveries, in particular with the pneumococcus work, and what I foresee— So right now, I've just gotten my foot in the door in that field.

I'm getting some resistance from the established people, for whatever reason. Probably just don't want another person to compete with them for grants and papers and whatever, getting papers in the journals. But that's gotten my ire up a little bit, and so I've made special requests when I've sent in papers and my grant, that such and such not review my paper. For example, my grants got their primary reviewer, was pretty nasty, and I do not know for a fact who it was, but when I looked at the list of people on the study section, I have a pretty good idea who it was.

So when I sent in the revised, I said, "I absolutely do not want reviewer number one to review my grant again. They do not understand the science," etc. And I do that with some papers. So I've had to fight back and carve out a niche for myself.

So there is competitiveness, I guess. My goal is to come out with some big hits in the pneumococcus area, get some papers that are going to be fantastic, and we already have one in the works that is something that no one else in the field can do, that is our expertise, and we've done it and made some nice discoveries. And I want these papers to get out there, and people to say, "Camilli's done it. He was right. We do need him in this field."

So the pneumococcus I have high hopes for. I think we're going to get established. I'm going to become established in that field, and the research is incredibly fun. We're doing stuff that should have been done in the past and no one's done it because they don't have the expertise. The *Vibrio* [*cholerae*] field is more of incremental, going the next step, building on what we've done, and not so exciting, with the exception of one project that is pretty exciting. We are actually past the planning stage and in the preparatory stage to do a genetic screen for *Vibrio cholerae* genes that are turned on during infection of humans, human volunteers. This is being done in collaboration with a guy named Jim [James B.] Kaper, who is the archenemy of John Mekalanos, my former boss, and who got me into a little bit— When I started collaborating with Jim Kaper, I told John at a meeting about that, and he was furious with me. So we had a big falling-out for a period there.

We've since come to some understanding, but I'm collaborating with this former enemy, Jim Kaper, to do this screen in humans. And if we do it and we're successful, it'll be the first— to our knowledge—[genetic] screen ever done in human volunteers. Usually you only want to test something important like a vaccine or something, a potentially finished product, in a human. You don't want to do basic research using humans.

But we've argued very strongly that we could benefit strongly from this, and gotten permission from NIH [National Institutes of Health] and the CDC [Centers for Disease Control] to do this. So we're gearing up to do that early next year, and that will be exciting, because we always rely on animal models and you never know if what you find in the animal has anything to do with the human.

VAN BENSCHOTEN: Exactly. Translates.

CAMILLI: So we're going to actually get data from humans. It's going to be very exciting, if it all happens. I give it better than fifty-fifty odds that we're going to do this next year sometime.

So anyway, I think we do have some potential for some interesting discoveries in the next five years, and in five years I hope to be firmly established in the pneumococcal field, doing really original important work. The *Vibrio* [*cholerae*] field I don't know about. I feel we're getting close to the end of our searching stage, and in fact, a lot of the projects in the lab are focusing on individual virulence factors, trying to figure out how they work, how they interact with the host. So for the *Vibrio* work, we're approaching the end of the discovery, searching stage, and we're entering the point where we have to focus on individual factors and just study them to death to figure out exactly what they do. And that's more difficult. That's more challenging work. You have to incorporate multiple disciplines. So I'm headed toward that.

I hope to take a sabbatical in the next few years, but that's a big argument. My wife is—She doesn't want to get up and go somewhere. Like I would love to go to Italy and do a six-month sabbatical and learn a particular field called proteomics, and have the kids learn Italian and us learn Italian. I have relatives there. My wife does not want to pack up and go. She'd have to quit her job. She doesn't get sabbaticals. So we're going back and forth on this, and she's going to win. There's no way we're going to pack up and move to Italy for six months. So I'd be forced to do a sabbatical in town, which is a joke. I mean, it's not what you're supposed to do.

But anyway, I need to learn some of these other techniques, and perhaps I'll do it by, maybe do two or three mini-sabbaticals, learning other techniques which I'll need to incorporate to figure out what individual virulence factors are doing. And I'm forcing my students to learn other techniques, too, like biochemistry, and the process I learned myself.

VAN BENSCHOTEN: How about in ten years for the lab?

CAMILLI: Whew. Ten years.

VAN BENSCHOTEN: Or is that too far ahead?

CAMILLI: In ten years, I would hope that through our efforts and others' in our field, perhaps involving us through collaboration, that we will have taken some of our basic research and come up with vaccines.

I've talked with some of my colleagues, like my close colleague across the street, Matt [Matthew K.] Waldor, who works on *Vibrio* as well. We've talked about, you know, ultimately we've got to put some of the stuff we're doing into vaccine design. His thought, and I agree with him, is we should do it ourselves. We shouldn't rely on others in the field to see our data, read our papers, and say, "Oh, let's try this." We should take advantage of our discovery and try vaccines. But it's a big thing to write the paperwork and the protocols to do vaccine trials, but we ultimately may have to do that.

So in ten years, I would like to see some of our basic research go into a new type of vaccine. For example, all our work in my lab on *in vivo* gene expression. By ten years' time, we should know pretty much what are the set of virulence genes that are turned on during infection and how can we have engineered strains to produce these things in the laboratory during growth in the lab. So that we could, for example, grow them up under— That special strain, growing under special conditions, whatever, that turn on all these virulence factors. Then kill the bacteria and use that as a heat-killed, or whatever, killed whole-cell vaccine. That should be much better than the whole-cell killed vaccine they have now. That's the simplest scenario.

Or design live attenuated vaccines, which we've really done— Intelligent strategies of manipulating them genetically to attenuate their toxicity, yet still allow them to colonize well, because you want them to colonize well and go through the motions, at least, in order to generate a good vaccine, a good immune response.

So anyway, ten years from now I would hope that we're starting to design vaccines and test them ourselves, because ultimately that's why we're doing our work, and that's what we want to do: prevent the couple of thousand deaths each year from cholera and the hundreds of thousands of cholera cases, and then the millions of deaths by pneumococcus.

Because that's the other thing. I always say this to people when I teach, and to my students and especially to my family. We've already made smallpox extinct, with the exception of some freezers. All of the obligate human pathogens, the ones that only live in the human, ultimately through efforts of people in my field, we're going to make those go extinct with good vaccines, and we are entering— A couple of decades ago we came up with antibiotics and had a major impact on human health. Now we're at equilibrium, where we can do no more with antibiotics.

Now it's the time to generate intelligent vaccine strategies, which also use information learned from immunologists, but intelligent vaccines that can wipe out these other pathogens. Then we'll achieve a new level of human health. Cheap vaccines that protect the Third World, as well as the developed countries, from common diseases, that's the future. Ten years from now, we should be starting to get there with vaccines. We should have a good HIV vaccine. We should have vaccines to cholera and some other things.

VAN BENSCHOTEN: Do you have any patents?

CAMILLI: Three patents.

VAN BENSCHOTEN: And what does that mean concretely to your lab, having these three patents?

CAMILLI: Nothing.

VAN BENSCHOTEN: Really.

CAMILLI: In terms of financial, nothing. Each time it's been relatively painless for us. Actually, four patents. I'm on two patents from my work at Harvard Med[ical] School with John Mekalanos, and I've gotten, you know, like maybe $1,000 in checks over the years. It's nice to every once in a while get a $300 check from a patent, but in terms of the money, it's totally meaningless, and definitely not enough to have anything to do with the lab. I have two patents here, actually one of which is submitted; we haven't gotten the patent awarded on it yet.

And I've always been reluctant to waste any of my time doing patents, but I have the university saying, "Yes, you need to patent this stuff." Now, recently, Tufts [University School of Medicine] got good at doing patents. They have a good office now that does it, which took very little work on my own part. So I went ahead and we filed a patent, and we're waiting. That's that huge stack of papers next to your head.

VAN BENSCHOTEN: Oh, wow, that is big.

CAMILLI: So, patents I'm not that interested in. I don't think I've ever had the type of discovery or invention that would make the university and myself a million dollars and be worth it. If I did come upon such an idea, I have no doubt I would work to get it patented, because I do believe in the concept of intellectual property rights.

So, I will say, related to that, though, that I have not really consulted much at all in the past. Occasionally I'll go do a single consult with a company, and give my opinion or whatever, something, and get a few dollars that way. But I do much less consulting than any of my colleagues, as I've learned. Others really rely on it, so they have, you know, ten or twenty thousand dollars income each year, because we're not paid well compared to our counterparts in industry. So, people rely on that.

I've never really gotten much at all. I'd say in my total career, seven years of being a professor, I've probably made less than $5,000 consulting. Recently a company came to me and

wanted to collaborate with me and to have me as a consultant, and they said, "Well, what would you—?" And they want to use some of the data we have. And they asked, "What type of arrangement would you fancy?"

And to their surprise, I said, "Research money for the lab."

Because they probably always get, "Consulting money."

I said, "Research money for the lab," because in this case it wasn't my time; it was data generated by people in the lab that they wanted, and this past year we were a little short on money, so I said research money, and they agreed to it. So we're in the final stages now of negotiation of some arrangement. Ideally what I'd like them to do is give me maybe half a student's salary, and in return, they'll get to use some of our data and we'll do a few experiments that will be of mutual help. They're interested in antimicrobials and diagnostics, whatever, so I think it's useful stuff.

VAN BENSCHOTEN: What about the argument that some people make, though, that, for instance, in the academic setting at least, academic patents, that it restricts sort of the free flow of information and actually might hinder science in the long term?

CAMILLI: Right. So, that's one of the things that I'm against. For example, this collaboration in the works right now had to do with data that we were ready to write up and publish, and I told the company that we're writing this up and publishing it. There was no discussion. I would not let any type of arrangement, collaboration, whatever, slow down the submittal and acceptance of a paper. If we're talking a few weeks to get a patent submitted, I would do it, but I could never feel justified in saying to a student that we're not going to submit this stuff until such and such happens. I'm never going to do that.

So for this company that wants to collaborate, I said, "We're about to write this up and submit it, and we're going to do it."

And they said, "Okay." And they even suggested, "Well, you should probably try and patent the stuff, too."

I went and talked to the people here at our technology transfer office, and they said, "Oh yeah, definitely. We want to patent that, (a) before you publish, and, (b) before you enter into an arrangement with the company."

So we hurried a patent application through, but all the while wrote the paper, submitted it, and got it accepted with no delay whatsoever. There are labs out there and professors out here who do willingly slow down the release of their data and release of their research materials for patent purposes, or collaborative purposes, or monetary purposes, and also, their research is influenced by their arrangements, and that's definitely a no-no. I mean, that's right up there with

falsifying data, which I guarantee you happens in the biological field, as well as other fields.

I feel I have strong ethics when it comes to that stuff, and that's one of the things I try and teach my students, mostly through example, but we do discuss this stuff as well. So I feel, you know, my wife makes more money than I do. She's in the business world. I don't feel like I need the extra money from selling off part of our research effort. If my wife quit her job and our standard of living was in jeopardy, maybe I would seek out more collaborations or more consulting, for example, where I would fly to some company a couple of times a year and give them advice. But so far I haven't felt the need or any reason to want to do that.

VAN BENSCHOTEN: A question about serendipity. Has serendipity played a role in any of your scientific research?

CAMILLI: Serendipity, I think, I feel a lot of the— First of all, being a scientist, being in the career I'm in had a lot of serendipity. Fifty-fifty odds that I would have become a scientist, based on things that happened in my life. So I feel very lucky to have such a great job and to be doing this rather than still being a busboy at the age of thirty-nine.

In our research I feel there has been some serendipity, but as much as one would expect, so the "prepared mind" thing, or doing the types of things where we expect some luck and we've gotten some luck. I don't think we've been overly serendipitous. I don't think we've been tremendously lucky in anything. In fact, I'd say we've been unlucky quite a bit. So I would have to say not much serendipity; about as much as one would expect.

The more research you do and the more chances one takes, the more serendipity comes into play, and so we put out as much as we would expect, to occasionally get a hit, some serendipity.

VAN BENSCHOTEN: Okay. I'm going to flip this.

[END OF TAPE 5, SIDE 2]

VAN BENSCHOTEN: This is tape six, side A.

You wanted to say something more about serendipity.

CAMILLI: So I guess what you're asking is what are the factors that advance our research and help us make discoveries, and serendipity plays a part, but I'd say no more than one would have expected, given our effort. What I think really has been the major component of our work has

been just that, doing work, hard work.

You've got the number-one thing is you have to be doing experiments. You can be the most brilliant person and have the greatest ideas, but if you're incapable of actually doing the experiments, sticking— You know, concentrating at the bench and troubleshooting an experiment and making it work, you're never going to get the data. So my modus operandi has always been "work hard." Do the experiments and repeat them and troubleshoot them and improve them, and get the data. Prove either yes or no to whatever hypothesis you're testing, without a doubt.

And my students, I train them to do the same thing. So they all put in fairly long hours, but they all have that stuff. I can't say all of them. One of them's been a wee bit of a problem. He doesn't really get satisfaction from doing experiments, a little bit— I would say quite distracted by things, unable to maintain his focus on experiments. But by and large, most of my students and postdocs will spend long hours troubleshooting an experiment, and in the end get it to work and derive tremendous satisfaction from getting that thing to work and getting the data.

And that is the central component of being a scientist. You've got to be curious. You've got to make a little hypothesis—that's important—and then do the hard work to test it, and do whatever it takes to test it adequately. You have to go talk to colleagues in the department, go read whatever you need to read, twist my arm into buying some expensive kit or piece of equipment to do it. But you've got to put in the effort to actually do the experiment.

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

CAMILLI: Nowadays, having seven people in the lab and my level of interaction with them, I would say these days it's like fifty-fifty. Half come from me and half they come up with. Occasionally they'll come in with an idea and run it by me and we'll together improve it or whatever, but I'd say a good half the ideas—and these are usually small, incremental things—come from the lab, and half from me.

Either during our lab meetings or in one-on-one conversations, I'll suggest something. Usually, in one-on-one conversations or lab meetings, more often than not it's me that spits out an idea, and mostly it's just I'm comfortable spitting out ideas, and they may have the same ideas but don't say anything. But so it's like fifty-fifty. Overall, you know, in my seven years of being a professor, and in most of my postdoc, I would say the vast majority have come out of my head.

And I have to say, getting back to an earlier thing about having a foundation of knowledge in your head, I've built up a foundation. I don't think I'm as knowledgeable as a lot of my colleagues. I don't have the memory that others, like Dan [Daniel A.] Portnoy has, and don't have maybe the same great intuition as him, but I have a fair amount of knowledge and it's enough to work with. I draw a lot upon stuff that I can't bring up, but I still feel has a role in

my thinking. I guess subconscious memory and foundation plays a role in some of my intuition and some of my decisions.

So I have to say, I do feel I have one strong suit which has been tremendously helpful, and that's I think I have a really good imagination. So I will often have these periods where I'm thinking about something and I'll just stop myself and I'll say, "You have a narrow focus here. Expand the possibilities when you're thinking about this particular problem." And so I'll rethink it. You know, when people talk about imagination, you mostly think, well, it's just innate; it's something you're born with. But I don't think that's true. I think your imagination draws upon your prior experiences and your prior concepts that you have in your head.

And so I think one of the things I benefit from— I mentioned earlier that I love to read about other fields, even if just topically. I just love to read about astronomy and physics and particle physics and god knows what. I suck a lot of that stuff in. And you learn different types of concepts, and I think those come into play when I'm using my imagination or whatever to think about our projects specific to the *Vibrio* [*cholerae*] stuff. So I think I do have a fairly good imagination, and often come up with things that I attribute solely to just being imaginative.

So, I feel in many ways mediocre about a lot of my attributes of being a scientist, and I truly believe it, that I'm pretty— Mediocre intelligence, mediocre memory, maybe even less than average memory, especially recently, mediocre in terms of my collaborating with and working with other people, but above, definitely above the average imagination, and above the average in work ethic.

So I think work ethic is the number-one thing. Got to have that. It takes a number of things to be successful, in my book, and I tell my students this, a tremendous number of things. You've got to write; you've got to be able to work with people; you've got to be able to read stuff. But the number-one thing is work ethic. If you aren't sitting there at the bench doing experiments, nothing's going to happen, even the best of intentions. Imagination is probably of lesser importance, but someone in the group's got to have some imagination, so I think I have a healthy dose of that.

VAN BENSCHOTEN: Did you take, at any point in your education, history of science classes?

CAMILLI: Nope. Never did. Read— I told you earlier that I read a few biographies of scientists that just made a huge impact on me. Not many, but a few that I did, I saw the life of the scientist, and that really attracted me, some early in my life, before I ever knew I was going to be very interested in actually doing science.

But there's one book [The Microbe Hunters, by Paul de Kruif] that a lot— Actually, I've learned that a number of people in my field have read this book, and, of course, now I'm blanking out on the damn thing, but it's a book about— Each chapter is a short biography of a particular scientist, like [Eli] Metchnikoff, who discovered phagocytic cells, or [Louis] Pasteur.

What is this book? I'll think of it before our interview's done. But that book made an impact on a lot of people.

So anyway, I would say I haven't read much. I'm not well read overall, but I did read a few biographies that made an impact on me, because that gets back to the adventure thing. I saw these people as adventurers and that's how these things were often written. There was always a little bit about the power struggles and the interactions with the foes and friends, but ultimately I see them, a lot of these people, as being explorers.

VAN BENSCHOTEN: How useful for your own work and research is knowing something about, say, the history of your particular field, bacterial pathogens? Or maybe even a little bit broader. Pull it back, in biology and microbiology.

CAMILLI: It's definitely important, and there's no way you can not get the essentials from the current type of graduate education. So, you know, the field's new enough. Basically, everything was done in this century [twentieth]. All of the molecular stuff was done this century [twentieth].

But you definitely get instilled in you the start of being able to study the microbial world when you hear about Pasteur and [Robert] Koch, and these people that isolated the microbe for the first time. And [Antonie] van Leeuwenhoek, who discovered the microscope. Everyone knows those stories, because they're just so fascinating. Van Leeuwenhoek discovered the microbial world. With his little teeny magnifying glasses he could see the amoeba and the other things; discovered a new world, a new frontier.

And then Koch and Pasteur and others at that time in the late 1 800s purified the pathogen from infected tissue. They would purify the pathogen. That's colonies on the surface of a petri plate, or they even started with just slices of potatoes. So a single cell would be diluted out from the mixture, and land and grow as a colony, and you could pick that pure colony, re-infect an animal. The animal got disease, and you could re-isolate the bacteria again from that animal in pure form. That proved the microbial origin of many infectious diseases. So that's the foundation. That's the start of microbiology, and everyone knows that. We all learn that.

And then the more recent stuff that's happened this [twentieth] century, the discovery of antibiotics, the discovery of DNA as authentic [genetic] material, we all have learned that. There is concern by the older faculty that the new students we train know absolutely nothing, and I've gathered that that happens continually from generation to generation. And I sometimes argue back to people that these students know things you did not know, and don't know. And so it's always a constant— There's no dumbing-down going on. It's just a change in what the area of focus is. So I think they all get a decent background in history, but we don't [teach] a history of science. That's at the undergraduate level when we get that. We have no offerings here on that.

VAN BENSCHOTEN: A question on technology. What effect do you think technology and technology innovations had on your science?

CAMILLI: Oh, incredible impact. That is one of the most critical things in driving our research, and others' in the field, is technology, like PCR [polymerase chain reaction]. We have five PCR machines out there. We have more PCR machines than any other lab in the department and they're all being used daily. So that technology has greatly facilitated our work.

So we're heavily reliant upon technology, and we're using new technologies. Like our big paper last year in [the journal] *Nature* came from the newest technology, DNA chips, microarray analysis. And we'll continue to move forward. One of my grants that's submitted wants to move into proteomics, so that bypasses DNA microarrays and goes right to looking at what proteins are being produced during infections.

So, looking at gene expression with microarrays tells you what genes are on and off. That's not always reflected in the accumulation of the protein products of these genes. That's really what you're interested in, what are the proteins, because those are the antigens for vaccines. Those are the things that are doing, acting as enzymes or whatever. So proteomics is the latest thing where you actually can use large-scale techniques and biochemical techniques to look at all of the proteins that are being made within the bacteria at a particular point in whatever environment you're growing them in. So, yes, new technology is very important.

I think I've already hit the point where I'm not assimilating it as much as I should. That's part of this changing over to new generations. I don't have the time to, you know, read all these ads for new equipment or new techniques, whatever, that are bombarding me from pharmaceuticals and biotech companies, and so I'm already slowing down in my ability to assimilate new technology. So in a way, I rely on the people in the lab to come to me and say, "This would really allow us to answer this question in a faster way or a better way."

And that's why you need grant money, to say, "Try it. Here's $900. Go buy that and do it." The field and the public [taxpayer] is [are] ripped off by the price of the research materials, because there are very few users for some of these things and so they charge outrageous fees. And when you have large grants from the government, you don't care. You just sign the bills and say, "Okay. Buy it."

So there could be more oversight in science, but it's supply and demand. It's a natural outcome. When a researcher is low on money, they'll scrutinize. Like for the past year when I was running a deficit, my supply budget was half of what— I was spending half of what I'm spending now, and our research was still going forward. Now that I'm flush again, I'm spending literally twice as much, and I guarantee that happens everywhere. The bigger the lab, the more grants the person has, the higher the percentage of waste there is.

VAN BENSCHOTEN: Competition. Is competition generally good for science?

CAMILLI: Definitely. You've got to have competition. You've got to have peer review of papers and grants. I always fantasized about just having no responsibilities other than my work, to be given money *ad libitum*, infinite amounts of money to buy whatever I wanted and do our research. In my thinking I would think, I'd say to myself, "You'd make much greater discoveries and advances under those conditions."

But that is very unrealistic, and probably out-and-out inaccurate, false. It would not work that way. That would be golden handcuffs on my creativity, my work ethic. So you have to have peer review to force you to go back and redo the experiments in another way, or prove it statistically, or increase the biological contribution of your work. You've got to have that in a peer review, even though it's painful.

And grants, got to have peer review in grants because we're all fighting for the same pot of money, and the United States does the best science in the world because of our peer review system. In other parts of the world it's the top dogs get all the money, and there's a hierarchy there that is not totally based on merit and original ideas, etc., etc. And that's one of the reasons that I think the U.S.—not only the volume of what we publish, but the quality of it—is the best in the world, and I think it's because we have the best system. And peer review is central to that.

VAN BENSCHOTEN: Have you ever been scooped?

CAMILLI: No. I've not been scooped. There have been a few things that could be seen as being scooped. Like the streptococcus project where we do this negative screen to find the central virulence genes, we had that idea and started doing it, and it was a very long and hard project. During the two years that we were doing the project, two other groups published papers doing the exact same technique in the same species of bacteria. In a way, those were scoops.

However, both of those reports were very small-scale screens and weren't quite done in the best way. So we felt that when we published our paper and we got it accepted, and it just came out, that we did it in a much better way and a more thorough manner. So, I've never truly been scooped completely, where I was just totally depressed and we did not publish that body of work. So I guess I've been lucky.

VAN BENSCHOTEN: What criteria do you use in choosing one research project over another? What are the important determinants about what flies and what doesn't?

CAMILLI: Well, ultimately, probably the most important thing is it's based on what I and the students perceive as the most interesting thing, and that doesn't always correlate with what's probably the most important thing to be studying. Like we're often sidetracked by something

that's just incredibly interesting, but ultimately turns out to be a distraction.

One kind of flaw in the way we do research here, and it's totally attributable to me, is what we study is very broad and very diffuse. We have a bunch of different tangents that we take because it's just interesting, but at the expense of focus. Like many [other] labs, everyone in the lab is focused on a different part of the same question and so they move much deeper in a much more rapid manner on a particular biological problem. We are moving on many different areas more slowly because of that. That has actually, I think, probably hurt my grant success in the past, because I'm not able to go in with a very focused grant with a ton of preliminary data, and it's because my interests are in what is interesting to me, and not what is best for my grants and moving the field ahead. So in a way, it's a little bit selfish; maybe a little bit stupid. But that's what we do.

So, the number-one thing is interest. Then second comes how does it fit into the project, like is it helping the project? We wouldn't take a tangent that totally has nothing to do with the main project. And the students actually keep me honest in that respect. Like they will often—I'll suggest something, and they'll say, "Well, this is going to take up all of my time for two weeks, and it doesn't really get to the main point we're after here."

And I'll say, "You're absolutely right." That happens weekly, something like that.

So, yes, interest, curiosity, and discovery are number one. Number two is how does it fit into the project? Is it the type of experiment that is going to help us answer the ultimate question we're after answering there?

And then a third thing is feasibility. Nine out of ten experiments out there [in my lab] fail, and if a student's had a string of failures and has tried to troubleshoot and still is failing, feasibility becomes number one. Before they lose their self-confidence or drop out of grad school, get them on a series of experiments that are going to work and give them some data, even though it's not maybe the main thrust of the project they're working on.

Because really, science, and especially for the young, the new people, you've got to have this positive feedback. If you go for months without any experiment working, that's just devastating, and really can change one's outlook on where they're headed and what they're doing and whether they want to continue in science. I haven't had anyone drop out, but I've had students who have questioned what they were doing. And twice I've been able to steer them back and get them back on the road. One has decided to finish out his thesis and go on and become a teacher, and leave the research field. This is the student that really can't focus, really isn't interested completely, interested in doing benchwork. So, anyway.

VAN BENSCHOTEN: We just talked about criteria you use for determining your projects. Let's sort of take it up another level, sort of on the national level. Clearly, there are institutions to determine the national research agenda, the NSF [National Science Foundation], the NIH [National Institutes of Health], other organizations as well. To your mind, the national research

agenda, does it work effectively? Are we, in other words, doing the research as a nation that you believe should be done?

CAMILLI: Yes, I do. Some people don't understand— You know, people outside of science don't understand how many, many labs can be doing basic research, where it looks from the outside that we're just following our whim and having fun cooking up new recipes and exploring things that have no bearing. But the system in the U.S. works because there's enough basic research going on that we can expect the big discoveries every so often that make tremendous impacts, either technological advances or diagnostic or therapeutic, whatever.

There's enough basic research going on that we expect and get a certain number of discoveries each year that are big advances. So I think the greatest thing about the research in the U.S. and the way the government lets the research happen is they fund basic research. You know, if people in [United States] Congress looked at my projects closely, they'd probably take my money back and say, "They're doing things that— " I'm probably overdoing it there a little bit. But they wouldn't understand the basics and the basic research areas that we're looking into.

Fortunately, they don't see the individual projects. They see the overall productivity and the discoveries that really impact human health and technology. So the overall agenda, I think, is, well, the federal government lets us do our thing. They see the total outcome and they give us the money.

At the next level are the scientists who are the people at NIH and other institutions like National Science Foundation, and those people, I think, do a pretty good job. You know, the people at NIH, the scientists, have a good feel for what areas need funding, and so I think at that level they're doing a pretty good job as well.

Things happen. Like all of a sudden this bioterrorism money that the government's throwing at NIH. I feel happy, because it's going to [positively] impact me. My specific study— My institute at NIH, NIAID, National Institute of Allergy and Infectious Disease, is getting a boatload of money this year, next year, and for many years to come, that's targeted toward bioterrorism. So it's going to help me out, because cholera is a potential bioterrorism agent.

So I'm happy at one level, but on the other hand I feel they're throwing too much money at us too quickly and that we're not going to get— We're going to spend it, but we're not going to produce the type of result they want. They want in five years' time incredibly rapid and accurate diagnostics and vaccines to protect against a number of potential bioterrorism agents, and we're not going to produce that.

That's the president and the federal government throwing that money at us and expecting immediate results, and they're not going to get it. Maybe diagnostics. There could be some major advances made in diagnostics, but in terms of vaccines, they don't understand the incredible complexity of generating a vaccine that works, because what they fail to understand is that these pathogens have often been interacting with humans [and ancestral species] for

millions of years, and there's been an arms race between our immune system and the pathogen.

And the level of complexity— You can't just go in and tinker with something and expect it all of a sudden to have made the discovery, because your immune system would have done that a long time ago. So often, the one thing I think about is we need to try things that nature never would have tried or is incapable of trying. But then I say, "Well, like what, for example, Andy?" And I can't come up with anything. It's very difficult.

VAN BENSCHOTEN: Well, periodically, projects come up, like stem-cell research, for instance. I mean, an earlier one would have been recombinant DNA. As you know, there is a great brouhaha about that and whether the public should be involved in determining whether it's used, how it's used, etc. The same thing has come up with stem-cell research.

Then the next question is, then, who signs off on stem-cell? Who determines, in other words, the direction, at least at this particular point in the national research agenda, whether this goes forward or not? If someone put you, let's say, at the head of a commission in order to determine whether that should go forward or not, who would you put on that commission? Not by name, but just generically. Who would be the people who would sit there?

CAMILLI: I would want a couple of leaders from fields who would benefit from the stem-cell research. I would probably put together a mixed group that maybe not would be representative of society, but would have opposite arguments, so they can discuss it, argue about it. So I'd put a few anticloning, antigenetically-modified-food people. I'd put a couple of people who are knowledgeable about these topics but could facilitate the debate, a couple of people of reasonable— I don't know who these people would be. I guess politicians that are open to hearing both sides of the story and coming up with ideas that—maybe not compromises—could put all the information together and discuss it that way.

Anyway, I'm a little biased about this type of thing, because, for example, stem-cell research, I would give it the green light without a second's thought. Knowing what I know, I would give it the green light. I think the goal, the job of the government and policy makers should be to do just that, make policy. Put into law that you cannot clone a human, and just leave it at that. Fund research that uses stem-cell research for all of the good things that people are saying it could be good for. In the United States, we should be able to control that. Like people are not saying they're going to do one thing with the money and then they go back to their government-funded lab and clone humans in test tubes. No one is going to do that.

Other people are going to do it. Companies are going to do it. Foreign scientists are going to do it. And so I'm a little bit of a fatalist in that I have absolutely no doubt that people are going to clone humans, no matter what the U.S. policy makers say or do. It's going to happen, and that's that. Let it happen.

We'll probably go through a stage that we will regret, where people are cloning humans

and children are being born with severe defects. When that happens, cloning's going to stop and there are going to be a lot of questions that are going to be asked, but ultimately we're going to work through that and people are going to figure out, okay, well, what are the problems? How do we clone so that children are born that are healthy, mentally and physically?

And most people would be scared to hear me say something like that because I believe that we have to go through all this and reach that stage and then deal with the ethical problems of cloning humans, because I have no doubt that we're going to reach that stage someday, whether we want to or not.

There's been no invention by scientists that has not ever been used, including the A-bomb. I don't know if fatalist is the word I should be using for that, but my belief is that any invention that has some potential good is invariably going to be done, tried, and used. All we can do is control the conditions under which that is brought forth. We can't stop it. You can't stomp it out. So I think it's ridiculous to outlaw stem-cell cloning. It's just crazy, absolutely ridiculous.

VAN BENSCHOTEN: I mean, on this hypothetical commission, you leave room for open-minded lawmakers. Open-minded lawmakers are influenced, of course, by their constituencies. Their constituencies, at least with stem-cell research and other issues, are sort of at the mercy of the media in many respects, Time and Newsweek, when they try to explain what is stem-cell research and why should we care. They also rely upon scientists, and this gets into sort of public policy. What role should scientists play, in your mind, in helping to increase the science literacy, let's say, of the American public?

CAMILLI: Yes, that's very important. I actually derive great pleasure out of talking about my research or other types of research to lay people, which I do, I find myself doing quite often. I'm not sent out by the university [Tufts University] or whatever to do that, but I often find myself doing that, and I enjoy it very much, and I love to see people's faces when they finally understand something that was bugging them, that they didn't understand. So I guess I derive some pleasure out of teaching. I also like to get into debates with people who feel strongly about a certain thing, and we argue back and forth about things, because I'm always prone to revising my opinion on something when I hear different people's opinions.

I'm losing track here.

VAN BENSCHOTEN: The role of scientists.

CAMILLI: Oh yes. So I think scientists interacting with lay people, politicians, media people, is incredibly important, and there's a lot of effort now to do that.

Maybe it's more hype than actually put into practice, but there should be a million mechanisms out there, some of which force scientists to educate people, whether it be kindergarteners, up to meeting with local politicians or whatever, because scientists, and rightly so, are often perceived in a skeptical, if not evil, light by a lot of nonscientists, and it's because our inventions are often used for purposes by nonscientists for whatever means, or whatever objective there is.

So, you know, science originally was devising better means of warfare, so that's given science a bad name. But as far as stem cells go, I'm a strong proponent of education. Anyone that is knowledgeable about it or has a vested interest in it should be educating as many people as they can.

And reporters are key. Actually, I was learning that. A neighbor of mine is a former reporter. He now works at the Department of Education for Massachusetts, and is heavily involved in the MCAS [Massachusetts Comprehensive Assessment System] scoring and policy. This is an exam that high school students have to take and pass before they can graduate, and it's a huge debate right now about should students be forced to pass this before they can graduate, and should we grandfather in all the students who were educated before the MCAS became a requirement.

Anyway, he's a statistician, but he was a former reporter. He has a tremendous relationship with reporters at newspapers because he was a former reporter and he knows a lot of them. And I've learned from him. He is now almost like a scientist. He's a statistician, and yet is able to— He has a very good relationship with reporters. He understands the reporters and what their job is, and he can communicate with them very effectively. And he gets his story across. He gets across the Department of Education's side of the story. Why is the MCAS important? Who are the students that are failing? How can we improve the curriculum? Etc.

So his viewpoint is now being brought to bear down on the education system. The education system is much more numerous and more of a powerful lobby than parents of students who failed MCAS. Incredibly strong constituency there. They should be winning the battle and they're not, and it's because of smart guys like my neighbor who get their story in the newspapers. So anyway.

VAN BENSCHOTEN: Have an outlet.

CAMILLI: Right. So I think interaction with reporters is incredibly important, and that's one thing I don't hear— I hear a lot about, you know, we need to educate the public, and we need to educate the policymakers. But I don't hear anyone saying, we as scientists need to seek out reporters and get our story across. That seems like the most obvious thing, because that's the mass media.

VAN BENSCHOTEN: Okay. We're at our time.

[END OF TAPE 6, SIDE 1]

[END OF INTERVIEW]

INTERVIEWEE: **Andrew Camilli**

INTERVIEWER: **William Van Benschoten**

LOCATION: **Tufts University Medical School**
Boston, Massachusetts

DATE: **11 October 2002**

VAN BENSCHOTEN: This is tape six, side B.

You had talked earlier about competition. And the other side of competition, obviously, is collaboration. First of all, maybe generally, what is the role of collaboration, do you believe, in science? And then, if there are any specific collaborations that you're participating in right now, if you could talk about those.

CAMILLI: Sure. I think collaboration is an amazingly good way to make important discoveries, and I was really taught that early on in my career from Dan [Daniel A.] Portnoy, who always told me, "You've got to collaborate. Collaborate with people with other areas of expertise." The reason is that you get two people with different backgrounds that get together to tackle a problem, and you come up with ideas, experiments, and results that either group would never come up with on their own. So it's kind of an obvious thing. There's a synergism there, and I've seen it over and over again. It's absolutely true.

That's one of the reasons I try to keep, at least at a topical level, well read in these other disciplines, not just in my own field, but in other fields. Like I read immunology, which is an important synergistic field with microbiology, or potentially synergistic. And even fields afar, like physics and biochemistry and things like that, I try to keep a little bit read in those things, just so I— Because although I know the importance of collaboration, I don't excel at it. I don't easily seek out collaborators. Most collaborations have been people that have come to me, so although I realize the importance of it and value it, I'm not very good at doing it. And it goes back to my kind of, I guess, shyness or whatever. I'm not willing to go out on a limb all the time with folks.

So, okay. So that's why I think it's very important, and I have had important collaborations, beginning with during my graduate career. While I was being trained in Dan Portnoy's lab, our lab collaborated with this guy named Lou [Louis G.] Tilney that I mentioned earlier, who's kind of a famous cell biologist. We did a project together that I never could have done. I never could have gotten as nice an answer to the question as I did in this collaboration. His lab had expertise fixing specimens and sectioning, and electron microscopy, and I had a biological problem: What's the role of this phospholipase in Listeria's infectious cycle?

Getting together, we proved that this phospholipase is needed to break open the phagosomal membrane, because when you make a null mutation in the phospholipase gene and do accurate measurements of how many bacteria are still in the phagosome versus out free in the cytoplasm of the host cell, we found a very strong, significant difference between them. So anyway. And that collaboration went far and above that, my own experiments, because through that collaboration between our lab and Lou Tilney's, we actually established a very important paradigm.

I had a little bit of a role in that, but it was really Dan and Lou Tilney's thing, and that is that *Listeria*, once it gets into the cytoplasm, can spread from one infected cell to another just like a virus would do. It never really leaves the cell. What it does is, it polymerizes host cell actin behind it and shoots out of the cell in a kind of— It's called a pseudopod. We call it a comet, and *Listeria's* at the head of this comet of polymerized actin, and it hits the cell membrane and punches out the membrane with *Listeria* at the tip, and the neighboring cell phagocytoses that pseudopod, and then the bacteria had spread to— It then breaks out of the double membrane vacuole, and is now in the cytoplasm of the next cell. And it was never exposed to the extracellular milieu, and that's what some viruses do. That's what *Listeria* does. It's a way to avoid antibodies and other things, and move, spread cell to cell in tissues. So that's a central feature of *Listeria's* pathogenesis.

And Lou Tilney who's, it turns out, an expert cell biologist, had studied actin polymerization. When they first looked at some scanning EMs [electron micrographs], or actually, it was transmission EMs, Lou Tilney saw this comet tail behind Listeria and said, "That's actin, polymerized actin." They made a major discovery together, the two labs.

So I saw first-hand, collaborations are extremely important and valuable. So in my own work I've had some fruitful collaborations. I guess one thing that comes to mind is really unpublished work right now, but it's pretty interesting. We've collaborated with— Well, actually, I tell you. There is one collaboration that was published. That's the subject of our Nature paper last year [D.S. Merrell et al., 2002. Host-induced epidemic spread of the cholera bacterium. Nature 4 17:642-45.]. And this may be, who knows, my most important discovery so far, my lab's. *Vibrio cholerae* causes this severe diarrheal disease, cholera, and we discovered that the bacteria that come out of humans in this profuse watery diarrhea are hyperinfectious.

The reason that's interesting is that cholera causes these very explosive epidemics, where it spreads tremendously fast. Nothing comes close to cholera for rapidity of spread. So it comes out into ponds, rivers, estuaries, whatever, and then someone else ingests the bacteria accidentally and then they get cholera and the cycle is repeated. So our observation that the stool bacteria are hyper-infectious led us to hypothesize that this could be a very important factor in the rapid epidemic spread.

So we made that observation and then we wanted to ask the question, well, what's the special properties of these bacteria that make them hyper-infectious? What I mean by hyper-infectious is it takes less bacteria to cause cholera in the next victim, so a reduced infectious dose, we call it, decreased infectious dose. And that's a critical parameter during infection, is

what is the infectious dose? Does it take on average 10,000 bacterial cells to cause cholera in a new victim, or does it only take 1,000 cells? The difference between those two is the difference between an explosive epidemic in a population, versus slow spread or no spread.

So anyway, we had these watery stools from cholera patients in Bangladesh that were just chockfull of *Vibrio cholerae*, almost pure *Vibrio cholerae*, and we realized that we could isolate the RNA from these bacterial cells in relatively pure form, because there were hardly any other bacteria there, and very little human cells. So it was almost pure bacteria. We collaborated with a guy named—this is one of my nervous tics is forgetting obvious things—Gary [K.] Schoolnik, who I had talked to a couple of times.

We were actually collaborating over another project prior to this. He does DNA chips, microarrays, and is doing very good work in that. So when we realized that we had these, we could do microarrays with the stool *Vibrio cholerae*—in other words, determine what are the patterns of gene expression of all the genes of the bacteria—we teamed up with Gary. We collaborated with him and very quickly his lab, who has expertise in doing the microarrays, it literally was three weeks; we had the complete gene expression profile of the stool bacteria. So we had all the information in front of us.

Well, we had one important set of information in front of us that might explain why they're hyper-infectious. And actually, there are some interesting data in there, some surprising data, and some data that gave us a hypothesis that we're now testing for why they're hyper-infectious. So that was a sheer, obvious mutual-benefit collaboration, because again, we had a biological question. We had a phenotype that was a very important one to study, and here was a group with technical expertise.

And I've had collaborations that go the other way, where I'm the one with the technical expertise and they're the ones with the biological question. So that was a fruitful collaboration. Now, with a former postdoc[toral fellow] of Gary Schoolnik's, a woman named Fitnat Yildiz, who's now at UC [University of California,] Santa Cruz, we're collaborating with her on— So *Vibrio cholerae* is a facultative pathogen, also lives in the environment. And we would like to know, what is its life cycle? What is its lifestyle in the environment?

There's a permanent reservoir there. It turns out it lives in association with blue-green algae and other types of plankton, and so we actually started growing blue-green algae in the lab and adding *Vibrio*, and sure enough, the *Vibrio* colonize or associate with the algae and grow much better, survive for much longer periods in water. And we're collaborating now with Fitnat Yildiz, who actually— Right across the hall from her was an algae lab, experts at growing the algae, genetically manipulating them, etc., etc.

So we've collaborated with her on learning about what genes *Vibrio cholerae* needs to associate with the algae. So not only are we studying pathogenesis of cholera, we're learning about its environmental phase of its life cycle, which is actually very exciting for us and others, because it's a completely untouched area, to understand its environmental reservoir.

So anyway, I don't know if that's enough about collaborations, but I'm a very strong proponent of it. I think it serves a major function and it should be and I think is being encouraged by all levels of not only our peers, but funding agencies, whatever. The writing's clearly on the wall that collaboration is a good thing, so, for example, program project grants which encourage collaboration are very important.

VAN BENSCHOTEN: On the other side of this tape we talked a little bit about public policy and science literacy and sort of the role of scientists in helping to create greater science literacy in the American public. Does your institution here at Tufts [University Medical Center], does it encourage you and other scientists to participate in public policy debates?

CAMILLI: Not really. There is the occasional, once- or twice-a-year e-mail, you know, the mass sending of the e-mail to all the department faculty to contact your local representative or congressperson about a particular subject, but invariably that subject is something of critical importance to us as scientists.

For example, last year there was a bill put forth—I think it was a bill—to include mice in a very strong animal protection set of rules. So, right now, if you do experiments with a dog or a monkey or any type of, I guess, higher life-form animal, the set of rules, the paperwork, the various permissions you need to get are just tremendous. If you want to do experiments with mice, the paperwork is tremendously reduced. I mean, you still have to write a protocol and show that what you're doing is ethical and of scientific value, etc., etc., but it's still an order of magnitude less obtrusive and time-consuming to work with mice.

So there was a bill put forth to include mice into that higher degree of protection, and there were a flurry of e-mails to talk to whomever you can, write letters to your congressmen and women and fight this bill, because it would really have a serious negative impact on our research. So anyway, that type of thing happens. So I would say we are not frequently or strongly encouraged, I'd say at a formal level, to educate or interact with politicians or other people.

On the other hand, verbally, it's frequently said, you know, several times a year, how important education of the lay people is. We're all aware of that, and I think in our own private lives many of us make a conscious effort to educate those that we can, but we don't really reach people on a mass format. So, that's the answer, is I think we're not encouraged that often.

VAN BENSCHOTEN: Let's talk a little bit, before we get to your lab and the composition of it, about biotechnology, biotech labs, which are booming. In a recent issue of The Scientist it was claimed that two-thirds of all R & D [research and development] now for basic science or scientific research, comes from the private sector. As you know, private companies are driven mostly by profit. They have other reasons for existing as well. How do you feel about the rise of industry labs, and the growing privatization of scientific research?

CAMILLI: Well, I guess the real question is, is it coming at the expense of publicly funded research, and I don't think it is. So I think it's fine. That's a free-market economy, and that's the way it works.

There is an interesting ethical question about what's going on in some of those. This gets back to the stem cell and cloning, applications for cloning humans, what's going on in these private companies. The government has to make rules that all research in this country abide by, such as no cloning of humans. So I do think there needs to be oversight of the R & D, and that probably could be equally said for other areas of research that could have potential deleterious effects on humans. I can't think of any examples.

So anyway, I don't think that private funding is coming at the expense of publicly funded research, so I don't think there's a problem there. I do— My own personal feeling is that I feel that much of the research that's being done in industry will never be made public, and that's a tremendous waste. But there's almost no way around it. I'm pretty ignorant in this area, but maybe there should be some type of, you know— Well, no; it'll never be done. I was thinking there should be like a ten-year release-all-data clause that everyone has to abide by, but they would never do that for reasons of culpability, like testing of products and someone ten years later gets cancer or whatever. You'd be able to go back to those records. So I don't know, I guess that data never ever gets released, most of the data. So in a way it's a waste but that's, I think, a necessary side effect or negative side effect of the free-market economy, so I don't see anything wrong with it. I mean, that's just the way it is.

VAN BENSCHOTEN: Let's turn to your lab, the composition of it. How big is your lab? I think you might have told me this before.

CAMILLI: Let's see. Right now I have seven people. I have four graduate students and two postdoctoral fellows. Both [postdoctoral fellows] are physicians who've come to learn to do research. Both are international. Carlos Osorio is from Chile. He's an M.D.-Ph.D., dual degree, so he has done some research before, but he's come to learn to work on *Vibrio cholerae*. Cholera struck South America big time, starting in '91. So he's from Chile. They got hit pretty hard, so he's very interested in studying that organism. And Carlos is fantastic.

Carolyn Hemsley is from England and is a physician, and has come to learn to do research. So she has a nice four-year fellowship to learn to do research. She's on maternity leave right now for a three-month period. She's also fantastic, very smart, excellent hands at the bench.

Then I have four grad[uate] students, one of whom is Irish, an Irish citizen who's here in graduate school. Then I have three American students, and then I also have one rotating graduate student who's here on a temporary basis, and new technicians. So that's the

composition. Should I go through all their names?

VAN BENSCHOTEN: Yes, okay. Might as well.

CAMILLI: My senior grad student is Michael Angelichio. He's the one student who's moving kind of slowly, his project. He's now applying for jobs as a teacher, which is a good choice for him because he's an excellent teacher, actually.

Next is David [L.] Hava, who's the student I mentioned doing the Streptococcus pneumoniae work, and he's fantastic. He is one of the best students I've had.

Then I have the two newer students. Oh, you know, I gave you the wrong count. Actually I have a brand-new student, so hold on. So then I have two third-year students, Susan [M.] Butler, who's from Ireland, working with *Vibrio cholerae*, and Anna [D.] Tischler, who's working with *Vibrio cholerae* as well. And then, I'm sorry, I have the one— I have a fifth student, a new student who just started a month or two ago, Julie LeMieux, who is now working with *Streptococcus pneumoniae*, and Dave is helping train her before he leaves, because he'll be graduating in a couple of months.

So actually I have three working on the *Streptococcus*, Dave, Julie, and Carolyn Hemsley, who's on maternity leave. In addition to that, I have the rotating grad student. So I have, luckily the woman on maternity leave, her bench is available. So I have seven people in the lab, physically, right now, when there's really only space for six. When Carolyn comes back from maternity leave, we're going to have a serious space problem for a couple of months until my new lab is built.

And I have an additional— I've given the okay and signed the dotted line to take an additional postdoc, Alan [E.] Basset, who's from France. He's getting his Ph.D. and has applied for fellowships, but I've guaranteed him two years of salary regardless of what he gets, if he gets a fellowship or not. But he should get one. He'll come and work on *Streptococcus* as well, but he won't arrive until early next year.

VAN BENSCHOTEN: What is the basic structure of the lab? You said, I think, earlier, that you have meetings every Friday. Do you have journal club?

CAMILLI: Yes. Our department has a number of forums for speaking and presenting data, so every Tuesday we have what's called research report. A student or a postdoc from within the department gets up and talks about their work. In every year, a student and postdoc has to go at least once. So in the typical five years that a student's here, they'll have given five research reports, at a minimum. So you really learn how to talk and think on your feet here. It's a good thing about our department.

Then on Wednesday we have an outside seminar speaker speak at noon, and then on Thursday is what's called journal club, where a graduate student, occasionally a postdoc and occasionally a faculty member, will present one or two exciting papers that they've read to the department. That's not really related to their work, or it's not their work, but it's just some exciting and important body of work that's just come out, and they'll get up and discuss the paper. So we have three things per week, and then virtually every lab in the department has their own lab meeting, so that's a fourth meeting per week.

So there are lots of responsibilities and formats for hearing about research. So, yes, my lab has our own lab meeting on Fridays. We used to have a joint lab meeting with two other labs, and it just got too big. As the labs grew, we had like thirty people in this small room, and my students would only get to speak once or twice a year, and that's not what a lab meeting is about. A lab meeting is to keep everyone abreast of your project, and not have to give all the background every time you get up there to speak, so it should be a more rapid frequency of presenting. So now we've dissolved that joint lab meeting, which we had started, and have our own small lab meeting, actually here in my office every Friday. We eat our lunch and someone talks.

VAN BENSCHOTEN: I've got a few questions about gender. How many women PI [principal investigator]s are there in your department here? We don't need an exact number; sort of a ballpark.

CAMILLI: So, the chair of the department is a woman, Catherine [L.] Squires. And then we have one, two, three— I'm missing anybody? So then including the chair we have four females, and then eleven males. That's kind of typical of a microbiology department. The biological sciences in general have a lot of women. Some places it's 50 percent. I think the students that are getting trained in my field is 50 percent across the nation, 50 percent male, 50 percent female. We do have a deficit of minorities, especially African Americans, that enter our field, for whatever reason. So our department's pretty average in terms of women.

VAN BENSCHOTEN: Given your own experience in grad school as a postdoc, as a PI do you feel that the playing field is level between male and female PIs in the biomedical sciences?

CAMILLI: No, it's not. It's uneven, and you know, that's true across the board and a hell of a lot worse in other areas, so I feel it's [relatively] good [in my field]. It's pretty close to parity, but there's still an unevenness, and it all boils down, and I now know first-hand about this, having two children, it all boils down to family.

Women are the primary caregiver. They carry the child, bear the child, and are usually the primary parent that takes care of the kids, and that has a negative effect on the woman's

career. And I'll just talk about my field. In my field, you can't really slow down. You have to keep your research going. You have to keep publishing papers. When you slow down, that's going to have a negative impact on you when you're trying to get a grant. So there is some unfairness, and that's the way it is. That's the way it is right now.

VAN BENSCHOTEN: Do you think that anything can be done to sort of level that field?

CAMILLI: First of all, I think that our nation should give six months' paid leave, maternity leave, and that everything under the sun should be done to ensure that that doesn't have a negative impact on a woman's career. How to implement that in the sciences is a very difficult thing. What one would have to do is at study sections where grants are reviewed, there would have to be a box checked that says "During this research period of generating this data, I was on maternity leave." And there's no way that's ever going to happen, and it's probably illegal to even talk about that at a study section.

So in other words, there's no accountability for the fact that someone may have reduced productivity of 30 percent during a few-year period because of childbearing. I see no way to do it right now. But I also see many women who have families and go through— Some women remain highly productive throughout the whole thing, not at the expense of their families. Some women slow down a little bit and then come back strong, and there's just a little blip on the screen in their history, and they continue to get funded and their research flies.

Others take a serious hit and drop out of being competitive for funding or drop out of science altogether. I think it's unfortunate and unfair, but I really don't see a solution right now. If you asked me to come up with a solution for that, I would not be able to give you one, a realistic one.

VAN BENSCHOTEN: Do you think that men and women do science differently?

CAMILLI: I don't think so. I think we're all students of our former mentors and we learn to do things from them, and so there's probably a little bit of difference in— I mean, women tend to have mentors that are female, if they can. I don't know the statistics on this, but I'll bet there's a statistical difference. Males like to get trained by males and women by females.

For example, my lab was all male the first three years, and I made a conscious effort to recruit women to the lab because I didn't want a bunch of men in my lab. (a) it looked bad on me, like, what, I don't take women? And, (b) I just wanted to have a mixed lab. Didn't want the boys' club thing going on, plus men are slobs.

So, what I've seen, I've seen no difference in how women do science. There is a slight— in my own experience—bias that women are less aggressive than men in some cases. There are

exceptions. And so overall, I would say that my female students in my lab do not come in and utilize my talents as much as the men do. They don't come in and show me their data frequently. They don't come in and just talk about science. "Well, did you read this paper? Isn't this interesting?" Etc. And I don't know what the reason for that is but I just would say that the women are not as, in general, not as able to freely come in and, you know, shoot the breeze with me, and talk about science; talk about the other important things like, you know, how science works behind the scenes, like the letters of recommendation and getting fellowships and grant writing and all that.

When I do talk to them about that, they're very interested, but they don't come in and probe my experience and thinking as much as the men do. And so I have in the past, but I don't do it enough, try to compensate for that by going to them. That's one thing I constantly have to work on is to compensate for that deficit, to have parity in the lab.

VAN BENSCHOTEN: You mentioned earlier African Americans, and it brings up the subject of other underrepresented groups, one of them being Latinos. How specifically might more members of these underrepresented groups be brought into the biological field?

CAMILLI: That's a tough— We've been dismal at getting African Americans into our programs, just dismal. And I guarantee you that if a qualified applicant comes along, we bend over backwards to get them to come here, but we're just not successful. Some places are very successful at competing for the few qualified minority applicants, and that also goes at the faculty level.

On the other hand, we have a really good system. We have a history of this and it continues to this present day of getting Latinos to come here. One of the ways we do it, in particular we have a strong tie with University of Puerto Rico system, and every year we go down and recruit students for— So we have two training programs. One's a high school, one's an undergraduate, minority summer program. So we go down and recruit undergrad[uates]s from various campuses in Puerto Rico to come do research here, and we end up getting a few of those that come to grad school here.

We also have gotten Latinos from around Boston via our— So this undergrad program also will take American students as well. The high school program is more of a local thing, and we get a lot of Latinos and African Americans that come through our high school program, and we've had a couple of those who've gone on to other things, but then have come back to grad school here. So we have done well, I think. Overall, if you look at just minority versus nonminority, we've done excellent at training, getting, recruiting minorities to our program, and training them, and they go on to be scientists. But if you break it down, I'm just guessing, but I'll bet we haven't had more than three African American students in our history.

VAN BENSCHOTEN: What is the best part about being a scientist, about being a PI?

CAMILLI: There are two things that stand out. One is the thrill of learning something new. It used to be me getting the— Doing the work with my hands, but now people come in and I see their smile on their face and they show me an experiment and a beautiful result, and we've learned something new about these pathogens. That's the number-one thrill, and that's why I'm in science. That's why all of us are in science. If you're not thrilled when you get that result, you should not be in science.

And the second, I'd say equally important, and this is something that I had no vision of early on when I was training, that this would be a part of being a professor, and that's the personal interactions, coming into a lab with seven or eight people, you know, from different places around the world, all intelligent, educated, and just talking both about science and about current events, whatever, so coming in and having social interactions with a bright group of young people, and dealing with and working with these people, is the number-two thing. And that's something I had no concept of, because as a student I would just have my nose at the bench, working and working.

And the most important thing then was getting data and positive feedback from my professors.

But now I've got to tell you, I wake up every morning, get dressed as fast as I can and get in here because I enjoy coming in here and working with my group. I don't know how that's going to change as I get older and the generation gap gets wider and wider. It almost certainly is going to change, and perhaps will be less enjoyable. But right now it's just immensely satisfying and enjoyable.

VAN BENSCHOTEN: And what do you enjoy least about your job?

CAMILLI: Again, two things come to mind. One is, the frequent failure of grants is just devastating because the number of hours, the number of days you put into writing those grants, and it goes to two people who have forty other grants to review, and they'll make trivial decisions about how they rank your grant, and they'll shoot it down, even though they know and you know that this is good stuff. And it's because of the system. Only one in five grants can get funded.

Any tiny little thing, even personal, if they don't like your former mentors, because there are these different camps; there are these different groups that compete with each other. That can hurt you. So getting back those grant reviews and reading them is the worst part of my job. It's infrequent. Maybe once every two years I will have gotten back a failed grant. But it still is always in the back of my head. So that's the number-one thing.

The number-two thing is a more daily routine thing, and that is going to meetings or

doing some type of paperwork that has absolutely nothing to do with my research, which I told you the other day is a good 10 or 20 percent of my time each day. It's more than that. It's probably 20 percent is doing things that have absolutely nothing to do with my research. And many of those things are bureaucracy. It's paperwork. It's job creation at the administrative level. And it's the honest-to-god truth.

I mean, I realize there has to be oversight with the financial part of what we do, but I see things that could be done much more efficiently. But, you know, I'm cognizant of the fact that this is perhaps a warped view. The administrators may have the opposite view that we are incredibly wasteful. They need more personnel to do oversight on what we're doing. And then probably the combined thing is working like it should and the best it should. But right now I feel that a lot of my time is wasted on administrative things that are just creation of paperwork.

VAN BENSCHOTEN: All right. My last question. I'll open it up to you, and if you want to add anything to the record or clarify anything, please do.

CAMILLI: So, one thing that I think I mentioned briefly was I think something that's interesting, at least to me, is how did I end up being a scientist. I said the other day, and it's my honest feeling, is that in many ways it was sheer luck. There were a couple of things that happened during my life that could have— It was fifty-fifty things, that could have gone the other way, that I would be doing something else.

Like I don't think I told you, my junior year of high school, when I had a 2.5 GPA [grade point average] and had no insight into what I was going to be doing, I either saw an ad, or a friend, whatever—I forget exactly what happened—joined the military. And so I, without talking to anybody, including my parents, went down to the military recruiter's office and they were immediately on top of me, you know.

And they said, "Well, no commitment, no anything. Let us just tell you what the various things are like, and let's take this test, and let's see how you do."

So they gave me this test and I missed one question out of whatever. Got like a near perfect score. And I could just see these guys frothing at the mouth, that they wanted to sign me up. So, that was a confidence boost: that I took some exam that I did very well on, because I was doing very poorly in school. But I took that exam, got the score, you know, a day or two later, I forget, and they were calling me and putting pressure on me to join.

And I sat down with my dad [John P. Camilli] and I said, "Dad, I've got to tell you something. I went and took an exam at the recruiter's office, and I'm thinking about a military career." And my dad's response was perfect. He didn't get mad or angry, because he's anti-, well, he's not antimilitary, but he would not have been happy had I joined the military. He said, "Well, it's your choice. You're seventeen now and I could see you doing well in the armed forces, maybe become an officer or whatever, or get into research." But he said, "You still have

a year left of high school. You should not commit to this. You should see what your options are next year." So I called back the recruiters and said no.

But I've got to tell you, I was this close. It was truly a fifty-fifty thing over me having just signed the dotted line, and even before having talked with my father. In fact, when I was wavering over to do it, that's when I decided I'd better go talk to my father. And there was something driving me toward doing it, and that was getting away from the peer pressures and stress at school, and stress at home, just getting away from it and going away.

In a way, science, I've been successful in escaping by going into science, because I got away from my family. I got away from really a depressing life that I had in school where I never interacted with anybody, never got involved in anything. So in a way, getting into science was also an escape for me, fortunately doing something that I love.

But as the years have gone on, I've matured and developed some social skills, and now it's actually much more than I ever would have expected, because I'm getting both sides of things. I'm still doing original research and figuring things out and satisfying my curiosity and explorer needs, but have also been integrated into society in a functional way.

Oh, the other thing I wanted to mention is, sure, like a minute after you left yesterday, of course, remembered the book that I had read that made a big impact to me, and that was The Microbe Hunters.

VAN BENSCHOTEN: Right. It's funny, because I thought of that in my head but I mean you were talking about the sort of microbial world and I was wondering, you talked about adventure, because The Microbe Hunters, as you know, is sort of cast in this very sort of Flash Gordon, you know, adventurer, conqueror.

CAMILLI: But just hearing about the scientists, and how— I'm sure it's almost fiction. But that, I remember after reading that book, that made a huge impact on me, and I've subsequently learned that it's had a similar impact on other people in my field, like Dan Portnoy. He said he read that as a kid and that got him— His father was a research physician, so I don't know how much— I take that with a grain of salt.

But when I read it, that's when I started thinking of myself as a scientist/microbial explorer, fantasizing about it. And there were some other books, both fiction and nonfiction, that kind of got me thinking earlier on. So that was the other thing.

Now, let's see. Is there anything else I want to add?

VAN BENSCHOTEN: I can halt the tape, too, if you want a little bit more time to think.

CAMILLI: I just think that, you know, moving beyond my own little bubble here, that I see great problems coming up for humanity in terms of infectious disease, and on the one hand, I think I've chosen the right career because there's going to be greater and greater need for investigators to help figure things out and come up with useful ideas, but I'm a little bit of a pessimist. I foresee hard times coming up for humanity as we overpopulate and the Third World actually gets bigger and bigger and bigger, and there's less and less clean water to pass around, and people are grouped into small groups with no sanitation, infectious diseases are going to get worse and worse and worse, maybe even cholera.

Ultimately, though, I have great faith and hope for humanity, because I think we're going to go through this phase, but we're going to get past it, and we're going to grow and mature and move on to better ways of creating societies. But I do think, I'm, I guess, a fatalist— I think it's necessary to go through this ugly period where we've overpopulated, overpolluted, and there's tremendous disease and death going on in the world. Maybe some people might say we've already hit it.

But ultimately I do have great hope for our species, and so I feel really good about what I do in that respect, that even though, to be honest, I'm just a teeny little part, a teeny little cog in all the wheels, a lot of these little cogs are making things happen, and so I do feel I can live with myself. Even though I have very little religion, I feel good about myself as, I guess, a humanitarian in my choice of career.

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