

A Conversation with Robert Groves

Hermann Habermann, Courtney Kennedy and Partha Lahiri

Abstract. Professor Robert M. Groves is among the world leaders in survey methodology and survey statistics over the last four decades. Groves' research—particularly on survey nonresponse, survey errors and costs, and responsive design—helped to provide intellectual footing for a new academic discipline. In addition, Groves has had remarkable success building academic programs that integrate the social sciences with statistics and computer science. He was instrumental in the development of degree programs in survey methodology at the University of Michigan and the University of Maryland. Recently, as Provost of Georgetown University, he has championed the use of big data sets to increase understanding of society and human behavior. Between his academic tenures, Groves served as Director of the US Census Bureau. Professor Groves is an elected fellow of the American Statistical Association, elected member of the International Statistical Institute, elected member of the American Academy of Arts and Sciences, elected member of the US National Academy of Sciences, elected member of the Institute of Medicine of the US National Academies and presidential appointed member of the National Science Board. The interview was conducted in early 2016 at Georgetown University.

Key words and phrases: Data science, federal statistical system, leverage-salience theory, nonresponse, responsive design, sample surveys.

1. NONRESPONSE RESEARCH

Kennedy: I would like to start with your research, your contributions to the scientific body of knowledge.

Groves: Oh, so this is the short part of the discussion, my contributions! (Laughing)

Kennedy: One of the ideas that you are most known for is leverage-salience theory (Groves, Singer and Corning, 2000). Toward the end of your life in academia, you were experimentally testing hypotheses

derived from that theory. What is your reflection on that line of research? Do you consider it finished, or is there a lot more that you think needs to be done in that area?

Groves: On that paper with Eleanor (Singer) and Amy (Corning), we had some data on which we were fitting main effect models on everything, and things weren't working. I had had an earlier conversation with Bob Cialdini, who I would say is an under-awarded intellect in our country. He is a very good thinker about social-psychological mechanisms that seem to affect behavior. I was corresponding with him while we were looking at the data, and it became obvious that the whole system of effects on survey response decisions is a set of interactive effects. There's very little in the way of main effects. What do I mean by that? Well, the effect of a particular wording requesting participation is interpreted differently depending on the context of the wording, and the stuff a person brings to the room. So it made sense that empirically we weren't getting stable main effects. That motivated the theory, which is basically a big interactive model of costs/benefits and the like.

Hermann Habermann is the former Chief Statistician, United Nations, and former Deputy Director, the US Census Bureau; 1555 N Colonial Terrace, Apt. 700, Arlington, Virginia 22209, USA (e-mail: hermannhabermann@aol.com). Courtney Kennedy is Director of Survey Research, Pew Research Center, 1615 L Street NW, Suite 800, Washington, DC 20036, USA (e-mail: ckennedy@pewresearch.org). Partha Lahiri is Professor, Joint Program in Survey Methodology and Department of Mathematics, 1218 Lefrak Hall, University of Maryland, College Park, Maryland 20742, USA (e-mail: plahiri@umd.edu).

But your question is broader than that. That somewhat theoretical paper motivated a bunch of experiments (Groves et al., 2012). Remember, the belief at that time in the field was low response rates meant nonresponse bias. We just believed that. I was actually taught that. If you look up early sampling texts, they just assert that. So we tried to turn the problem on its head and, in an experimental way, induce nonresponse bias. We wanted to create nonresponse bias under experimental conditions. My reflections are that you can make it happen, but you really have to jack up the stimulus so that there is a single basis of decision. The only reason to respond or not respond must be the experimental stimulus. Then you can induce bias.

Now, another paper, the meta-analysis that I did with Emilia (Peytcheva), was earth-shaking to me (Groves and Peytcheva, 2008). It was like questioning our faith. There we assembled a large number of studies where we had a pretty good proxy estimate of nonresponse bias. And there was just no relationship, empirically, between response rates and nonresponse bias. It was just astounding. It was like someone said Santa Claus didn't exist when you were a kid.

That led to my current belief, which is that we're sort of lucky. The mechanisms that produce the decision to participate or not participate in a survey are myriad; people make decisions on different bases. That's great. Our statistics are multivariate, and the covariance between the decision to participate and what we're measuring tends to be small. It doesn't have to be that way at all, but that appears to be what normally goes on. Hence, we can get away with low response rates in a lot of cases. But there is no theory that protects us. So every once in a while we get burned, big time.

My regret in that research has to do with the paper where we did something similar to what Jelke Bethlehem had done to express nonresponse bias as a covariance term. I still don't think people get that. And that's everything as I see it.

Habermann: Why don't they, though?

Groves: Well, I actually had a proof, which as I look back on it, was horrible. It was tortured algebra that was much too long (Groves, Presser and Dipko, 2004). My hunch is maybe ten people have read that proof. And then Jelke published a wonderful statement of that in a missing data book (Bethlehem, 2002). But I feel that is a failure of that whole set of papers—that the memory of most people is not about how important that covariance term is but instead the erroneous notion that response rates don't matter.



FIG. 1. Bob at Michigan circa 2004.

Kennedy: There is some discussion that a low response rate survey is no longer a probability-based survey. In my mind, that is not helpful because it mixes probability of selection, which we still do know, with probability of response, which we don't know. What is your take on that?

Groves: I think the evaluative criteria and the language have gotten messed up. Remember, when Knowledge Networks started, their distinctive brand image was that they were a probability sampling shop. Some economics work went in that direction, too, and I think there wasn't a counter-voice at the time to say, wait a minute, probability sampling isn't the issue. The issue is—what's the mean squared error of what you're doing? You can start off with a probability sample, but if you don't measure it, it's not useful.

2. RESPONSIVE DESIGN

Habermann: When you got to the Census Bureau, was your nonresponse research inculcated? Did that change while you were there?

Groves: I didn't do this sort of work at the Census Bureau. I didn't see radical redesign of what they were doing as very feasible in a tenure of a director. I did



FIG. 2. *Bob with students at ISR, circa 2006.*

think something was possible with paradata development and its use in design and operations.

Let me try to link this to adaptive/responsive design. If we don't know the mechanisms that affect response propensity, and hence, we don't know the correlation between those mechanisms and the variables we're measuring, then you don't have much purchase on nonresponse bias, right? So, okay, what could we do? Well, we're in an adjustment world, and so enriching the data record on the frame, or at least the sample, makes sense. And that led to the paradata work (a term of Mick Couper's), which led to the responsive design activity.

Motivating this was a course we taught at JPSM. (Trivellor) Raghunathan, Frauke (Kreuter) and I mounted what was the best educational experience in my life. Raghunathan is a missing data guy out of the (Donald) Rubin tradition, and I was playing with the causal mechanisms that produce participation. We decided to co-teach a course. It's the only course I've ever taught where the students at the end of the course said, "Can we keep meeting? Do we have to stop the course now?" So we met over the summer, and we ended up writing a paper that Frauke led (Kreuter et al., 2010). The intersection of what Raghunathan and I were doing became the collection of auxiliary variables, called paradata. He would do it for adjustment or imputation,

and I would do it for responsive design. That was a key moment for me.

When I got to the Census Bureau, I thought the right move was to encourage use of paradata and responsive design. They weren't going to abandon traditional sample surveys, but maybe they could patch through with responsive design. We won't know for ten years whether that is successful.

Kennedy: Responsive design is very much in vogue in survey research. There seems to be great interest in it as a fix for the problems surveys are facing, but the evidence for responsive design in practice so far seems mixed.

Groves: I think that term has been abused. We were devoted to randomized experiments as part of the responsive design because only through randomized experiments can you measure the uncertainty of the intervention as well as the effects of the intervention. I think that got lost. Responsive design to some people means you try design A, if that doesn't work in the middle of the survey you try design B, and then if that doesn't work you try design C and so on. One other regret is that we probably should have called it adaptive design. The survey field is missing a link with the adaptive clinical trial field, which has become very sophisticated on the modeling side. If I were 30 years old and smart enough, that would be the link I would try to make.

Kennedy: I'm curious if you have any advice for young survey methodologists. Are there any research gaps that you think are not getting the attention they deserve?

Groves: Well, I guess I don't have much hope for surveys. You guys are down to a nine percent response rate or something like that? I just don't see anything that we have in our toolkit to overcome the massive social forces that are producing that behavior. We're not smart enough to induce better response rates. So I think the future is really messy. It's piecing together a variety of data that are relevant to the phenomena we're interested in. I think Partha and Hermann's world of much more sophisticated statistical modeling and blending data together is the future of surveys.

3. SURVEY METHODOLOGY AS AN ACADEMIC DISCIPLINE

Lahiri: You worked with Graham Kalton, Stanley Presser and others to establish JPSM (the Joint Program in Survey Methodology), which is considered one of the most successful multidisciplinary and multi-institutional programs in the world. Can you please tell us how the collaboration came about and what challenges you encountered?

Groves: Given that Hermann is here, I remember a lunch we had in 1990 in that restaurant in the basement across from the White House. Hermann was really a mentor to me, and I was this Midwestern yokel. I had been interested in the idea of creating a multidisciplinary program. I had started a little effort at Michigan that was embedded in the sociology department. It was one of these creations where as soon as it grew to be visible, the antibodies of the host began to attack it in various ways. I actually think I taught in that program for free for about six years. I just thought it was a good thing to do. But during the lunch, that I probably had to pay for, I remember you said, "make it short." So I wrote up about five pages, and initially that was my contribution.

Habermann: Yes, the JPSM was the only initiative that survived of all the things that Michael Boskin (chair of the Council of Economic Advisors) wanted.

Groves: Boskin really wanted to do something good for the federal statistical system. At this time, I was an associate director at Census on leave from the University of Michigan, but when this happened I stepped down from that position and became a researcher at Census to avoid any conflict of interest. Then the National Science Foundation issued an RFP for the joint

program. It was clear that it had to be sited in Washington. Stanley Presser and I talked about how we could do that with Michigan and Maryland. Then Westat became an attractive partner for multiple reasons. One, they're the biggest federal contractor on surveys; two, they had statistical talent principally at that time, which would be important for students at the Maryland campus.

My favorite story about that time was when we were laying out the curriculum. We let the statisticians and the social scientists go off and determine what a master's-level person needs to know to be a great survey methodologist. The two groups came back with 120 credit hours or something. They all said, "Everybody has to know what I know and they also have to know what I know I don't know." We had horrible fights to get it down to something that would actually work. It was still a pretty heavy master's program.

Lahiri: My next question is related to our co-teaching the JPSM PhD seminar course. Do you remember how each year you and the Ann Arbor students would drive out in a van to visit Maryland and then the Maryland group and I would drive out to Ann Arbor?

Groves: Oh yeah. That was great fun, wasn't it? That was really also social bonding. That was one of the things we knew was a real burden on the program—keeping the two sites together, both on the faculty side and the student side.

Lahiri: They still do it, but instead of driving, they fly.

Groves: Soft.

Lahiri: I have been in math departments and statistics departments, but I have never seen this kind of course. The seminar emphasizes a heavy involvement of PhD students, particularly reading and discussing papers, and closely interacting with fellow students and faculty. What are your views on the importance of such a course in a PhD program?

Groves: When I was doing that sociology program, I realized that the typical entrant into a graduate program is above-average in intelligence and academic performance, but most of them haven't experienced a certain way of reading technical material in journal articles. They generally have been taught to read to absorb what was found in the work and remember that. But actually, I think, professionals read technical material to find out what needs to be done next. In addition to critiquing the findings, professionals find holes. It's all about—given this and other things we know, what's the logical next research step? The goal in that course—I don't know



FIG. 3. Partha (far left) and Bob (third from right) during PhD Seminar visit from JPSM students, 2009.

whether we ever fulfilled it—was teaching them to read in a completely different way. To implement that, we read articles to motivate what the next research project ought to be. As an instructor, my memory was: you can never prepare for a class like that. We were constantly ad-libbing because it was real-time. There were no lectures or anything. It was a course where students got out of it what they put into it. I think there were actually some real papers that came out of it, but it was great fun too.

Lahiri: Did you create that kind of course because it was a multidisciplinary program with collaboration between statistics and social science?

Groves: The whole program was conceived as an intellectual bridge between multiple domains of knowledge. That course, needed to be jointly taught (by one statistician and one social scientist), or we wouldn't have achieved that. It was good that the students were confronted with these two knowledge domains; some could do the bridge better than others, but the fact that they had to think about the bridge was important.

4. JPSM'S IMPACT ON THE FEDERAL STATISTICAL SYSTEM

Habermann: Given that, if today we were doing this, should we be concentrating on how to provide assistance to the official statistical system. That is, should we develop a mechanism to provide contract R&D support when necessary? I think you essentially did that with the NSF research node project. In the end, I believe it was intended to provide research and development support from outside the official statistical system to the agencies.

Groves: I don't know. My mind isn't settled on this stuff. Remember how my thinking was framed—I came out of this survey research center where research innovation and measurement innovation is in the DNA of the place. It too has this data collection production culture and the research culture, but the research culture dominates the data collection culture. The agencies are quite different; culture change takes time. We must be patient.

Habermann: How important is the research culture? I mean, one of the reasons you developed the NSF node project was to get academics to focus on problems that the Census Bureau was concerned about.

Groves: The Census Bureau is not unlike an auto manufacturer. In 2015, you need a unit that's thinking about the cars in 2050. That unit will be a loss leader; you will never make money off of it in any way. They'll have a 90% failure rate, but every once in a while they'll come up with something that will completely transform the production side. That's the "R" side. Then you need a "D" side. For example, responsive design isn't research in my view; it's really development. There's no discovery that's going on there; it's implementation of research ideas. That has more support than the "R" side in these cultures.

A very common course of knitting these together is to let a migrant go from the "D" side to be a leader on the product side. It's not unlike what is happening in car manufacturing. You take a designer and have them run the factory. They end up being a different factory manager than a production manager. Historically, real successes on innovation in the agencies have occurred

with that kind of move. That, however, takes leadership to say, “This person isn’t the best manager, but this person can be a wonderful innovator. Let’s boost them up in the management when they move over to the production side.”

5. THE US CENSUS BUREAU

Habermann: Let us talk about your work at the Census Bureau. What you said about the future of surveys—what does that mean for the future of the Census Bureau?

Groves: The US decennial census is a constitutional mandate. In my lifetime, I can’t see any changes in the basic enumeration mandate of the census. I think we’ll continue to try to count everybody, unless there’s a radical change in the political philosophy.

Now let me put my bureaucrat hat on. One wonderful thing about the decennial census is that it spurs innovation in the other ways because of the cycle of funding. Every other statistical agency is stuck with a flat and gradually declining budget. So the fact that you have these ups and downs is a wonderful attribute of the Census Bureau.

I just visited a young group of people who are working on a paradata and real-time survey management platform. So imagine this room filled with plasma TVs, and you can sit there and watch interviewers drive down streets and stop at an address, and you can double-click on the interviewer and you have the interviewer’s workload. They’re using paradata for predictive modeling of both where should the interviewer go next, and is it time to abandon this case.

If you thought about the Census Bureau as separated into decennial versus everything else, and you imagined breaking off just the decennial as another organization, let the funding go up and down every ten years—the survey would gradually atrophy, I think, because there’s little investment capital in surveys anymore.

Habermann: What about the argument some would make that the culture of the decennial census tends to inhibit innovation in survey methodology?

Groves: That’s absolutely true, but I don’t think that’s an inherent feature. I think that’s a leadership challenge in how you manage that program. I think it’s fair to say this decade, the 2020 crew, has larger representation from non-decennial people than was true in 2010 and 2000. There was a tendency once you entered the decennial that you would stay there and no one would ever replace you. Now there’s at least a little movement.



FIG. 4. Bob and Cindy Groves during swearing in at Census with Secretary of Commerce Gary Locke, 2009.

Habermann: I would like to go back to your time at the Census Bureau. When you arrived as director, you had some ideas about what you wanted to do, both in terms of the decennial and the other parts of the Census Bureau. Can you reflect on the reality of what happened and how it differed from your expectations?

Groves: The fundamental thing that most people have forgotten, but I will never forget was that when I walked in, there were predictions of doom for 2010. The development of a handheld collection device had just blown up, and so I walked in expecting to have a horror show of management problems and Congressional oversight. But I discovered two things: one, the team under Arnold (Jackson) was much better than outsiders realized. They had re-assembled; they had a credible set of activities and management structures. But the biggest gift that I was given was Tom Mesenbourg, as a deputy director, who is a fantastic manager and very smart. So while I was prepared for just horrible things, it turned out much better than I ever thought.

When I walked in, it was July of 2009. So if you think about that—there’s nothing you can do with 2010. The only thing I did was to push paradata and real-time reporting, and I was able to get in a little. We had daily reports and some predictive modeling happening. Then there were just little interventions that were required.

The other thing I wanted to do was create some culture of innovation. We started a program where the lowest clerk in some regional office could suggest something that would get to me or a group around me, and we’d give out little innovation grants. Then the other thing was the NSF research nodes. Those were the fun jobs I enjoyed while I was doing all the other stuff.

Habermann: Are there some things that stand out that you would have done differently?

Groves: Well, there are tons of things. I made a mistake on the evaluation studies. Embedded in every decennial there are a lot of experiments and evaluation steps. I should have intervened on those.

Habermann: Intervened to make them more rigorous?

Groves: Some of them had design flaws or weren't addressing the right questions. One of the weaknesses in a place like the Census Bureau is the lack of training in experimental design. So their experiments are designed by survey statisticians, and they're not as useful as they should be. So I blew that. And that's really a bad failure because those are the seeds for 2020, or they should be.

Habermann: I never made a mistake while I was there.

Groves: I know.

Habermann: You mentioned that you saw the statistics agencies as the engine to produce information needed for democracy. I think this idea is really crucial. Do you think we've lost some of that, and why?

Groves: The advances in technology have far outpaced the advances in statistics and surveys. The federal government now has CTO and CIO positions; they create data officers, and the statistical agencies are not even at the table—as if statistical agencies don't know anything about data! So the computer scientists have really dominated the way we think about data. Also, what we do isn't as sexy as what they're doing.

Habermann: That's absolutely right.

Groves: What the federal surveys do is like infrastructure, like bridges. We don't care about bridges until they fall down. We just assume they're there, that somebody must be looking at this.

Habermann: That brings up another issue. When engineers build a bridge they have a sense for how good the bridge has to be—what is its carrying capacity. Do you think we have similar criteria? For example, how good does the monthly unemployment data need to be for a well-functioning society?

Groves: But the monthly unemployment rate doesn't have one use—there are a thousand uses, and you can't optimize quality if you have a thousand different uses. I think that's the fundamental problem of most statistical agencies.

Now let me comment on something you said earlier about the public/private partnerships because I think some countries are doing this better than we are right now. And that's a real impediment at this bridge moment, as I see it. If the Census Bureau, for example, were not burdened by all of these regulatory constraints to cooperate with the private sector, my hunch is that things would be better. So we've set up a regulatory framework that restricts the behavior of these agencies in a way that isn't good for the country.

Habermann: Certainly public/private partnerships are very popular at the moment. However, there is also discussion about whether these entities have such different missions that the concept of a partnership may not be meaningful. For example, one of the definitions of government is it's allowed to do only those things which somebody has said it can do, whereas the private sector can do anything it can get away with that's not proscribed. In addition to the for profit private sector a sector that is becoming increasingly important in the production of statistical information is the not for profit private sector. Sometimes these are called NGOs.

Groves: That's an interesting sector, and they are bridge organizations, in a way. And it could be that



FIG. 5. (Left) Groves counting the first person in the 2010 Census in Noorvik, Alaska, above the Artic Circle. (Right) Public outreach for the 2010 Census.

that's the other thing that needs to happen, the foundations need to step up in this decade and help.

Habermann: You were talking about alternatives. Pew Research Center, I would argue, has become the voice of statistics, at least in Washington D.C. area.

Groves: It's interesting. So why has Pew been able to do this? One restriction Census and BLS have placed on themselves is that they must be purely objective. They've come to interpret that as, "we only present the data, we don't comment on the data," and they've gone even further to say—you know, we have these statisticians who review every word, and so on. But what Pew brilliantly does, is to have journalists, sitting next to the survey researchers. You have people whose job it is to write to the mass public, and they're working hand-in-hand. There's nothing in law that prevents the agencies from trying to do that.

6. DATA SCIENCE

Lahiri: What's your vision for Georgetown's Massive Data Institute?

Groves: I'll say what I think is going to happen. I think the continued decline of surveys will be part of our future. There will be good people—who we sometimes think of as "survey engineers"—who will fix things here and there, and that will keep them going for some time. That will go for a while. And then at a certain point, I think the cost of doing those little fixes will be prohibitive. I think we need about ten years of work that will keep probing for every particular survey purpose, new blends of existing surveys and more ubiquitous so-called "big data" stuff. This will be horribly messy; some of the data are terrible. In my terminology, these are not "designed" by us, these are "organic data," so we don't control the measurement process at all.

We have a bunch of statistical models where we have measurement error properties of indicators of the model. Those, I think, will arise as more prominent tools in a way you can treat these big data as observations on things you're interested in, filled with measurement error. There's a missing data problem for coverage that the survey people jump on most quickly. So I think there's a bunch of statistical modeling work that needs to be done, in order to locate what portion of the stuff we've been measuring and estimating in surveys could be more efficiently measured in blends. Then, I think, the next ten years would be redesigning the surveys to be mated to those organic data. The other surveys for which we can't find other data resources need

to be protected, or at least re-examined to decide how much we want to do with that sort of measurement.

So what are the impediments to that world? The data owners of this new world are mainly private sector firms who don't have a mission of serving the common good. They're there to make money; rightly so. So there's a gigantic access issue. Connected to the access issue is the privacy issue.

My belief, we'll see whether I'm right or wrong, is we actually need a new institution that will build a safe environment for private sector folks, to allow access to their data for common good purposes. And we will blend the federal data in statistically. So we're at another one of those bridges—we have great survey statisticians and great computer scientists. We need to form a bridge between those knowledge domains—and we need the social scientists, too. The social scientists know what questions to ask. But the social scientists and statisticians generally don't have the big data analytic tools yet. That's going to be a half-generation, I think, but we can eventually train people who can do both of those things.

In a way, it reminds me of pre-JPSM. When I was growing up intellectually, I had two mentors: I had Leslie Kish, a survey statistician, and Charles Cannell, who was a social psychologist. They didn't get along, really. When I would talk to either one of them, they would sometimes say, "He doesn't know what I know. Why are you talking to him?" And I always thought they were talking about flip sides of a coin. They were so close, but they couldn't realize how close they were. I think we're at another moment like that; it's just so obvious that we have these two knowledge domains and we have to put them together. We're not going to make progress until we do that. We have to get them together.

Habermann: Do you see the academic world as playing a leading role in establishing this neutral arena where you can bring all these people together?

Groves: I don't know. I think the huge impediment is access. The statistical system is a wonderful building block of a democracy—it really, really is. It's not just platitudes. You can't run democracies without objective data. If we lost that because all the data is in the private sector and they say, "nah, we don't want to do this" or "we'll do it on our own terms" it's really important to solve this problem.

Lahiri: I think one potential problem where people from different disciplines are coming together is small area estimation.

Groves: I think the small area estimation family of models has a bright future with big data because some of the big data will essentially be observations at a higher level in a nested model.

Lahiri: It seems like many federal agencies are interested in this kind of problem.

Groves: The traditional approaches are quite weak on spatial and temporal granularity. For example, national crime data is of interest to almost no one in the criminal justice system. They want to know what happened on this block yesterday. They want more timely and finer-grain data. That's hard with the survey paradigm. It really is hard. We don't do very well at that.

7. EARLY DAYS

Lahiri: I think many students and others in the field would be interested in learning about your intellectual upbringing.

Groves: So I was a sophomore at Dartmouth, and James Davis—who was the inventor of the General Social Survey and a quantitative sociologist—had left Chicago and was at Dartmouth. He taught a course, and it was the early days of time-sharing computing. That was like the first interactive computing, where you didn't have to have cards. For some reason, Dartmouth was kind of a seed of those developments. Davis taught a course where we had a primitive interactive statistical platform, and he would essentially say, "Here's a data set, and here's a codebook. Write a paper." We had a little computer lab, and I still remember being in there at three in the morning on an old teletype terminal, and I think I had done a factor analysis of all variables—incidentally stupid move. And it was like, a stack of output. Took forever. And then I remember going through that and going, geez, I learned nothing! And Jim said, "you shouldn't have done that." But he let us have that freedom, and he was there kind of as a safety net. This was a pretty small class, as all Dartmouth classes are, and so he taught me statistics. I thought this was the coolest thing ever, and I started taking computer science classes and other math classes. Then when I was applying for graduate school, I was trying to find a place where I could do both sociology and computer science as joint program, and Michigan led me to believe that I could do that. When I arrived in Ann Arbor it became crystal clear that that was not possible. And Jim was a great friend of a man who recently died, Phil Converse, a legendary political scientist. At that point, I knew French, and Phil was working on a book on

French political decision-making that would now be a Bayesian hierarchical model—he didn't have those tools, and he was doing all sorts of things.

Jim sent a note to Phil saying, "I have this kid coming to Ann Arbor, why don't you talk to him." So I just started working for Phil for free and that put me in the Survey Research Center, which is just a wonderful environment. I started going to seminars on different things and discovered Leslie Kish—and that has its own story. So I remember taking a sampling statistics class in the summer, and Leslie came in to visit the class. He was probably in his sixties or so at this time, and he gave a completely garbled presentation. I had no idea what he was saying. But at the end he said, "I'm always on the lookout for research assistants, and if you're interested, come by my office." This is not, in retrospect, so funny. But I thought everyone would do that! So this lecture was in a building about half a mile away from the Institute for Social Research (ISR). I remember running from that building to Leslie's door, thinking I had to be the first in line! And it turns out I was the only person there.

But I remember going to Phil—I deeply respected Phil—and I said, "Phil, I found I'm interested in this survey sampling stuff, and would you mind if I started working for Leslie?" Phil was just a great human being and he said, "Oh, I'm really interested in that, too. I don't blame you. If I hadn't become a political scientist..."—and at that time, he was a National Academy of Science member. I mean, one of these towering figures, and he gave me his permission. So then the way you learned at ISR as a student there was, you literally did the sample design, from the bottom. And we wrote interviewer instructions and the like. That was a wonderful apprenticeship. Then I started discovering that the sampling statistics only went so far and there's all these other quality features of surveys, and I wasn't learning that from Leslie. That's how I hooked up with Charlie Cannell.

Lahiri: Can you tell us about ISR statistics visitors you met when you were a student?

Groves: Morris (Hansen) and Leslie were sort of contemporaries. Morris, Bill Cochran and Alastair Scott all visited. Leslie had some fund to bring in visitors. So even though I was a graduate student, I was able to meet all these guys, which was fascinating. Hansen was just a powerful intellect and personality combined—just a force.

Cochran had worked with Fisher at Rothamsted Experimental Station, and they used to do Christmas skits there. At one of the dinners, Cochran—he must have

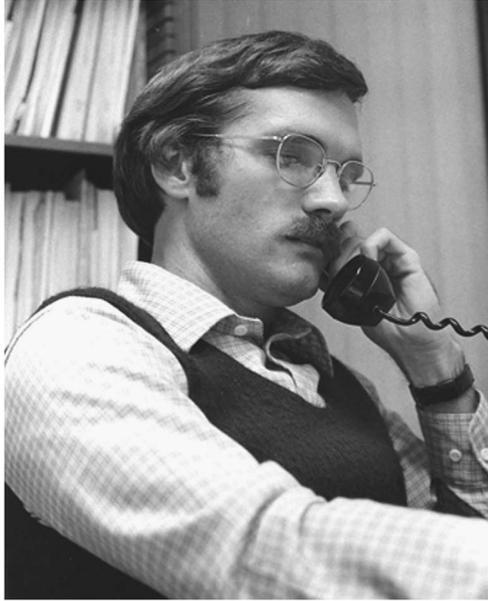


FIG. 6. Bob at ISR, circa 1980.

been in his late seventies at that point—he stands up and he says, “I’d like to sing some songs that we used to?” and he sang this song, and it was making fun of Fisher’s treatment of missing data. And the song—I’ve forgotten the lyrics and everything—but it ended by saying that when the missing data got to a certain level, don’t worry about it, because Fisher will make up all the stuff anyway. And it just brought down the house and was really funny. So Leslie was important to me. He taught me a whole lot of things and was a very caring person, too. ISR is actually a wonderful culture. There is an unusual number of people who care

about younger people, I think. It’s a very nurturing intellectual environment. That shaped how I fundamentally think about everything, actually.

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