Statistical Science
2016, Vol. 31, No. 3, 433–441
DOI: 10.1214/15-STS545

A Conversation with Michael Woodroofe

Moulinath Banerjee and Bodhisattva Sen

Abstract. Michael Woodroofe was born in Corvallis on March 17, 1940, and grew up in a small town called Athena in Oregon. Michael graduated from McEwen High School in 1958 and entered Stanford University, from which he graduated four years later with a major in Mathematics. He earned his masters degree and Ph.D. from the mathematics department at the University of Oregon in 1964 and 1965, respectively.

Michael Woodroofe has had a distinguished career and is widely recognized as a preeminent statistician and probabilist. He has broad interests and has made deep and significant contributions in many areas in statistical inference and probability, including biased sampling, shape-restricted inference, sequential analysis, nonlinear renewal theory, modern nonparametric inference, statistics in astronomy and central limit theory for stationary processes. He has published more than 100 research articles, written a SIAM monograph and authored a book. He is a former Editor of *the Annals of Statistics*, a member of Phi Beta Kappa and a fellow of the Institute of Mathematical Statistics.

Michael's professional positions have included being on the faculty of the Department of Statistics at Carnegie Mellon University and the University of Michigan at Ann Arbor, where he has been on faculty for more than 40 years. He was a founding member of the Department of Statistics at the University Michigan in 1969, retaining a joint appointment with Mathematics, and served as the Chair of the Department of Statistics during 1977–1983. In addition, he has held visiting positions at Columbia University, Massachusetts Institute of Technology and Rutgers University.

Michael and his wife, Fran Woodroofe, reside in Ann Arbor. He is the father of one daughter, Caroline, and two sons, Russell and Blake.

Key words and phrases: Biased sampling, nonlinear renewal theory, sequential analysis, shape-restricted inference, statistics in astronomy.

Bodhi: Michael, I want to say that it's a privilege to be here to interview you. I would like to thank you for being a great advisor and for your guidance, encouragement and inspiration.

Mouli: Likewise, great privilege to interview you, Michael. I'd also like to thank you for being a fantastic mentor, during the initial phase of my career, and collaborator.

Moulinath Banerjee is Professor of Statistics and Biostatistics at the University of Michigan, Ann Arbor, Michigan 48109, USA (e-mail: moulib@umich.edu). Bodhisattva Sen is Associate Professor of Statistics at Columbia University, New York, New York 10027, USA (e-mail: bodhi@stat.columbia.edu).

1. CHILDHOOD AND SCHOOLING

Bodhi: We know you are from Oregon. Where was your home town? Tell us a bit about your family. How was your childhood?

Michael: I come from a place called Athena. It was a very small town. My parents were very interesting people—I still remember my mother and father quoting Shakespeare to each other at the dinner table when I was a small child. My mother was an English teacher. My dad farmed. We lived in town and he farmed the

family land. My childhood was very good. I was the only child in the family.

Mouli: When did your interests in mathematics and science begin to emerge? What influences drew you into mathematics and science?

Michael: Oh, pretty early. My dad had been a science major; he was a science teacher at the high school in Athena for a while. So he was an important influence!

When I went to college I was undecided between math and physics. It was basically that the math classes were pretty well taught and the physics classes were atrociously taught. My son Russell (Figure 2) reports something similar. When he came to the University of Michigan, he was undecided between computer science and mathematics. It was again the case that the math classes were well taught and the computer science classes were like a factory.

Bodhi: What can you tell us about your high school?

Michael: It was a very small school—less than 100 students in the whole darn high school. So everybody had to do everything just to have enough bodies to make it go. So believe it or not, I was the center in the football team! I was a bit heavier then (Michael now weighs 125 pounds).

Math and physics were my favorite subjects. However, the school did not play any significant role in my academic interests.

2. STANFORD

Mouli: Why did you decide to go to Stanford as an undergraduate? Was it academic excellence as well as proximity to the Northwest?

Michael: That was more or less my parents. It was certainly academic excellence and it isn't that close—Stanford is still an 800 mile drive from Athena!

Bodhi: Tell us a little bit about Stanford as it was when you were there. How was the experience in general?

Michael: In general it was terrible. I didn't fit in socially. Everybody else came from a family that had a lot more money. They had all gone to private schools. I just did not fit in at all.

However, academics were good, very good. I majored in mathematics. I took some physics courses, I wish I had taken more.

Mouli: Who was the person who influenced you in Stanford?

Michael: John W. Lamperti (now an Emeritus Professor at Dartmouth), a probabilist, influenced me quite a bit.

Bodhi: You were initially interested in mathematics and then you gravitated toward probability. Was that because of John or something else?

Michael: Well, let me see. It wasn't because of Stanford. I had the most unfriendly introduction to probability you can imagine—Loéve was the textbook and (Samuel) Karlin was the teacher.

Mouli: So, Karlin wasn't an easy instructor?

Michael: No, no. I remember we walked into the class, the first day of class, it was a big class. He couldn't meet in Sequoia Hall. He said he was going to fail enough students so that in the winter term we could meet in Sequoia Hall. He was really over my head in that class. A lot of students were graduate students from statistics. One of them was Grace Wahba—she tried to help me. She could see that I was struggling.

Bodhi: Did you have any interactions with the statistics department there?

Michael: I did not have any contact with the statistics department except for the probability class.

3. GRADUATE SCHOOL AT UNIVERSITY OF OREGON

Mouli: What led you to come back to Oregon? Was there any particular reason that you stayed back in Oregon to finish your Ph.D.?

Michael: They offered me the best deal. They gave me a fellowship; I didn't have to teach at the University of Oregon. The masters was a part of the Ph.D.

Bodhi: Tell us a little bit about University of Oregon as it was when you were there.

Michael: At that time Oregon was a hotbed for Banach algebras. That was the main topic in the math department.

Mouli: What led you to pursue probability?

Michael: It was a complete accident. When I got there I already had quite a few graduate classes at Stanford in probability, and it was determined that I did not know enough topology to take the knot theory class. So they put me into something called "testing statistical hypothesis" and that was an interesting class. So it was a complete accident that I ended up in statistics. I liked the guy teaching the class—Ted Matthes.

Bodhi: Your Ph.D. was on the "Statistical properties of the number of positive sums" of i.i.d. random variables. What motivated you to choose this as your dissertation topic?

FIG. 1. *Michael (front row first from right) with his teammates after winning a trophy.*

Michael: When I said I wanted to work in statistics Don Truax gave me some stuff to read. It was all about the arc-sine law. It just sort of developed from there. But he was not my advisor; Ted Matthes was my advisor. The Ph.D. topic grew out of the reading I did.

Bodhi: I actually did not know of this interesting connection till last year—that Ted Matthes did his Ph.D. from Columbia. I can now trace back my academic roots, from both sides (you and Mouli), to Columbia—Ted Matthes completed his Ph.D. from Columbia in 1960 and my academic ancestor Meyer A. Girshick, from Mouli's side, also did his Ph.D. from Columbia in 1947 (under Abraham Wald).

Mouli: How much do you think your thesis influenced your later career?

Michael: It's vaguely related to the renewal theorem and random walks. So to that extent it gave me a little foundation.

Bodhi: What were your other topics of interest in statistics at that time?

Michael: At that time rank tests where the big thing. Seemed like everybody was doing rank tests.

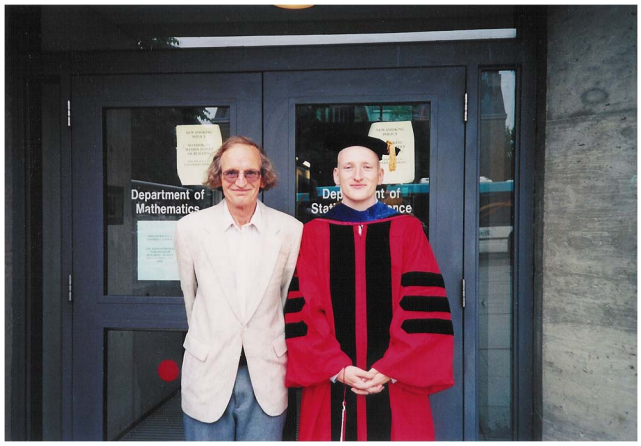

FIG. 2. *Michael with his eldest son Russell after his Ph.D. commencement (in Cornell, Ithaca).*

4. PERSONAL LIFE

Mouli: Now let's digress from academics a bit and talk about your other interests and hobbies. One that's fascinated us is your interest in playing ice hockey (Figure 1). How did that develop?

Michael: That's a nice story to tell. When Blake, my youngest son, was about 8 and I was about 50, it became clear that Blake was going to like hockey. So I started counting the number of games I would have to watch—50 games a season times 12 years—that's a bunch. So I thought I should learn something about the game. At that time the university offered something called adult hockey class. The other students in the class were mostly university students. So I was actually a little scared when I went out—because here's a chance to knock a professor on his butt. But the young

FIG. 3. *Left*: *Fran, Michael and Caroline*; *right*: *Michael with baby Russell*.

people liked me—I was always trying. I was not much of an athlete, I was old and slow. But I was trying and they liked me. It was 1991 when I started, I think.

Bodhi: Any other sport that you liked before getting so involved in hockey?

Michael: I always liked baseball.

Mouli: What team did you support, if any?

Michael: When I was in Pittsburgh it was the Pirates. I like the Tigers now that I am near Detroit.

Bodhi: We have also heard that you used to coach your eldest son Russell and his friends for some Mathematics competition. Was it the Olympiads?

Michael: I am not sure what it was called, but it was a competition for eighth graders. There were about 4 or 5 students and it was for one year. We would meet several times a week actually, before school.

Bodhi: We know you have three kids. How difficult was it to be a good father and still devote so much time to academics?

Michael: It's not easy—there's only so many hours in the day. My wife (Figure 3) would often complain that I would go down to the basement after dinner and work all night.

Mouli: How would you spend your summers with your family?

Michael: We have a cottage in northern Michigan which we bought in 1978, I think. We use it more or less all summer. I bike around the trails; I have a mountain bike. I like swimming. My kids enjoy it also. It's very nice in the summer time.

One nice thing was that for a long time we did not even have internet access. So if I sat down to work, there was nothing else to do but work. I couldn't check e-mails or surf the web. I would also take a few books up there.

5. ACADEMICS AND POST-PH.D. EXPERIENCES

Bodhi: You have a wide-ranging interest in statistics and probability. Before going into some of these areas in detail, as an overview, what are the landmark topics you've visited during your career that sketch out the contours of your research trajectory? We'll return to some of these more in depth later.

Michael: Sequential analysis has always been of interest to me. That was partly the times—there was quite a bit of interest in sequential analysis during the late sixties and early seventies. Nonparametrics is also such a topic.

Bodhi: You had early research interests in nonparametric function estimation, specifically kernel density estimation. Some of your early papers were on this topic. What was the precursor to this line of research?

Michael: I was a research associate at Stanford for one year and I had no teaching. So I had lots of time. I read Parzen's (Parzen, 1962) paper and thought that was interesting.

Mouli: So this was after your Ph.D. that you were a research associate at Stanford? How did that happen?

Michael: I think it was my advisor and Don Truax, who called the people at Stanford and recommended me.

Bodhi: Walk us though the initial years of your research career: your research interests, the people you looked up to, your favorite statistics books.

Michael: I liked time series. I hadn't done anything yet, but I liked sequential analysis. It was when I went to Carnegie Mellon (Carnegie Tech then) as a tenure-track faculty that I started working on it.

I looked up to (Herman) Chernoff and (Herbert) Robbins.

(Erich) Lehmann's statistical hypothesis—that was my introduction to the subject.

Mouli: How about the other Lehmann book?

Michael: It wasn't a book yet—it was a set of notes.

Bodhi: Your first teaching job was at Carnegie Mellon University. What prompted you to move to Michigan?

Michael: Well, Robbins was on the faculty here for a while, and I came here hoping that he would come back but he didn't. I think Michigan actually fired him at some point because he never showed up. He was at Columbia at that point.

Mouli: Did you ever think about heading back to the Pacific Northwest? Why did you end up staying at Michigan, even though Robbins never came back?

Michael: It's a good university and it's a reasonable place to live... lethargy, inertia. Hira Koul once said he came to Michigan State and just got stuck. That's pretty much what happened to me.

Bodhi: What got you interested in sequential analysis? Did Norman Starr have a role to play in it?

Michael: Yes, Norman Starr got me interested in sequential analysis. Norman Starr was also at Carnegie Tech when I was there, and he was a student of Robbins. So I got sort of secondhand exposure to Robbins. Sequential analysis was sort of developed at Columbia—first Wald and then Robbins.

Norman Starr and I came to Michigan together, but unfortunately he died young. He was still in his sixties. He was older than me by six or seven years.

Mouli: Can you elaborate on the connections between sequential analysis and nonlinear renewal theory?

Michael: It's a little easier if you talk about the sequential probability ratio test and renewal theory. If you always land right on the boundary when you stop, then you can work out the probabilities exactly. There is a little martingale trick that lets you do that. So, the question becomes how much you jump over the boundary when you stop. And that's a question that can be answered as a corollary to the renewal theorem. So, in some of my best work I generalized that from straight line boundary to a curved boundary.

I think it was the applications to sequential analysis that caused nonlinear renewal theory to be developed.

Bodhi: Besides you, David Siegmund and Tze Leung Lai were two other key figures of your generation in the field of sequential analysis. Though you never collaborated with them, it appears that the three of you influenced each other's research to quite an extent. Can you comment on your relationship with them?

Michael: No, I have never written a paper with them, but our relationship was very friendly. When we would get together we talked about math.

I got to know them quite well when I was at Columbia in 1970–1971. Lai was a student there, finishing up his Ph.D. at that point, and Siegmund was on the faculty.

Mouli: In the late 70s and early 80s you also worked on numerous topics, including (i) the theory of repeated significance tests, (ii) fixed-width confidence intervals, (iii) asymptotically point-wise optimal rules, etc.—lines of research that neither of us is very familiar with. Would you care to elaborate on any of these problems and how they might be useful in modern day applications?

Michael: Repeated significant tests happen all the time—people keep watching the significance level until it falls below 0.05; OK, now we can stop, now we can publish! The sequential estimation, what that needs right now is a success story from the applied front. Fixed-width confidence intervals—that's a theory in search of an application.

Bodhi: You have always maintained a strong interest in physics and astronomy and been very active at the intersection of astronomy and statistics. Can you elaborate on how the different collaborations developed, in particular, your interactions with Byron Roe and Mario Mateo?

Michael: In the case of physics, I got a call one day from Byron Roe (in the late nineties). He said he had an interesting statistical question that needed an answer. It was an embarrassment to the subject that it hadn't been solved. The problem is suppose you have got a count that consists of a background plus a signal, both Poisson, and suppose you don't observe anything. If you go to a book, it will tell you the asymptotic theory, but the asymptotic solution is just nonsense if you observe nothing. It almost made a Bayesian out of me, not quite.

Now about the astronomy collaboration with Mario Mateo. I hate to admit it, but that was the case of the university doing something right. This was about 2002, I think. They wanted to encourage interdisciplinary research and they said, OK, we have got this pot of money that we will make available for people who propose interdisciplinary projects, and the smart thing they did was instead of breaking up the sum into a whole bunch of tiny piles, they just had two pretty big piles of money. It was competitive. There were sixty proposals of which two were funded. One was ours. Mario was the driving force. He is just very smart. He put together a strong proposal, and he was politically smart too. He put together a team that involved him, me and

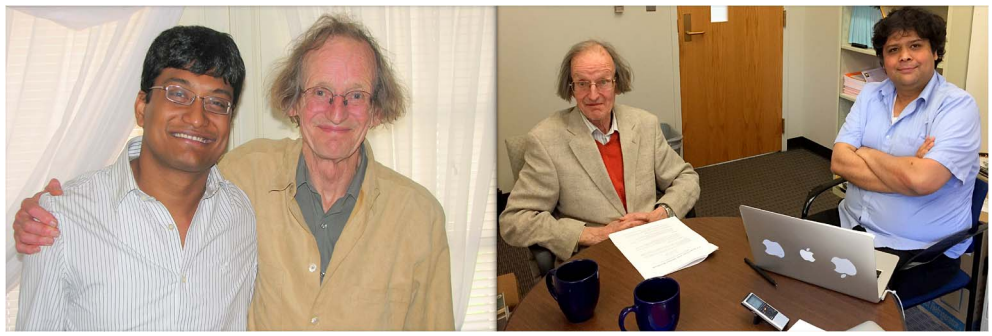

FIG. 4. *Left*: *Bodhi with Michael after his Ph.D. commencement* (*in Ann Arbor*); *right*: *Michael with Mouli on April* 27, 2015, *the day of the interview* (*in Mouli's office*; *Ann Arbor*).

Jim Joyce from philosophy. The committee that evaluated the proposals consisted of two natural scientists, two social scientists and two people from the humanities. We had all three—humanities, social science (if you think of statistics as social science) and natural science.

My previous work on applications to astronomy, for example, the Lynden–Bell estimator (Woodroofe, 1985), was just an outgrowth of the mathematics. Irving Segal at MIT had asked me about this question when I was there (visiting MIT math during my sabbatical) and I finally worked it out.

Mouli: A lot of statisticians have taken an avid interest in more biological applications, and you seem to have avoided that. Was there any particular reason? In particular, given that this century is likely to see massive strides in biology, we are likely to see even more statisticians gravitating toward these applications. Do you have any particular views on this?

Michael: I always wanted to take the road less traveled.

About biology: I think it is probably the right thing to do in this day and age. If I were starting out, I'd try to learn some biology. If you want to do interdisciplinary research, you really got to learn something about the field. So one thing we did right with the astronomy project, when we got the grant from the university, was that we used part of the money to offer a seminar course. And in that course I lectured on astrophysics and Mario lectured on statistics.

I had attended a course taught by Mario quite sometime back. After I wrote the Lynden–Bell paper I decided that I wanted to learn some more about astronomy so I went over and sat in his class. This was in the mid-nineties. That is how I met him.

Bodhi: Slightly later in your career, you became interested in shape restricted function estimation. Given that one of our (Figure 4) main research interests also lies in this area, we are curious to hear your thoughts on it. What got you interested in this area of research?

Michael: I had to review a paper by Piet Groeneboom (Groeneboom, 1985). I learned a lot from that paper. I think that was my first introduction to the subject. Also, I was in some situation where I needed a consistent estimator and I realized that the shape restricted monotone estimator would do the trick.

I think it's an interesting area, but we have got to get more interesting shape restrictions.

Mouli: In the 90s, you started working on central limit theorems for stationary processes. Did this line of research start with your papers with Michael Maxwell? Can you comment on this research area and what prompted you to pursue this topic?

Michael: No, it started back in 1992, but I don't remember what provoked that. I wrote a paper on CLT for stationary processes (Woodroofe, 1992). As for Maxwell, he was my Ph.D. student from the math department.

Mouli: You are still working on this topic, collaborating with Dalibor Volný, isn't it?

Michael: I am still working with Dalibor Volný and it is pretty much in the same direction. It's going great—I choose to go to France frequently.

Bodhi: We know that this can be difficult, but what do you think are some of the most significant papers you have authored?

Michael: I would include the papers on nonlinear renewal theory. The paper with Maxwell (Maxwell and Woodroofe, 2000) had a big impact. I am sure I am forgetting some.

Mouli: You visited Columbia, Rutgers and MIT for substantial periods during your academic career. How were these experiences, and how did these visits shape your research career?

Michael: The year at Columbia (1970–1971) had a big impact because Robbins was just such a dynamic character. That was the reason I concentrated on sequential analysis.

I liked the people at Rutgers a lot—(Harold) Sachrowitz, (Arthur) Cohen, (Bill) Strawderman. I got along with Sachrowitz especially well. I wanted to stay in Rutgers. However, Fran, my wife, wanted to come back; she won.

6. STATISTICS: RESEARCH, TEACHING AND MORE

Bodhi: You became the Editor of *the Annals of Statistics* in 1992. In fact, you were the last solo Editor of the journal. How was that experience? What would be your advice to the future generation of Editors and Associate Editors of such important journals. They clearly have a very important job, guiding the research focus of statistics as a field. What are your thoughts on this?

Michael: It was a lot of work. I think we were receiving around 200 papers every year. That's just too many.

My advice: Get a good assistant! Try to clear your schedule so that you can concentrate on editing the journal. It's almost a full-time job.

Mouli: To what extent do you think Editors should screen out papers themselves before passing them to Associate Editors?

Michael: Some screening is almost unavoidable. You look at a paper and think, "Do I want to waste an Associate Editor on this paper or do it myself?" That happened to me several times—papers that were obviously deficient. One paper, fortunately I have forgotten the author's name, started off by saying, "Let $\|\cdot\|$ denote the norm in $D[0, 1]$," which is, of course, not normable. That was a pretty clear reject.

You have to spend a lot of time on these papers because you have to write the reports yourself. So you spend an enormous amount of time on bad papers.

I think there has been a change from when I started out. Nowadays, I think probably referees often do not have enough time to read through the papers in a lot of detail. Even when I was an Associate Editor I always felt that I should really understand the paper and the proof if I were to give a positive recommendation on the paper.

One thing I did was to check the proof to make sure it was right. I remember once I sent a paper to a referee who I thought was pretty reliable. The authors sent back the revision and while reading through it, I saw what I thought was a mathematical mistake that did not get caught the first time. So I wrote an apology letter saying, "How do you justify this step?" And we never got a resubmission; never got an answer to that question.

Mouli: You have been a highly valuable mentor to young statisticians. I benefited a lot from your mentorship. Is there anything you can say about that process? Would you share with us your extensive experience in student advising? We still recall your excitement in connection with advising when you once stated that student advising is really where research and teaching intersect and that's what you enjoyed most about it.

Michael: I benefited a lot from you Mouli.

I find that the apprentice system works pretty well. You start off doing some joint research. In the beginning the advisor sort of takes the lead and at the end the student is supposed to take the lead.

Mouli: Hopefully, but that sometimes doesn't work. I have seen with many students that the student never takes the lead.

Michael: It sure worked with him (pointing to Bodhi); but then he is an exceptional case. Although I must say that as good a student as Bodhi or Wei-Biao Wu (full professor at University of Chicago) was, probably the best group of students I ever had was in about 1990. I had five students all of whom were just really great—Mei Wang (she is now at University of Chicago), Vince Melfi (Michigan State), Jeffrey Eisele (Vice-President at a large drug company), Mauro Gasparini (Professor at Torino) and Jyoti Sarkar (Indiana University). They were five really outstanding students at the same time.

Mouli: How many students have you graduated till now?

Michael: 43, I think. Actually, it should be 44. I would count Yizao Wang (now at University of Cincinnati) as well. Apparently I was not an advisor for him on the thesis, but in fact I was. Half of his thesis is about central limit theory for random fields. He was another super student.

Bodhi: Are we, as academicians, doing enough to teach statistics the right way to our Ph.D. students? If you were allowed a free hand, what would you change, if at all, about the Ph.D. curriculum, say, at the University of Michigan.

Michael: As far as the mentoring goes, I don't know if I can make a general rule, meaning what works for me might not work for somebody else. As far as the

preparation goes, I always wish that we taught our students a little more math. But then we have new tools now and then there's only so much time and it's important to teach the new tools, mostly computing.

Mouli: A more philosophical question. All of us will agree that Statistics has evolved and changed over the years. How do you view the change? We have data science now. What do you think is the future of statistics as a discipline?

Michael: I am optimistic. I think we will flourish. There is so much data now. And that's why I say I am sorry to see less math in the program, but we really need to teach the students how to deal with large data sets. And I think we will come through fine. I also think that we ought to be encouraging our students to get masters in some substantive fields while they are doing their Ph.D. (something like a dual degree). As I said, if you want to do interdisciplinary research, you got to learn something about the other discipline, say, a masters in science or social science plus a Ph.D. in statistics would be a very good foundation.

Bodhi: A related question: What would be your career advice to a fresh Ph.D. student in Statistics? What do you think is a good background to pursue a Ph.D. in statistics?

Michael: Mostly look around, try new things.

I still think that for an undergraduate degree math and computer science both are probably the best choice.

Mouli: Statistics and probability have become somewhat more disconnected in recent times as compared to before. Having worked at the interface, what are your thoughts on this?

Michael: You are right, they are not as close as they used to be. The emphasis in probability has gone to continuous time and there is less emphasis on limit theorems than there used to be. And personally I am sorry because, you are right, I used to live in both worlds.

Bodhi: Which new statistical areas today interest you most and which do you feel you're most likely to work on if you were starting your career today?

Michael: If I were starting out now, well, nonparametric regression, interpreted broadly, and probably applications to biology because that is where the interest is going to be, although I like physics and astronomy.

Mouli: What are some ways that it's easier for new faculty, compared to when you started, and what are some ways in which it's harder? What do you think about how the field operates today: Pressure on youngsters to publish a lot, lots of students, grants; working on too many problems at the same time which for most researchers necessarily compromises the amount of thinking that goes into each problem (maybe an influence of engineering)? How do you feel this bodes well for the evolution of the discipline?

Michael: I think it was easier when I got out because it was a sellers' market. Now, it's not so anymore. I think getting tenure was quite a bit easier when I was young. It was just assumed that you would get tenure and the question was whether you would get it before 30 or not. If you didn't get it by the time you were 30, that was thought to be a stigma you would never live down. So, it's a lot tougher now.

Then, there is too much emphasis on the number of publications these days, and that's unlikely to change. I think this happens when you start getting the college involved and they have to sign off on these decisions (recommendations): they can't really distinguish good work from bad work, but they can count!

7. UNIVERSITY OF MICHIGAN

Mouli: Are there some colleagues at Michigan you would like to acknowledge as having shaped your academic views and research interests or having had an impact on your research career?

Michael: I guess, Bill Ericson, to some extent. He was the chairman when I was hired and the founding chairman of the department. We didn't do research together but he was very supportive. I think it is fair to say that he had some influence in the way I developed. Norm Starr and I came together to Michigan. We worked together quite a bit.

Bodhi: You have sometimes called yourself the "closest to a Bayesian in the department." In particular, could you elaborate on your interactions with Bruce Hill?

Michael: When I came to the department it had a very Bayesian flavor to it. Ericson was a Bayesian and Bruce Hill, of course!

The papers with Bruce: That collaboration was more or less a case of him having some probabilistic questions that he wanted some help with. Actually one of those papers, I think, is very good—on the Zipf's law (Woodroofe and Hill, 1975; also see Hill and Woodroofe, 1975). We start off by showing that if there is some sort of statistical irregularity, then it has to be Zipf's law.

Mouli: Can you comment on how you'd compare the department from the late 90s onwards to before that time? These were probably two distinct phases?

Michael: When we started out we were supposed to be a sort of small theoretical department and now we are a much bigger department. I think we are much stronger now on the applied side than we used to be. That's good, of course. But that came at a high price, I thought.

Bodhi: To what extent did you interact with the mathematics department, since you had a joint appointment with them?

Michael: Almost all my interactions with the math department have been through graduate students. I have advised several theses over there. Looking backwards, I see there's Maxwell, Robert Charles Hagwood, Nancy Heckman, Mark Finster and Allen Foy. And how can I forget one of my favorite students, Mei Wang?

8. POST-RETIREMENT

Mouli: How do you spend most of your time post-retirement?

Michael: Badly. I waste a ridiculous amount of time watching sports on TV. I used to think TV was a waste of time; it is. So I actually don't like what has happened to me in that regard. I don't play hockey anymore. I do not even do long bike rides anymore. Till a couple of years ago I was going to Dexter and back—that's 25 miles roundtrip: I liked the Dexter Cider Mill. I have not done that for a couple of years. I may try again this summer, I am not sure, if it ever warms up.

Bodhi: We know you used to spend quite a bit of time at your cottage (in northern Michigan), especially during summer. Do you still do the same?

Michael: Yes, we do. Both Fran and I go every summer. She likes it more than I do, I think.

Mouli: Any specific plans for the future?

Michael: As long as my health holds up, I would like to travel. I will keep working on central limit theory for stationary processes. I have always had an interest in stationary processes, ever since graduate school.

ACKNOWLEDGMENTS

The authors would like to thank Bob Keener and Mary Meyer for suggesting some of the questions.

REFERENCES

GROENEBOOM, P. (1985). Estimating a monotone density. In *Proceedings of the Berkeley Conference in Honor of Jerzy Neyman and Jack Kiefer, Vol. II* (*Berkeley, Calif.*, 1983). *Wadsworth Statist./Probab. Ser.* 539–555. Wadsworth, Belmont, CA. MR0822052

HILL, B. M. and WOODROOFE, M. (1975). Stronger forms of Zipf's law. *J. Amer. Statist. Assoc.* **70** 212–219. MR0440763

MAXWELL, M. and WOODROOFE, M. (2000). Central limit theorems for additive functionals of Markov chains. *Ann. Probab.* **28** 713–724. MR1782272

PARZEN, E. (1962). On estimation of a probability density function and mode. *Ann. Math. Statist.* **33** 1065–1076. MR0143282

WOODROOFE, M. (1985). Estimating a distribution function with truncated data. *Ann. Statist.* **13** 163–177. MR0773160

WOODROOFE, M. (1992). A central limit theorem for functions of a Markov chain with applications to shifts. *Stochastic Process. Appl.* **41** 33–44. MR1162717

WOODROOFE, M. and HILL, B. (1975). On Zipf's law. *J. Appl. Probab.* **12** 425–434. MR0440764