Statistical Science
2001, Vol. 16, No. 3, 295–309

A Conversation with Ramanathan Gnanadesikan

Jon R. Kettenring

Abstract. Ramanathan Gnanadesikan was born on November 2, 1932 in Madras, India. He received his B.Sc. (Hons.) and M.A. degrees in 1952 and 1953 from the University of Madras and also studied at the Indian Statistical Institute during those same two years. In 1953, he came to the United States to pursue a doctorate degree in statistics at the University of North Carolina in Chapel Hill. He studied with Professor S. N. Roy and received his degree in 1957. Then he began a 34-year industrial career at Procter & Gamble, Bell Laboratories and Bellcore (now Telcordia Technologies). His time in industry was interspersed with teaching assignments at the Courant Institute, Princeton University and Imperial College. He served as professor of statistics at Rutgers University from 1991 until his retirement in 1998. In 1965, Ram married his statistician wife, Mrudulla, who is well known for her work in statistical education. They have two sons, Anand, a researcher in oceanography, and Mukund, a physician specializing in childhood psychiatry. Ram is a Fellow of the Institute of Mathematical Statistics, the American Statistical Association and the American Association for the Advancement of Science and an elected member of the International Statistical Institute. He was elected to the Order of the Golden Fleece for leadership while a student at the University of North Carolina in 1957, honored by the Association of Indians in America in 1989 for his contributions to advance information technologies and their impact on the communications industry in the United States, and singled out by the State of New Jersey Senate for unique contributions to arts and letters and to greater understanding between the people of India and America in 1989.

The following conversation took place on January 18, 2000, in Tucson, Arizona.

WORKING IN INDUSTRY

Kettenring: Ram, during your days at Bell Labs you built one of the most successful statistics research groups in industry. How did you manage to pull this off?

Gnanadesikan: When I joined Bell Labs actually it was a very small group. John Tukey was the head. He was splitting his time between Princeton and Bell Labs. The other members of the group were Milton Terry, Martin Wilk, Mike Healy (a visitor), Anne Freeny, Shirley Reed and Marilyn Huyett. This was the group in 1959 when I joined. The following year Bill Williams, Liz Lauh and Colin Mallows joined us. It was a unique culture, but it didn't have certain of the characteristics that the entire statistics effort took on in later years.

What do I mean by the culture of Bell Labs? As Hendrik Bode, who wrote a book called *Synergy: Technical Integration and Technological Innovation in the Bell System*, described it, the essence of the success of Bell Labs was the synergy that brought together people with very different skills, very different approaches, experiences and training and who shared a certain value for this interaction across borders. Statistics was imbedded into that culture. That was a big plus to start off with because

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it was the hallmark of the statistics work at Bell Labs.

Nevertheless, the group was small and we tended to operate individually. We had our research interests, as well as interactions with other people in the synergistic environment. However, the group didn't have that much of a cohesive mission or a set of goals to reach. That is what started happening with the evolution of the department.

Kettenring: When and what were the early steps of this evolution?

Gnanadesikan: For example, after John Tukey, in the early 60s, Milton Terry became the head of the department. I was promoted to being a supervisor six months after I got to Bell Labs and Martin Wilk was splitting his time between Rutgers and Bell Labs. Then Colin Mallows and Bill Williams joined us, and all of the time, of course, we had management that consisted of people like Bob Prim and Henry Pollak, who were extremely enthusiastic about supporting the growth of statistics. The management's value system, along with the fact that different people began to manage the statistics effort, provided good incentives for us. Milton Terry, for example, emphasized the importance of the business problems of AT&T. He got involved in demand-modeling kinds of activities for Wide-Area Telephone Service (WATS). He did it pretty much by himself, but the value was there for us looking at real problems that were of importance to the telephone industry. And then Martin Wilk became the department head, when Milton Terry got promoted to being an Assistant Director, and Martin gave up his Rutgers connection and brought a lot of enthusiasm and energy into the department.

The push in the growth of numbers of statisticians started because of the successes that we had already had in demonstrating the value of statistics in such a synergistic environment as Bell Labs. We pushed very hard to solve significant problems and the management was there to support our growth. John Tukey, even when he was the head of the Statistics Group, had an interesting job of "bracketing" Bob Prim. In one capacity, as head of the statistics group, John Tukey reported to Bob Prim and, in another capacity, as Associate Executive Director, John Tukey had Bob Prim reporting to him. So there were all these subtle management influences and help, but primarily it was in the hands of the people that did the statistics work and demonstrated the relevance of statistics to a large number of significant problems. This is what resulted in the growth. From about 1964, when Martin Wilk became the department head, until 1968, we grew from a group of about six or seven people to well over 25.

In 1968, I was promoted to head a second department. The following year when Martin left Bell Labs for AT&T, Colin Mallows succeeded him. All along we ran the two departments very closely. It was an administrative convenience to have two departments, but the mission continued to be the same one of working on exciting problems of statistics within a milieu of science of technology involving a lot of very smart scientists and engineers across the board. So that's the context of the success story.

Kettenring: Are any of the principles that were involved in this story transferable to other environments, such as university statistics departments?

Gnanadesikan: The culture of the university is considerably different from an industrial research environment. My experience has been that in industry one necessarily starts small in terms of numbers of people. You grow by hiring others who share important values and attitudes even if they bring different skills. You identify opportunities with significant challenges, couple the resources to address such problems almost in an imperceptible or an unobtrusive way, because you are dealing with very smart people all around. Then you succeed, and success breeds success.

In a university, the model typically is one of hiring individual people who come in with their own strengths. It is very difficult to have a mission for the entire department. So you have a "multi-modal" kind of a situation with each professor or faculty member having her/his own interests. Although there is some interaction within the department, there isn't anywhere near the kind of synergy that one finds in an industrial research environment. On the other hand, the university is full of opportunities, because there are engineers and scientists, biologists, economists, physicists and computer scientists in other departments. But there isn't an actual bringing together unless somebody makes the effort to do that. So that's the reason I think the model in the university tends to be different from an industrial environment.

Kettenring: The hiring process of course is very critical, especially when you are talking about small groups of people. Did you have any particular approach to hiring people?

Gnanadesikan: Besides looking for very smart people, you really need to look at a set of values and attitudes that people bring with them. In order to be a broad department you need a variety of skills, a variety of approaches, and a willingness to be open about trying different things. But you also need to have the shared sense of curiosity, willingness to listen to problems that other people have, relating your own expertise to their expertise, solving problems that are of importance to them, and finally benefiting your own discipline by feedback from solving those problems back into your discipline. So you find this out, whether the people have these qualities. It's a hit-and-miss thing maybe, but overall I think we made very few mistakes and we found the right kind of people. That is really the reason for the success story at a place like Bell Labs.

DATA ANALYSIS AND METHODOLOGY

Kettenring: Data analysis seems to have been at the core of much of the statistical activity at Bell Labs. Can you say more about how this came about?

Gnanadesikan: In 1959, when I joined Bell Labs, data was very much a part of almost everything we did. The following summer, David Brillinger, who was a first-year student of John Tukey at Princeton, was a summer student at Bell Labs. I have had a delightful set of interactions with David starting from then. At one party that summer or fall at David's house, John Tukey, Martin Wilk and I were there together. We were talking about the state of what, at that point, was the major focus of a lot of academic statistics, namely, mathematical statistics. Some of us felt that, while no one can dictate what people should work on, a lot of what was done as mathematical statistics really fell between the stools of mathematics and statistics in that it was neither a contribution to mathematics nor was it a contribution to statistics in the sense of being useful for looking at data or analyzing data.

We were wondering how to bring back the original focus of people like Fisher, Pearson and others into statistics in terms of data being the primary concern of statistics. And I remember very well that John Tukey, in his usual fashion, sat silent for a while and then snapped his head up and said "I think what we are talking about is data analysis." That, as far as I know, was the birth of the phrase "data analysis." I myself didn't think that it was a good idea to have a new term because of the obvious fact that data was, after all, historically the start of statistics. I thought that just saying that statistics should be concerned with data would be fine, but John Tukey was right in coining that term. It really did start a revolution in data analysis. More and more people began to recognize the importance and the central role of data in statistics. I think many years later Marvin Zelen was the one that said that statistics to him was the science of data, and I think that's a very apt description of what statistics is all about. It's the science of data.

Kettenring: What were some of your favorite examples of successful data analysis projects?

Gnanadesikan: Focusing on things that I personally was involved in, one of the examples that comes to my mind is the problem of speaker recognition that Martin Wilk, Marilyn Becker and myself in the Statistics Department (as well as later on, Ken Wachter and Paul Tukey), and a group of people in the acoustics area, Max Mathews, Peter Bricker, and Sandy Pruzansky, worked together on. In fact, John Pierce was the one that should be credited with bringing this problem to our attention.

In the Math Research Center we had this wonderful informal tea everyday that brought people together to talk about anything that was on their minds. One day John Pierce, Max Mathews and I were standing there and Pierce said, "Hey, there is this problem called speaker recognition; maybe the statisticians and the acoustics people ought to get together on this." That was kind of an informal introduction that resulted in a collaboration that lasted for years in solving, or attempting to solve, the problem of speaker recognition. It was one of the earliest attempts. In those days, speech recognition was much farther along than speaker recognition. The idea of identifying people from their voices (is there anything unique about people's voices?) was a massive problem involving groups of acoustics people and statisticians. I had to learn a lot about acoustics as an area, and the acoustics people learned a lot about statistics before we were done. So, that's a problem I remember with great fondness.

Another example was one that had to do with long-term aging of semiconductor devices that were used in a transatlantic cable (the TAT-5 cable) that AT&T was laying under the Atlantic Ocean. We were looking at long-term aging of transistors and diodes that were used in the repeaters in these cables. This involved our working with engineers who were based in Allentown and Reading, Pennsylvania, and a group of us, Innis Abrahamson, Jane Gentleman and myself, were involved from the statistics side. That was again a very large-scale problem which lasted for at least three years. It started by looking at the data on aging, but it ended up with methods for selecting the devices that actually went into the cable repeaters. In those days the gold standard was that anything that was made had to last at least for forty years, and as far as I know those cables are still working. That was yet another data analysis project on which I remember teams of people working together from several sides of a problem.

Actually, when I started at Bell Labs there was a group of psychologists, Mort Deutsch, Seymour Rosenberg, and Bob Krause, who would wander into the same tearoom I mentioned. They were in fact my first data analysis consulting contacts at Bell Labs. I learned a lot about human memory and learning from those people. One of the things that they tended to do was to use so-called standard inference tools of statistics. I never realized how much we statisticians had emphasized tests of significance, but their exclusive tool often was to do an analysis of variance and publish the results with their p-values, or an indication of whether it was significant at some arbitrary level of significance. I learned from them that one of the things we needed to do as statisticians was to develop better and more revealing methodology for the analysis of variance.

Continuing further, after you joined us in 1969, we had that large management science problem, working with people at AT&T on how to establish a rate of return for the company on the basis of the risks that it takes. This brought us in touch with economists, who of course have thought a lot about risk and rate-of-return, the idea of betas and various measures of risk of stocks and so on. We took a completely statistical approach, while the economists kept us honest about relating it to the risk side of things and their models of risk and rate-of-return. That again was a very large-scale data analysis project that lasted for several years.

In each of these cases, there was a substantive problem that we worked on, with people that brought other skills and backgrounds from other disciplines to it. But we also extracted an amazing variety of methodologies feeding back into statistics. To mention a few things, the speaker recognition problem was my first real exposure to large data sets. Large in both dimensionality, because the number of variables was in the hundreds, and large also in the number of repeated observations. We were talking about literally millions of observations. This now was in the mid-sixties! And in fact, it got me thinking more about something else that became a major part of my methodology interest in graphical techniques, because with such high-dimensional data and large numbers of observations, graphical display of data is clearly the way to go. So while we were doing that work, in fact, I remember that some people from Los Alamos and other groups came out to talk to us, having heard about our work on large data sets. They came out from the computing areas of Los Alamos to look at what we were doing by way of graphical displays of large data sets. As another example, the robustness ideas that you and I got involved in, residuals and their relationship with robust estimates, those got shaped initially in my mind when we were working on the problem of long-term aging of semiconductor devices for cables. Because it was clear that there were some really peculiar things in that data set, and to look at all of that data would have been really impossible unless we had a robust background against which we could detect the mavericks that were departing from normal behavior. So that fed back into methodological interests later on. Moving on, the management science problem of risk versus rate-of-return got you and me thinking about various methodologies relevant to classification and clustering. So, both in terms of the contribution to a substantive problem and in terms of the methodological feedback to statistics from working on those problems, those are some of the examples that come to my mind.

Kettenring: Is there another example of a statistical method of general use that came out of the speaker recognition research?

Gnanadesikan: Yes. One form of the data in that problem was a matrix of spectral energies cross-classified by something like 57 frequency bands and 250 ten-millisecond intervals. So it's a two-way table of data in a single utterance of a spoken word. One of the things we did with that data was to look for nonadditivities in that two-way table, whether the frequency and time aspects would be additive or would have some interactions. It seemed that a simple starting point would be to look at Tukey's one-degree-of-freedom for nonadditivity. Now I am talking about work that was done by us in the 1964–1965 time frame, which I talked about at the 1966 IMS meetings in London. One thing that seemed very natural to do to study the nonadditivities was to plot the residuals from additivity against the product of the row and column effects. For those that know the work of John Tukey and Don MacNeil, this is essentially the same thing that they later on dubbed "The Diagnostic Plot." There is a difference in a scaling factor, but it's essentially the same kind of plot. Also their emphasis was using the plot as a way of actually finding out what specific power transformation would enhance additivity. But we used it just as a way, first of all, of detecting the presence of nonadditivity of the sort addressed by Tukey's one-degree-of-freedom test. This is something that is very obvious, especially now that it's even described in textbooks, but at that time it was not a method used routinely by people. The method was graphical in nature but it was also what I like to call a useful technique for "exposure"—namely, to show the presence of nonadditivity and then to guide you by trial and error, if necessary, to determine what kind of transformation of the data would diminish the nonadditivity or enhance the additivity.

Kettenring: You've really used the data analysis problems as a strong stimulus for your own research in statistical methodology over the years!

Gnanadesikan: Yes, indeed. In a nutshell, I think that is really the thing that I've enjoyed most—to contribute to, and think about, solving substantive problems, and then benefit my own discipline of statistics from having worked on those problems.

Kettenring: Of all the examples that you have given us, and others that you didn't get around to, is there one that stands out simply on the dimension of providing a leap forward in terms of the problem that you were confronted with?

Gnanadesikan: I guess the speaker recognition problem would fall under that category. In a different sense, the problem of risk and rate-of-return also belongs there, not so much in clarifying the economics aspects, but in a surprising way. The definition of rate of return in terms of risk that the Federal Communications Commission specified ended up depending on demonstrating through statistical means that the rate of return should be commensurate with the risk. So, in terms of two very different kinds of impact, those two examples stand out in my mind.

Kettenring: You've given us some interesting comments about the origins of the term, "data analysis" and today we find that people use "data analysis" and "statistics" interchangeably. From your point of view, is there a useful distinction?

Gnanadesikan: As I said, I didn't think that it was a great idea initially to coin a new term for this area and yet certainly in the beginning years, at least for the first decade after the 60s, and maybe even into the 80s, it did serve a useful purpose. But then, as usual, what happens with these kinds of things is that people began to do their usual thing under the new umbrella of "data analysis." So, maybe it's not worth distinguishing the two, but it is important to keep in mind that at the center and core of statistics lies data, including either the lack of data or an excessive amount of data. These are two extremes that have a lot of challenges to be addressed. I'm not as bothered by data analysis and statistics coming back together. In a sense, it is history repeating itself. We are going back to our origins.

Kettenring: Another distinction I've heard you talk about over the years is the difference between consulting and research consulting. What is this difference and how does it relate to what we have been talking about?

Gnanadesikan: I think a lot of people use the word "consulting," especially when it's done in universities, for things which are fairly short turnaround. Somebody walks in and says, "I've got this data set, what do I do?" I guess the caricatured description is "go do a t-test", but the essential characteristic of that type of consulting is that it's very short-term. There's not as much learning that goes on, especially from a statistician's point of view, either about the other person's discipline or where the problem came from. And often the formulation of the problem is somewhat narrow and focused. Research consulting tends to be long-term. Its nature is such that there has to be a lot of learning, as in all the problems that I mentioned. There is a lot of time involved in learning each other's disciplines and a lot of reworking of the data and the hypotheses and where do we go from where we are? These tend to take not months but often years.

I realize that not all statisticians are going to have that approach to consulting, but research consulting to me has that long-term aspect. On the other hand it also has tremendous value in feedback into methodologies, keeping the frontiers of new methodological development constantly moving forward. That's what I call "research consulting." It's research in the substantive area, and it's research in statistics as well.

Kettenring: Going back to the early 60s, what was your involvement in the flurry of research activity on data analysis?

Gnanadesikan: As I mentioned, I recognized in my interactions with the psychologists the need for more informative tools in the context of analysis of variance. Of course, even before that I was involved in thinking about design and analysis of experiments starting with my thesis work and my subsequent employment at Procter & Gamble. Analysis of variance and design of experiments were kind of the bread and butter of much of my interest in those days. But I began to recognize that what was available by way of tools at that point was not adequate. Martin Wilk also had a very strong background in design and analysis of experiments, and Cuthbert Daniel had come out with his paper on half-normal plotting. So, given my multivariate interests, Martin and I started interacting: what about some revealing and useful graphical probability plotting types of tools in the context of analysis of variance? That work got started in the early 60s.

In 1961 there was a session of invited papers at the IMS meetings in Seattle, and the speakers at that session were John Tukey, Cuthbert Daniel and myself. I presented the work Martin and I were doing developing the one-degree-of-freedom graphical probability plotting technique, which was in a sense a multivariate analog of the half-normal plot of Cuthbert Daniel for univariate data. It was in the context of single degree of freedom decompositions, things like two-level factorial experiments. The session was chaired by Alex Mood, and the discussants were Alan Birnbaum and Art Dempster. John Tukey gave the paper, which was subsequently published in the *Annals of Mathematical Statistics*, called "The Future of Data Analysis." It is probably the classic paper in laying out what would become the major thrust of data analysis from then on through several decades. My presentation was on a particular graphical method but fitted in with the data analysis theme of that session.

Kettenring: What were your favorite papers on methodology research?

Gnanadesikan: I thoroughly enjoyed working on all of them. However, if I am to think of what might be some of the favorite groupings of papers, I probably would think of the paper I coauthored with Martin Wilk on probability plotting, which was the one in which we defined so called QQ and PP plots and various ramifications of those, and the series of joint papers coauthored with various people on probability plotting methods for analysis of variance and multivariate analysis of variance. I would also mention the paper coauthored with you on robust estimates, residuals and outlier detection with multivariate data and a number of papers you and I, often along with other coauthors, have published over the last two decades on clustering, classification, and pattern recognition.

MULTIVARIATE ANALYSIS

Kettenring: As you have just indicated, much of your own work has actually been on multivariate aspects of data analysis. How did you get involved in multivariate analysis in the first place?

Gnanadesikan: I was always interested in geometry more than algebra and thinking about things in a geometrical way, spatial thinking, became very natural. But formally, my first exposure to multivariate analysis was when I was a student of C. R. Rao at the Indian Statistical Institute. There was a strong Indian school of multivariate statistical things, including the theoretical kind of work that Bose and Roy had done, and C. R. Rao was involved in his own work in multivariate classification and applications in anthropometry and biometry. C. R. Rao had just finished, or was just finishing his book, *Advanced Statistical Methods in Biometric Research*, at that point. That was my first exposure and I think the combination of the two things was one of the sources.

Also, while I was at the Indian Statistical Institute for a year, by sheer luck, Professor S. N. Roy from the University of North Carolina was on a sabbatical there. I had a chance to sit in on his lectures and to meet him. That planted the idea in my mind that maybe I should go abroad to get a Ph.D. and that's how I ended up coming to North Carolina, although my contact there in terms of admission and so on was actually with Professor Hotelling and not Professor Roy. Eventually, I ended up working on my thesis with Professor Roy.

Kettenring: There was a story you told me once about raising elephants in India. What was that about?

Gnanadesikan: Professor Roy told the story when he was visiting Bell Labs while he, Srivastava and I were working on a book, later published by Pergamon Press, on the analysis and design of multivariate experiments. Roy's training was in physics, and like many early pioneers in statistics, he had the feeling that statistics has to be relevant to some real problems. The story is about a man in India who raised elephants. Somebody one day asked him, "Why do you keep on raising elephants?" and he replied, "Well, of course, to sell them." Roy then said that mathematical statistics often is in that same category; you do it because it's done, not because it has any value—I mean the elephant is obviously valuable for a lot of things, but you don't take time to think of that. You just say, "Oh I do it because other people want elephants." That was Roy's description of the state of mathematical statistics in those days.

Kettenring: Tell me more about the book with Roy and Srivastava and how it fitted in with your interests in multivariate analysis.

Gnanadesikan: The introductory chapter has a discussion of the philosophy of the roles of statistics in the real world, including the value of both formal and informal procedures of data analysis. Multivariate analysis, at the time we wrote the book, was developed a lot by analogy with univariate statistics. This is reflected in the book. Because univariate analysis of variance had a major emphasis on tests of significance, multivariate analysis of variance got started with a major emphasis on tests of significance. Just like in the univariate situation, in the multivariate case a null hypothesis is a straw man. But in a univariate situation, when you reject a null hypothesis you can do some simple things to find out what the real structure is. What is the alternative that seems most reasonable? In the multivariate situation, there isn't much you can say when you reject the omnibus null hypothesis. There's not much insight you can get from using those procedures.

Stimulated by a paper in *Biometrics* by Steel, David Finney wrote a short critique, which was justifiably quite negative on multivariate analysis, in the sense of its being narrowly focused on tests of significance. Then, at the first multivariate analysis symposium organized by Krishnaiah, Oscar Kempthorne gave a paper which was critical of most of multivariate analysis—"theoretical multivariate analysis" as he called it. I think the criticism was very well justified.

When Roy, Srivastava and I wrote the book, we had material that was new in two ways. One was the emphasis on graphical techniques to reveal the structure in data and not just do a test of significance and forget about the data. The other piece was that there are some challenging, still unsolved problems in the whole area of designing experiments from a multivariate point of view. If you had not a single variable but several variables, perhaps not all on an equal footing, how do you design an experiment that takes this into account? There are new designs that are needed for that kind of situation. Those were the two new pieces in that book, plus the philosophical part in the front, which talked about relating statistics to real world problems.

Kettenring: Later you wrote your own book on multivariate methods. How did this come about and why did you write this book?

Gnanadesikan: Over the years I had been working on various aspects of multivariate data analysis methods. I felt that there wasn't a book that really concentrated on data and data analysis techniques. I decided to structure a multivariate book according to the needs of multivariate data analysis. In 1966, which was the first year of the so-called general methodology lectures of the American Statistical Association, John Tukey gave one lecture on time series, and Martin Wilk and I gave a joint paper on multivariate analysis, multivariate methods, and multivariate data analysis methods in particular. That occasion, plus a second occasion provided by one of the multivariate symposia that Krishnaiah organized, gave me a chance to put together an overview of multivariate analysis that provided an overall structure, although not the details, to write a book. Then in 1969 I spent a six-month sabbatical period at Imperial College at the invitation of David Cox. I taught a course there and I found teaching the course was extremely helpful in actually developing the material in such a way as to be able to write the book. That was the start. It still took a long time because the book came out in 1977. So I took my time putting things together and trying it out in various forms. But the reason I wrote the book really was not only because I had done some of the work in multivariate data analysis, but also because I felt there was a need to have a new kind of book. I think that at the time that my book was published, it certainly was different from all the multivariate books in print. Subsequently, of course, there are other books that have also developed a data analysis focus.

Kettenring: Yours was not a standard textbook. Was it used nevertheless in classrooms? Who uses the book as far as you can tell?

Gnanadesikan: You are right; it is not a standard textbook. Amongst other things, most publishers like to have a set of exercises at the end of each chapter. My approach was to expose readers to issues and methodologies that are appropriate for today's data and then let them analyze their own data using these kinds of technologies and tools. Interestingly the book did get used in graduate courses on multivariate analysis. Stanford was one of the places that I know used it. Aside from universities, the book had a much larger reception amongst users of statistics: people from environmental sciences, the health sciences and the physical and engineering sciences. A lot of people wrote to me saying that they had read the book and were using this or that from it. So, it had a different kind of market in addition to the university one.

Kettenring: Was it translated into other languages and used in other parts of the world?

Gnanadesikan: In Japan it was a great success. Professor Okamoto translated the book into Japanese and, although there is a second edition of the book in English that came out in 1997, I

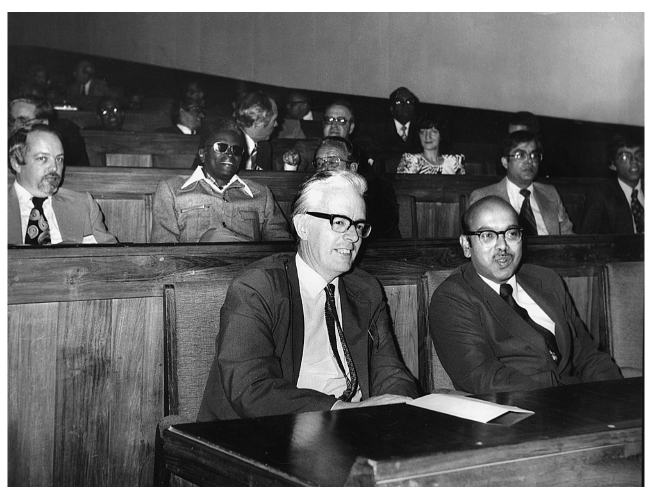

FIG. 1. *With Sir David Cox at the ISI meetings in New Delhi, India, in 1977.*

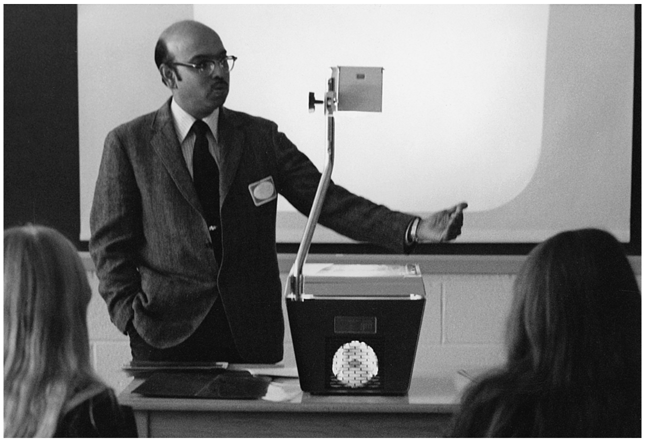

FIG. 2. *Talking to undergraduate students at College of St. Elizabeth on career day.*

should tell you that even now I get a small trickling of royalty checks from Wiley from the sales of the Japanese version of the first edition.

Kettenring: Was it error free?

Gnanadesikan: Okamoto is amazing. When he looked at the book and translated it, he had counted the number of points on every picture and, because there were fewer points on some plots due to overplotting, he actually wrote to me saying that you claim that there are something like 158 observations plotted on a particular plot but I only count 149. Certainly the Japanese version was completely error free, but I am not willing to say that for the English editions!

Kettenring: What were some of the statistical methods that you emphasized in your book?

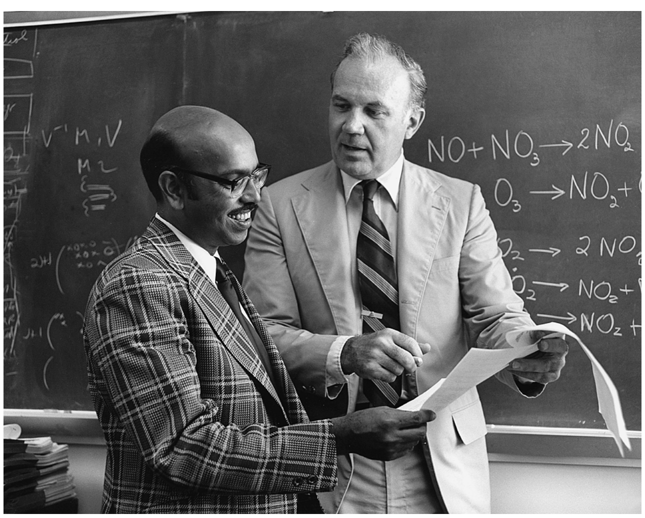

FIG. 3. *With Dr. Kenneth McAfee, Head of Physical Chemistry Research Department at Bell Labs, discussing collaborative work on air pollution in 1975.*

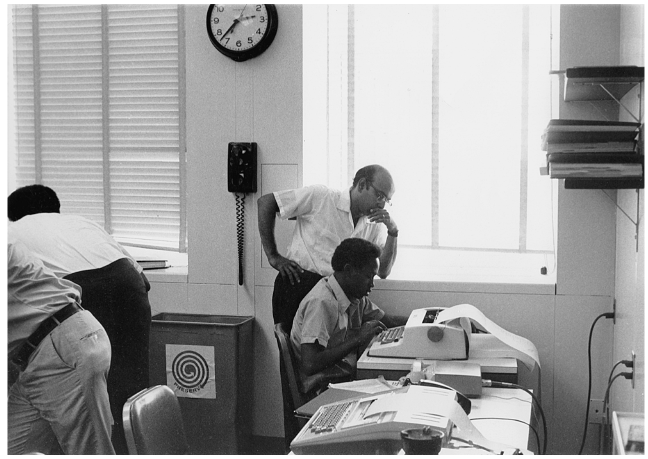

FIG. 4. *With a Summer Science School student at Bell Labs in 1970.*

Gnanadesikan: The book reflected various major trends associated with my research interest over the years. They fall into broad categories. The first category that comes to my mind is graphical methods. And by that I mean methods from displays that act as graphical summaries of data, to informal tools for analysis of the data, all the way through to aids in interpreting the information contained in the data. Graphical methods are a major thread through a lot of the methodologies that I have been involved in with others.

A second major thread would be robust estimation, residuals, and outlier detection. I think the emphasis on residuals and outlier detection is not just to get rid of the outliers so that they don't influence the estimator, but to think of robust estimation in the context of producing "robustified" residuals, if you want to call them that, which would be useful in detecting outliers. Identifying outliers is one of

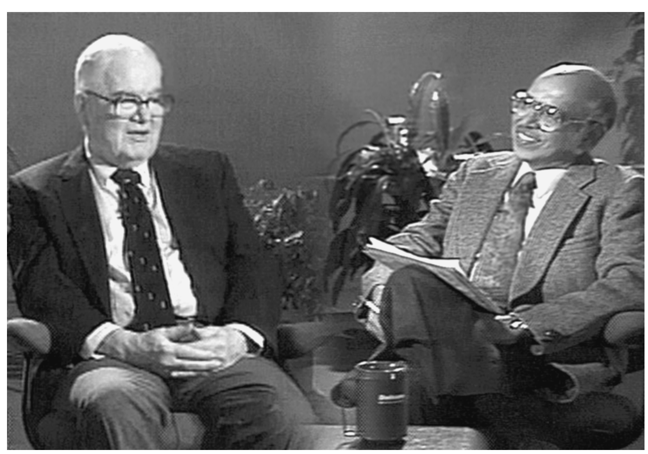

FIG. 5. *Interviewing John Tukey at Bellcore in 1993.*

the most interesting things to do in many scientific and technological problems.

As an aside, one other thing about my interest in residuals that I recall is that, in the late 60s, Wayne Larsen and Susan McLeary (now Susan Devlin) worked on something called "partial residuals." It turned out that their idea of partial residuals was fairly closely related to a method invented by Ezekiel. Historically, I think Ezekiel's was the first definition of partial residuals. But when Larsen and McLeary were working on it, I looked at the draft of their memo, and had a different idea, which they termed the "G" partial, as distinguished from "L" partials which was their idea. Also, Colin Mallows had yet another version, which they called the "M" partial residuals. The "G" partial residual is basically what was later called "adjusted residuals" by Mosteller and Tukey. They are the standard partial residual now. So that was an example of not only a redefinition of what you plot (which do you adjust for what?) but of what do you plot against what? The so-called G partial residuals idea was another example of the combination of a statistical summary with a graphical display that might be helpful in detecting such things as multicollinearity and the strength of the relationship with, or the additional information in, a variable in a regression context.

A third major area of statistical methodology that I emphasized in the book is classification and clustering.

ROOTS AND VALUES

Kettenring: Let's jump back to your early days in India. What influences led to your interest in statistics?

Gnanadesikan: I was born into an academic family and my father was a very admired professor of zoology who later on became a Vice Chancellor of a university. A Vice Chancellor is sort of like a university president here, except that the duties are not as heavily involved in fund raising. It's more of an academic administrative position. I grew up with a lot of academic values and academic people around. One of these was a man by the name of Professor Messiahdoss, who was a mathematician. He was interested in probability and statistics, although he was really an analyst. When I was in high school, he gave me a little book (unfortunately I do not remember the title), but it was by Tippett, and it talked about the use of statistics in a variety of situations, particularly industrial applications. And then I remember a big food shortage, it may even have been a near famine, in India. I read in the newspaper that somebody by the name of Mahalanobis had stated that if only statisticians had been available and had played an important role they could have done two things; first they could have forecast what might happen in terms of crop yields in the future, but second, they could have also designed experiments to improve the yields as they went along. I said to myself, "You know, this field sounds interesting to me if it has this much value." So that was my first inkling of interest in statistics.

However, I have to tell you that the early college training I had was in the physical sciences in a major way. Other than the standard English literature and local language literature, the other domains covered in my case were mathematics, physics and chemistry. All the bright students were going into engineering. But there was a new honors program in statistics, headed up by P. B. Patnaik, who was a student of Bartlett. There were only ten seats in the entire university for this, and I think the ten chosen came from the top 15 students from the previous stage statewide exam, taken at the end of the first two years of college. So, a very bright group of people, at least academically speaking, were turned on. We went into this field because it was new, although engineering was the glamour field at that time.

I think my interest in statistics peaked when I went to the Indian Statistical Institute after the honors program and saw the variety of things going on. It was not just the research in statistics but the sample surveys, the agricultural work, the design of experiments work. And there were many visitors such as David Finney. After all that exposure, I decided that I was going to go on into statistics for my graduate work in the United States.

Kettenring: What was the role of your family and others in shaping your personal values and philosophy?

Gnanadesikan: I described my academic family background already. Additionally, I can mention that I had a personal and direct experience with Mahatma Gandhi, which has had a profound effect on how I think about things and, in particular, people issues and societal issues. The rest of my life was affected a great deal by that. For instance, the kinds of things that went on in the south when I was a graduate student at Chapel Hill. This was in 1953, prior to the 1954 Supreme Court decision on desegregation of schools. It was a race-segregated system. I must tell you that when I arrived I was not prepared for that, to see things which were marked "colored" and "white" on water fountains, restrooms, waiting rooms, etc. I quickly became involved in issues of race relations. I remember that there were some who felt that as a foreign student I had no business getting involved in any of this. I am sure that the influence of the Gandhi approach to life was saying to me that these are human rights issues, these are universal problems, and you ought to be involved.

By the time I left Chapel Hill, I had indeed become involved. I was instrumental in founding the first International Student House and it came in very handy when the first two undergraduate African-Americans were admitted to UNC, because there was a question of where they were going to be welcomed and be put up. It was a very natural thing for them to come and live at the International House. This was a very small step, but nevertheless an important step. I was also directly involved in some of the sit-ins to desegregate lunch counters and things like that in and around Chapel Hill.

Later on, while working at Bell Labs, long before it became the practice of society to recognize that women were very valuable contributors if you were willing to take them on a part-time basis, I did so. Employing women on a reduced time basis was something that I felt was very natural to do. Many of these things, especially the last item, for example, and more important things like affirmative action in general, I feel are not only right from a moral point of view, which I might trace back to Gandhi's influence, but in fact pragmatically the correct thing to do because if you deny yourself access to these groups you are then actually denying yourself very skilled and talented people. That, I think, is the real reason why affirmative action actually caught on in a lot of industries, including places like the Bell System.

Kettenring: Can you mention some other examples of work that you did to help minorities and women fulfill their potential?

Gnanadesikan: In the late 60s, especially after the Martin Luther King assassination, there were a lot of cities that broke out into race riots. Plainfield, New Jersey, was one of them. Newark was another. One idea that occurred to me, along with like-minded people in Bell Labs such as David Slepian, the mathematician, and Denny Dudley, who was working in the staff side, was to see if Bell Labs could help address some of these problems or at least their root causes. I proposed that a summer science school might be a way of bringing in junior high school students primarily to catch them early enough to influence their career choices. Bring them in, let them see and work along with the other scientists at Bell Labs. I wrote a proposal for this and William O. Baker, the Vice President of Research in those days, was very supportive. And we launched a summer science program. The students stayed with scientists and their families while participating. The first time around maybe we didn't realize all the things that could go wrong, but we learned a lot from it. It was so successful that in modified and extended forms, including follow-ups, and expanded to include high school students, it was continued on at Bell Labs and was carried over with us to Bellcore. The last time I checked, which was somewhere in the mid 90s, they were still doing such programs in several tiers, depending on which year of school the students were in.

LEADERSHIP

Kettenring: Let's go back now to the 1984 period when you were deeply involved in the formation of Bellcore, now Telcordia Technologies. At this point in your career your responsibilities were expanding well beyond statistics into the larger arena of information sciences. Tell us about this period and your growing administrative responsibilities.

Gnanadesikan: The nice thing about the creation of Bellcore, the research part at any rate, was that those of us who went there were wanted both there and also back at Bell Labs. But Bellcore, because of its newness and the idea of building something from the ground up, excited my interest. Henry Pollak, who was my boss at Bell Labs and had been extremely supportive of statistics, was going to Bellcore. Also, prior to Henry's decision to go, I had been involved in a small committee appointed by Arno Penzias to plan what kind of a research group and effort should go on at Bellcore in the future as part of the responsibility of management at Bell Labs. So I perhaps had more information than one would normally have about what the possibilities could be, and that excited me. My first job there was as a Division Manager of Information Sciences, which included, in addition to statistics, the areas of discrete mathematics, economics, and human information processing research (mainly man/machine interactions). That breadth was a very natural extension of my interdisciplinary research and scientific interests.

After Henry Pollak retired, I became the Assistant Vice President in charge of what was initially called the Mathematical, Communications and Computer Sciences Research Lab involving larger, but still cohesive, areas. Later on the center was renamed the Information Sciences and Technologies Research Lab. But the activities, the scientific disciplines and the people were much the same. It was a wonderful group of very smart people working together, bringing their own viewpoints and strengths for solving many significant problems. And, as far as my own background in statistics, I found it extremely natural to not only be involved in indulging my scientific curiosity across statistics and many of these disciplines, but in administering such a lab. My statistics training was extremely valuable. There is a tremendous amount of overlap between statistics and communication sciences, statistics and computer science, and statistics and various areas of mathematics which of course has been classically recognized. Artificial intelligence, learning, coding, using coding to get efficient compression of data—a lot of these are really statistical ideas. And I learned a lot about communications, engineering and computer science. It was a lot of fun.

Kettenring: Your leadership skills have also been used extensively in a variety of leadership and service positions in the profession. Looking back, what were some of the highlights for you in playing these different leadership roles, and also why did you get so heavily involved?

Gnanadesikan: I was fortunate enough to be working for organizations that were willing to support my efforts and involvement in these societies. Well, there's always a dimension to a professional career involving both the professional aspect and the duties in whatever organization you're working for. So I thought of being involved in professional organizations not only as a source of places you went to present your papers or to interact with others about your research interests, but as the way to develop the future of the discipline and the people coming into the discipline. So it was a very natural thing to be involved in professional societies.

Kettenring: Are there some specific examples of things you did that stand out in your mind?

Gnanadesikan: A couple of things come to my mind from the time I was President of the Institute of Mathematical Statistics (IMS) that I'm very pleased about. I felt a very important thing to do would be to recognize people who were just starting their careers and I created an ad hoc committee that I, in my naivete, called the "Young Researchers Committee." The very first chairperson pointed out to me the ageism in that title and they preferred "New Researchers Committee." I said, "That's fine; I'm glad you corrected me." So the New Researchers Committee was formed, and it was composed entirely of people who were within five years after finishing their Ph.D. It was an ad hoc committee, but I am pleased to say that it has become a standing committee and, from all I can see of the IMS, it's a very active committee addressing needs and problems of a critically important group and bringing to the attention of the IMS membership as a whole what these needs are and what they would like to see the IMS address.

Another thing that came up at the time was an IMS report by Ingram Olkin and Jerry Sacks that focused on the value of cross-disciplinary stimuli to statistics. It recommended the formation of what is now known as the National Institute of Statistical Sciences. My interest at that point was bringing a cross-disciplinary emphasis into the IMS and, given my background, it seemed to be a very natural thing to do. I tried to play a role in getting the other statistical societies, the American Statistical Association (ASA) and the Biometric Society in particular, involved in thinking about the possibility.

I served in several positions with the ASA. For example, I served on and chaired the Publications Committee. Grappling with ideas of trying to get new journals on the one hand, but on the other not to pay the price of fragmentation, and trying to get a central journal like *JASA* to be responsive to the wider and ever increasing interests of the membership was something that very much interested me. I had interactions with various committees and editors thinking about how to do things in such a way as to not create fragmentation but make the existing journals reflect the breadth of the discipline.

I was on the ASA Board of Directors, and I should mention a couple of things about that. I was probably a lone voice in opposing the buying of a building for the ASA. The other thing that comes to my mind, a more positive thing, is that after the first meeting I attended, I remember I was very surprised at how "inefficient" the running of the meeting was, given what I was used to in the industrial setup. So I wrote a letter to Richard Savage, Ralph Bradley and Bill Kruskal, the triumvirate of the Past President, the President, and the President-Elect of ASA, expressing my concern about the inefficiency of the meetings. A committee was created to study the matter. John Neter chaired that committee and I served on it. We came up with the first set of guidelines, identifying such things as the importance of identifying what's an action item, what's an information item, etc., to mark these clearly ahead of time so that people don't end up talking about action items and then classifying them as information items or vice versa. It was an interesting exercise and probably of some continuing value as well.

I also served on several joint societal committees. One that stands out in my mind is the COPSS Award Committee, which I served on and later chaired. The satisfying aspect here was the recognition that the future of our discipline is in excellent hands, but the disappointment was that we had to choose only one person from among such an exciting group.

Kettenring: What about at the international level?

Gnanadesikan: I should mention the International Association for Statistical Computing (ISAC), which is a section of the International Statistical Institute (ISI). In the mid 70s Graham Wilkinson and Merv Muller talked to me about the possibility of creating a section of the ISI for statistical computing. One of the really successful things that we were able to do at Bell Labs and Bellcore was the integration of statistical computing into statistics, where the line between computing and statistics is not all that sharp and there are benefits flowing across that boundary in both directions. So it was natural for them to say to me, "What about ISI recognizing statistical computing as a natural part of statistics?" Maurice Kendall was extremely interested and valuable and agreed to be the first President of IASC. I had the pleasure in 1977 of chairing the first meeting of the IASC in New Delhi. When Maurice Kendall passed away soon after being elected President, Merv Muller who was serving as Vice President succeeded him. I then became the President-Elect for the next term. So the IASC was something I had a hand in founding.

I have become more involved in the ISI recently, at least officially. I am now serving my second consecutive term as a Vice-President of the ISI. I have always found the ISI—and I am beginning to appreciate it even more—to be unique in that it represents an international community with a whole range of backgrounds and interests and stages of development of statistics and its applications. You have developing nations and you have well-developed nations. In running programs we have to keep in mind constantly that we have to do things to satisfy the needs of such a wide spectrum of interests and backgrounds, never forgetting, for example, that what may not be a very timely seminar or workshop for a developed nation may be just the right one for technology transfer for developing nations. Technology transfer or knowledge transfer, tools to enable people to do their jobs more efficiently, running workshops, hands-on training, these kinds of things are very important in that context.

Kettenring: You were also involved in a number of advisory panels and committees at the national level, weren't you?

Gnanadesikan: Yes. One of the early ones that comes to mind is serving on the ASA's U.S. Census Advisory Committee in the mid and late 1960s while Ross Eckler and George Brown, in turn, were the Directors. Another example is helping in the formation of, and then serving two terms on, the National Academy of Sciences Committee on Applied and Theoretical Statistics (CATS). Two other slightly different examples are serving on the Mathematical Sciences Education Board (MSEB) and on the Advisory Committee of the Division of Mathematical Sciences of the NSF. All of these experiences were valuable opportunities to see the field of statistics in larger contexts of science, technology and societal needs and to bring statistics to bear on addressing these needs.

WORKING IN ACADEMIA

Kettenring: After you retired from industry, you spent five years at Rutgers. What courses did you teach there and how did you enjoy working with students?

Gnanadesikan: I really did enjoy that tremendously. Of course, I had always had some contact with universities. In fact, early on in my career I had the experience of formally teaching courses at places like the Courant Institute at New York University. So I've always had an interest in teaching and at least occasional opportunities to organize and present material. At Rutgers I was fortunate enough to be able to choose what I wanted to teach and I chose to teach an advanced level graduate course and an introductory level undergraduate course. The undergraduate course was a lot of fun in the sense that the students were interested and it was the very first statistics course.

Kettenring: That would be basically introductory statistics?

Gnanadesikan: Yes. I taught with a data analysis emphasis and used examples of data tailored to whatever the composition of the group happened to be. Given my wife's interests in statistics undergraduate education, she was involved very heavily in developing activity-oriented statistics and I was a very willing and enthusiastic guinea pig to try out her ideas. Students really enjoyed that a great deal. One negative aspect of that experience, incidentally, was that these courses tended to be very large (120–150 students was not uncommon). By the end of the term I probably could identify about 75% of those people by name and face, call them by their names and recognize them outside of classes, but I would have liked to have been able to do that for all of them.

At the graduate level I taught multivariate data analysis and both semesters of a two-semester course on interpretation of data. I found that very, very exciting. Less than half the students would be from the statistics department. The others were from all over the place, including from the medical school, education school, economics, engineering, environmental science, management science, etc. So that course was a lot of fun because these were mature students who had their own data sets and research that they were doing. They brought a variety of problems to the course. I gave them two assignments, both of which were data analysis projects. There was one student who actually did 90% of the data analysis with a hand-held calculator, but that's virtually impossible to do. You clearly needed to use the technology of statistical computing to do the exercises. So the course had value in bringing computing and statistics together as well. Those were things I very much enjoyed and my interactions with the students meant a lot to me.

PERSPECTIVE

Kettenring: Looking ahead, Ram, what do you see as the major challenges and opportunities for statistics?

Gnanadesikan: Statistics in my view is at the very center of the information age. After all, it's concerned with the collection, analysis and gaining of insights into data and interpreting what you find by way of information in the data. You can use information and statistics almost synonymously. I know there are parts of information that are not statistical but statistics is at the very least a very important core of the information age.

While there are ever increasing opportunities for statistics, I have a feeling that sometimes statisticians feel insecure with respect to mathematicians or computer scientists. We are the data science people, so we need not feel that way. And, at the same time, we need some humility and an interest in learning about the real problems on the other side of the fence. Don't sit on the fence, jump. Jump to both sides of the fence and do it early enough, commit yourself and find benefit by not only solving substantive problems but also by feeding back into the development of new statistical approaches. There are going to be problems we are not even aware of right now.

As mentioned, large data sets were already an issue in the early 60s. And that challenge is still around. But let me mention the other extreme: I think there are going to be more and more problems where there is sparse data. Gone are the days when statisticians can just say, "You don't have a large enough sample; go get a larger one before I talk to you." We need to think very carefully about problems of sparse data. The Bayesian approach may be suggested as a way to handle that, but I'd like to think that there are and will be other ways of addressing these problems. We need to think hard about such problems and the continuing overlap between statistics and other disciplines. We've now gone through cycles of interactions with mathematics and computing science and pattern recognition kinds of problems.

The information era is going to create new opportunities. I think there's a vast area of molecular biology that is going to open up not only the frontiers of medicine and the health sciences but also expose very interesting statistical problems. So I would suggest that statisticians get in there, be open-minded, have some humility, have real curiosity and work like the devil to get it going. Statistics will have a great future, both as a discipline and as a relevant cross-disciplinary science.

Kettenring: What would you say to a student who might be considering a career in statistics?

Gnanadesikan: It's a very exciting area. It would be preferable to have a background in some other disciplines as well. It doesn't have to be the discipline you would end up working in later on, but study things like science, different kinds of areas of science and engineering. Learn about science and technology, think about the problems around you in the world, and relate statistics to those. There are going to be large numbers of not only intellectual opportunities but career opportunities as well. Since statistics is a very central part of the information age, you can have a bright future in it, provided you have these other curiosities and interests.

Kettenring: Ram, you didn't say anything about the possibilities of finding a spouse through statistics.

Gnanadesikan: Well, I should say, "Go to professional meetings." That's where I met my wife, Mrudulla. We met at a statistics meeting and we are now coming up on the 35th anniversary of our wedding.

Kettenring: Apart from the people we've already discussed, were there others who had a major impact on your professional development?

Gnanadesikan: That, I guess, is one of the most fortunate things in my life. I should mention some of the ways in which I got to meet other people who have had major influences. When I joined Bell Labs, there was an activity that was called the "Statistical Summer Seminars." As I recall the history of it, it was something that Fred Mosteller and John Tukey had started with funding from the Office of Naval Research. The idea was to bring together a small group of people with a theme for a week of discussions. And they just interacted with each other and talked about things with this over-all umbrella theme guiding them. People would make presentations and then there would be a lot of discussion. When I joined Bell Labs, Milton Terry was basically in charge of Statistical Summer Seminars and he recruited me to serve as the secretary-treasurer of that organization. It fell to me then to actually choose the topics, find the people to come to it, etc., and that was a lot of fun. We used to meet very much in the same spirit as the Gordon Research Conference except that this was a smaller group. We would meet typically in the New England area in the summer for about a week. We held sessions in the morning and the evening with afternoons left free for discussions and interactions. It was self-supporting. People who came from industry paid enough of the expenses to cover other people who didn't have the funds. As an aside, money was left over and, if I'm not mistaken, it provided the initial funding and formation of the endowment for what is now known as the COPSS Award given annually to an outstanding statistician under the age of 40 years. That was a very good use of that money.

One meeting that I remember took place in 1963 in Lexington, Massachusetts. The topic was "The Relevance of Systems of Statistical Inference to Science and Technology." Some of us presented what we did by way of data analysis, but there were others who represented the various approaches to statistical inference. That was the time that I really got to know Oscar Kempthorne, who was his usual self in challenging people to think clearly. Martin Wilk was there. Jimmy Savage and Howard Raiffa were largely representing the Bayesian approach to things. David Cox, Herman Chernoff, Lincoln Moses, Charlie Stein and Marvin Zelen were also there. It was a fairly small but amazing group of people to get together to talk about an exciting topic: how relevant or irrelevant is any system of statistical inference to problems of science and technology? The majority opinion that emerged was that by and large perhaps none of the systems of inference are going to push science and technology even one step forward in their realms of discovery and invention. It has always been interesting to me that the people who represented decision theory, people who represented Bayesian analysis, people who represented the Fisherian approach, people that represented the Neyman–Pearson approach, came together. All the different inferences systems were represented plus a whole bunch of people who had experience in analyzing data from problems that were at the frontiers of science and technology. It was the recognition by these people who were often the founders of the different systems of inference or at least strong proponents of them, saying, "Oh gee, my system of inference is not going to add very much to solving this tough problem of science or technology." It always struck me that it was another example of where "disciples" often tend to be more intolerant and sure that they have the universal solution to everything than original "prophets" do. Anyway, those are some of the names of the people whom I got to know there and subsequently I continued to enjoy interactions with many of them.

At Bell Labs we had a lot of short-term visitors. They would come for periods of one to six months. We had visitors such as Egon Pearson, David Cox, Frank Yates, John Nelder, George Barnard, Ricardo Maronna, and so on. These are all great minds of statistics, at least in my time, and interacting with them, seeing how they thought, how they worked, had major impacts on me. And last, but not least, I should mention the really important role in my growth that has occurred because of the vast array of collaborators that I have been very fortunate to have in my career. In the front of my Wiley book, I have a dedication, which is taken from the Sanskrit Upanishads, and it reveals how much I appreciate and value the impact that my collaborators have had on me.

Kettenring: We have covered a lot of ground already. That's really your fault because you've covered a lot of ground over the years. Do you have any regrets?

Gnanadesikan: I really have had a marvelous time in statistics, and I'm glad I lucked into this discipline. I wouldn't change a thing.

Kettenring: Ram, before we end this conversation, there is one thing I think we should clear up. Most of your statistical friends have known you during your days in India, North Carolina, or New Jersey, and here we are today sitting in the bright sunshine of Tucson, Arizona. What are we doing here, and where are we going to find you in the future?

Gnanadesikan: Last year Mrudulla and I decided to retire from our jobs. It was a young age actually for her retirement. We decided to move to Tucson and split our time six months here and six months at our home in Martha's Vineyard. And you can guess which months we're going to be here and which months we're going to be in Martha's Vineyard!

Kettenring: Thanks for that tip, Ram, and thank you for sharing your thoughts with *Statistical Science*.

Gnanadesikan: Thank you, Jon.

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