Statistical Science
2002, Vol. 17, No. 4, 458–466

A Conversation with V. P. Godambe

Mary E. Thompson

Abstract. Vidyadhar Prabhakar Godambe was born on June 1, 1926, in Pune, India. He received the M.Sc. degree in Statistics from Bombay University in 1950 and the Ph.D. from the University of London in 1958. Between periods of study, from 1951 to 1955, he was a Research Officer in the Bureau of Economics and Statistics of the Government of Bombay. Following a year as Visiting Lecturer at the University of California, Berkeley (1957–1958), and a year as Senior Research Fellow at the Indian Statistical Institute in Calcutta (1958–1959), he became Professor and Head of the Statistics Department at Science College in Nagpur. He was promoted to the position of Professor and Head of the Statistics Department in the Institute of Science, Bombay University, in 1962. In 1964, he left India for North America, becoming for one year a Research Statistician at the Dominion Bureau of Statistics in Ottawa. After subsequent Visiting Professorships at Johns Hopkins University and the University of Michigan, he joined the University of Waterloo Department of Statistics in 1967 and has been at Waterloo ever since.

Professor Godambe is a Fellow of the American Statistical Association, a Fellow of the Institute of Mathematical Statistics, a Member of the International Statistical Institute, and an Honorary Fellow of the International Indian Statistical Association. He is the recipient of the 1987 Gold Medal of the Statistical Society of Canada and is an Honorary Member of that society. Upon his retirement in 1991 he was awarded the title of Distinguished Professor Emeritus at the University of Waterloo.

The following conversation took place in Waterloo in August 2000, mainly by correspondence. Some of the responses are taken from "Briefly about myself," an autobiographical piece written by Professor Godambe in 1998–2000.

Thompson: What do you remember about your childhood in Pune? Were you seen to be gifted at a very early age?

Godambe: I was a very sickly child. So my parents put me in the elementary school which was closest to our residence. Any other school would have meant more walking to school and back. The school was not great, but the tuition fees also were less than for many other schools. I spent five years (aged 5 to 10) there to receive elementary education in arithmetic, language (Marathi), geography, history and so on. I do not have pleasant memories from this school or the second school that I attended (which was better class). Because of my ill health, and even otherwise, I could not easily mix with other students. The second school had better quality of instruction than the first, yet until age 13 my learning in school was routine. I did not much like going to school and preferred to stay at home and think of things myself, taking now and then a sip of tea. During this period, the only "gift" some people around could see in me was that of rather unusual intellectual concentration.

Thompson: What kinds of things did you read as a young person?

Godambe: Looking back, I think in my childhood I was more preoccupied with cosmological–philosophical questions than most other children. But

Mary E. Thompson is Professor, Department of Statistics and Actuarial Science, University of Waterloo, Waterloo, Ontario, Canada N2L 3G1 (e-mail: methomps@uwaterloo.ca).

I do not remember having read any specific philosophical works until the age of about 14. At that time I read a work on Hindu philosophy. In a competition based on the work, I won a prize at our school. From high school to college years (ages 14 to 21), though my official record (except for the last year) was nowhere near the top, I was a much better read student outside the school curriculum than most. I read about relativity, quantum physics, communism, socialism and other topics.

In the last year, matriculation year, we had a teacher, Dr. P. G. Sahasrabudhe. He taught us Marathi. He was a most effective teacher and a scholar. Even today I think he was far more clear headed than most Marathi scholars of the time. He organized a study group of selected students. I was a member. We used to meet once a week at his home to discuss problems of then current interest primarily from a historical point of view. This went on for quite a few years. The atmosphere in the study circle surely must have stimulated me intellectually. I often disagreed with our teacher, saying that his arguments were not compelling enough logically or otherwise. Yet his style of forcefully and clearly putting forth his convictions, I think, had an influence on me.

During this period I was immensely influenced by the writings of Bertrand Russell. I read many of his books.

Thompson: What was it that attracted you in the writings of Bertrand Russell?

Godambe: The discussions in our study group must surely have raised many questions in my mind. Also at that time I spent considerable time studying the literature on the epistemology underlying the important developments in the sciences—relativity, quantum mechanics, evolution and the like. I continued to read Hindu philosophy. With this background, though it sounds absurd today, I was then at least subconsciously searching for the roots of human knowledge! In this subconscious search Russell's writings influenced me most. With his lucid style, he explained and answered even deep philosophical questions, without introducing any technical jargon. Now over the years my views on the subject have changed. Today I do not always find Russell's answers as compelling as I found them then, but that is beside the point. What is important is that in those school–college days his writings provided a strong intellectual stimulus.

Thompson: How did you approach the study of mathematics? When you were taught, were you permitted to question the foundations?

Godambe: In school and college we were required to solve tough mathematical problems. But questions about foundations of mathematics were never raised. I started thinking about them seriously, on my own, after my first degree. Soon after, I ran into foundational topological questions concerning complex variables. My teachers could not answer or even appreciate my questions. Fortunately, I happened to meet Dr. P. Masani, who then had just returned from Harvard. It brought me a great relief when I heard from him that my foundational questions were quite legitimate, and some of them had already been solved, while others were being investigated.

Thompson: How did you decide to enter the field of statistics?

Godambe: I clearly remember. Professor Mahalanobis, with his influence on Nehru (who was then prime minister), persuaded the Government of India of the necessity of conducting sample surveys on a regular basis; these would provide socioeconomic information necessary for the country's five-year plans. The plan to collect these data would create job opportunities for people with statistics qualifications. Although I had a bachelor's degree in Mathematics and Physics, the mathematics qualifications provided relatively much fewer job opportunities. To combine good job opportunities with my interest in mathematics, I chose statistics for my master's degree. In this choice, I was not altogether wrong. A few months after receiving my master's degree, I landed a comfortable job in the Bureau of Economics and Statistics, Government of Bombay. I was given double the salary than the one I asked for! This seldom happens anywhere, all the more seldom in governments in India. Further, there was no "routine work" expected of me. From then on started my career in statistical research.

Thompson: Who were your early mentors in research? When did you become aware that research and writing academic papers could be a way of life?

Godambe: No mentors that I can think of. In the Bureau of Economics and Statistics in Bombay, I could see at first hand the reality of random sampling, hence started investigating the associated problems.

I was very much encouraged by the Director of the Bureau, Mr. Sankpal, to pursue my research interests. My research interest soon concentrated on problems of survey sampling, the central concern of the Bureau. In a year's time of employment, in 1951 I published a small research paper in the *Journal of the Royal Statistical Society, Series B* (*JRSS-B*). Mr. Sankpal appreciated it most. Soon, however, Sankpal left the

Bureau to take up a U.N. assignment. I still continued with the Bureau for some time until I submitted for publication in *JRSS-B* my 1955 paper entitled "A unified theory of sampling from finite populations" (Godambe, 1955).

Thompson: Why did you choose to send your first papers to *JRSS-B*?

Godambe: Before going to England I wrote six papers, three of which were theoretical; the remaining three were kind of technical papers. These latter three were published in the *Bulletin of the Bureau of Economics and Statistics, Bombay* in 1953 and 1954. They were quite appreciated in the statistical circles of Bombay.

I think M. G. Kendall was instrumental in getting *JRSS-B* to publish the 1951 and 1955 papers—I acknowledged him in the 1955 paper. The remaining paper was sent for publication in *The Annals of Mathematical Statistics*. It was rejected, with the referee's report saying that he found the English hard to follow and that he thought the results would be primarily of interest to the Indian audience and should be published in India. The paper remained unpublished.

Thompson: Why did you begin to work on the theory that eventually led to estimating functions?

Godambe: When the conventional theory of unbiased minimum variance estimation was introduced to me in 1948, my immediate reaction was that "modal unbiasedness," rather than "mean unbiasedness," was a desirable property for an estimate. And from among all the modally unbiased estimates, one should choose the estimate whose distribution has maximum probability at the mode for all the parametric values. If such a "modally best" estimate existed, its choice would implement a deeply intuitive principle (which could be called a "modal principle") that we tend to act or infer assuming that the event under observation has greater probability of occurrence than any other event that could occur. Several years later (in 1958; most of the intervening years I did survey sampling), I proved under some conditions that if such a modally best estimate existed, it would be given by the maximum likelihood estimate; however, the maximum likelihood estimate was not modally best! Yet all this endeavor was not a waste. It suggested a generalization. An event referred to in the modal principle need not be an estimate (a function of observations x); it could be a function of the observations x and the parameter θ, that is, an estimating function. If M_g is the mode of the estimating function $g(x,\theta)$ the estimate is obtained by solving the estimating equation $g(x,\theta) = M_g$ for θ, given x. Of course the mode M_g could be absorbed into the estimating function g, calling $g - M_g$ a function g_1, so that the estimating equation is $g_1 = 0$. Struggling further along these lines, replacing mode by the more manageable expectation, in 1960 I arrived at the criterion of optimality for estimating functions (Godambe, 1960). Accordingly, the score function was the optimal estimating function in a parametric model! This criterion of optimality is now in common use.

IMPERIAL COLLEGE AND BERKELEY

Thompson: In 1955, you left India and went to England. Why did you decide to go abroad and what attracted you to England?

Godambe: Why did I want to go abroad? By 1955, Mr. Sankpal, who had hired me and encouraged my research, had left India for a U.N. assignment and subsequently the atmosphere in the Bureau changed. I decided to go abroad for further studies.

It was less expensive for me to go to England than to the U.S. The U.S. universities would not offer me any teaching assistantship because I was not working in a university in India. Also, somebody in Bombay knew George Barnard, and he gave a reference.

Thompson: So Barnard agreed to be your official supervisor. How did that arrangement work out?

Godambe: When I went to Barnard, in our first meetings he could note my preoccupation with survey sampling problems. This was natural, because I had already two papers on the topic. So he suggested that instead of my working with him, I could get a degree far sooner if I could work with somebody in the field of survey sampling. My immediate reply to Barnard was that I would keep aside the problems of survey sampling for the time being and work on general inference, which was Barnard's specialization. He agreed. I had many stimulating discussions with Barnard, primarily on statistical inference, but also on other topics of common interest such as politics and philosophy. The discussions with him, whatever the subject, were so lively that even today, after so many years, I remember them. So the arrangement of working with Barnard went well. I do not think I could have had a better supervisor.

Thompson: Why did you then go to Berkeley for a year?

Godambe: While working in Imperial College for my Ph.D., I was short of funds. Also, at that time Barnard was going to be out of England for a while. During this time I thought of getting a job in some

U.S. university to earn some money. I applied to a few universities and had offers from some of them. The reason I went to Berkeley was probably that, during Neyman's visit to Imperial College, both Barnard and Birnbaum (who also then was visiting there) recommended me positively for a position at Berkeley. Indeed the work load at Berkeley was relatively light compared to other places, and the salary also was all right.

There was also another side to the story. While at Imperial College I thought I did not have enough freedom to pursue my own ideas. This was natural. I was older and more mature than most other students who did not mind "restrictions" imposed by a degree program. Actually, I was thinking of giving up working for a degree altogether. But Barnard was considerate and let me leave the country for some time without foregoing the degree.

In fact, I wrote my Ph.D. thesis while I was at Berkeley. Somehow the environment there was conducive to writing. The thesis was submitted to Imperial College for a degree while I was stationed in London on my way back from Berkeley to Bombay.

During my stay at Berkeley, I had a brief correspondence with Carnap, who was on the philosophy faculty of UCLA. I had referred to Carnap in my 1955 *JRSS-B* paper for his two kinds of probabilities, one frequency, the other logical. When he received my reprint, he wrote back saying he never thought that statisticians were so logical minded! But his two-page letter I found rather confusedly worded. This surprised me, for in his works he emphasized clarity of language (as did other logical positivists) to the extent of distinguishing the "meaning of" and the "symbol for" a period. But of course a letter is different from a formal communication.

Thompson: Perhaps, too, clarity cannot exist without confusion!

Godambe: I enjoyed my year's stay in Berkeley. But there I missed listening to, or participating in, the discussions of basic controversies (e.g., Neyman–Pearson vs. Fisherian or Bayes theory) that I had been used to in London. I believe these controversies have played a positive role in the development of statistics. To them, for instance, we owe the development of conditioning, a central idea in modern statistics.

Thompson: You met Allan Birnbaum at Imperial College in 1957 or thereabouts. Was he already talking about the principles of inference at that point? Was conditioning a topic of conversation?

Godambe: When I met Birnbaum, he was like other American statisticians. He emphasized mathematical rigor and clarity. He liked a paper on the two-sample problem that I had just then written, and made many useful and encouraging comments. (He thought my paper could possibly bring some unification to the literature on the subject.) But during this brief period, I do not remember Birnbaum telling me about conditioning. Either his great result on conditioning yet was a future event, or alternatively he thought I might not be interested in the topic.

A NEW APPROACH TO SAMPLING FINITE POPULATIONS

Thompson: After Berkeley you went back to India, first to the Indian Statistical Institute (ISI) at Calcutta. Did you find people there with whom you could talk?

Godambe: Except for occasional discussions with C. R. Rao, there were not many interactions directly related to my interests. However, ISI at that time was one of the most exciting centers of statistics. Statisticians and other scientists from all over the world visited the Institute for brief periods of time. This contributed considerably to the intellectual atmosphere there. Also at the time the famous British geneticist, Professor J. B. S. Haldane, had taken Indian citizenship

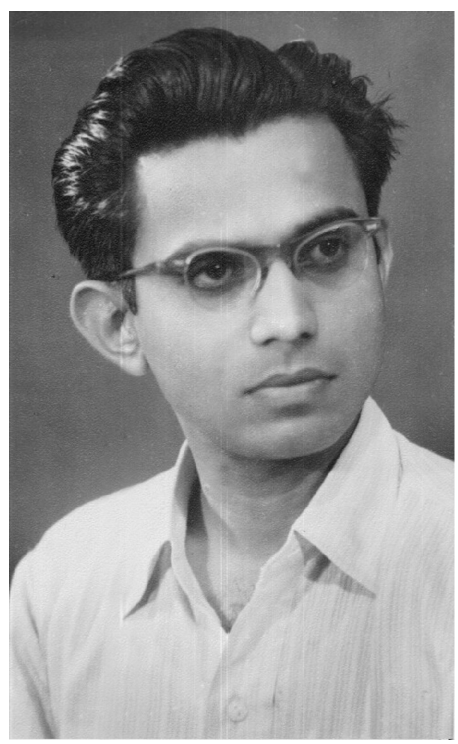

FIG. 1. *In Pune, around 1950.*

FIG. 2. *In Nagpur, around 1960.*

and had a regular position at the Institute. Haldane made a permanent impression on me by his courage of conviction and downright honesty. He adopted Indian citizenship because of his love and respect for the country. Yet he did not hesitate to criticize, sharply and loudly, the Indian bureaucracy which he felt was stifling Indian science. I was privileged to have a friendly relationship with Haldane and still remember a few conversations I had with the great man.

Thompson: After four more years as an academic in India, in Nagpur and Bombay, you came back to North America, to the Dominion Bureau of Statistics, now Statistics Canada. Had they advertised for statisticians?

Godambe: Now, I do not remember many details here. Nathan Keyfitz from the Dominion Bureau of Statistics was visiting the Indian Statistical Institute the same year when I was there. Possibly I spoke to him—that I would like to visit Canada sometime. After he went back I received from him (or from the Dominion Bureau) a card to be filled in. This possibly could have been my biodata card. Then, for a couple of years or so, nothing happened. I almost forgot about it. My memories were refreshed when I saw an interesting paper in the *Journal of the American Statistical Association*. The author was Ivan Fellegi and the affiliation was the Dominion Bureau of Statistics! I wrote him for a reprint, and possibly also enquired about the card I had filled in a long time before. He apologized for the delay and sent me an offer.

Several years later, when I visited Nathan Keyfitz in Chicago, he treated me at his home with a grand party. We still now and then exchange holiday greetings.

While at the Bureau I was again encouraged to work on my research in survey sampling, and the most important thing I did in the year I spent there was to write my back-to-back 1966 *JRSS* papers, "A new approach to sampling finite populations, I and II" (which I tend to think of as one paper) (Godambe, 1966).

Thompson: How do you account for the immediate impact of that paper?

Godambe: Let us see the background. In 1962, Birnbaum established his famous results relating the likelihood and conditionality principles. For several years, Barnard had been proposing (in a sense) a radical use of the likelihood function in statistical inference. Neither Barnard nor Birnbaum was familiar with survey sampling, and hence their theories were based on the assumption of a hypothetical population model, in common use in statistics. How to interpret the Barnard–Birnbaum basic results concerning likelihood and conditionality concepts for actual survey populations, which unlike hypothetical populations consist of labelled individuals? This question for the first time was formulated and analyzed in my 1966 *JRSS-B* paper. The main result was that the likelihood function was independent of the sampling design! Yet most practitioners believed that their estimates or inferences were essentially based on the sampling design they had employed to draw the sample. This conflict explains the impact of the paper.

In fact, I think the almost immediate recognition of the 1966 paper made statisticians revisit my 1955 *JRSS-B* paper, which had not until then received much attention. Hence came about the 1968 Chapel Hill Symposium on New Developments in Survey Sampling.

The Chapel Hill conference gave rise to a heated controversy, another of the controversies about the fundamentals of statistics that were current around that time. The issue at stake was: Do survey population problems need, for their analysis, a model different from that of the traditional conceptual population model used in theoretical statistics? Of course I was on the side giving a definite affirmative answer to the question.

WATERLOO

Thompson: You held visiting positions at Johns Hopkins and Michigan, inspiring researchers such as Richard Royall and Bill Ericson to write about survey population problems, before coming to Waterloo in

1967, just as the Department of Statistics was being formed. What were your first impressions of Waterloo?

Godambe: Yes, due to my lectures and my emphasis on the likelihood function in survey sampling (then just established, in my 1966 *JRSS-B* paper), Royall at Johns Hopkins and Ericson at Michigan went on to develop their own theories independently, model based and Bayesian. Watson also, briefly, had some interest in survey sampling; and it was he who invited me to come to Johns Hopkins, because of my 1955 *JRSS-B* paper, which he felt had gotten to the heart of sampling theory. (See Beran and Fisher, 1998.) But apart from these people, neither at Johns Hopkins nor at Michigan was there much enthusiasm about survey sampling. Later on, I found out that this lack of enthusiasm about survey sampling was common to most statistics departments on the American continent.

Waterloo was no exception when I came here in 1967. But Waterloo was different from many universities on the North American continent. Here in the Statistics Department was a small group of young competent people who were working enthusiastically on the basic theory of statistics: confidence intervals, fiducial inference, likelihood, ancillarity and so on. For me, this was encouraging.

I have some heartwarming memories of my first day in Waterloo. Coming from the Toronto airport in the limo, somebody on the University faculty, after a very brief introduction, invited me to stay at his home for a couple of nights! Anyway, foregoing this generous offer, I went to stay in the Waterloo Hotel (King and Erb). When I arrived there, it was about eleven at night. It being Sunday, everything was closed. I was thirsty for a beer. The hotel attendant told me that at that time nowhere in the town could I get an alcoholic drink. O.K. But about ten minutes later, there was a knock on my door. The attendant had brought in his hip pocket a bottle of beer for me! This hospitality of the town I experienced on many occasions afterwards.

Thompson: When I first came to Waterloo in 1969, the Chapel Hill Symposium was a recent memory, and you were organizing the Waterloo Symposium on the Foundations of Statistical Inference. It was a great deal of work and a very exciting occasion. Was it worth all the effort?

Godambe: Yes. Besides myself, David Sprott and Jim and Jack Kalbfleisch had attended the Chapel Hill conference. So it was often talked about in our department. This conference had been a special success for me. The topic of the conference was primarily based on my work; the names of most participants were suggested by me. The local people at Chapel Hill generously obtained the funds and all the organizational machinery. I went there as a guest participant!

The conference put into my head the idea of having a conference on a broader topic than the one held at Chapel Hill. The topic of the new conference would be Foundations of Statistical Inference. The proposal was to hold it at Waterloo. David Sprott, the founder Chairman of the then just established Statistics Department, fully supported my idea and found the necessary funds. Because the statistics group here had natural inclinations to statistical inference, they also fully supported the idea. Hence ultimately the conference came about in April 1970. It lasted for about a week. More than forty experts from all over the world enthusiastically participated: from Canada, Fraser and all University of Waterloo faculty; from England, Barnard, Bartlett, Cox, Lindley; from India, Basu, Rao; from the U.S.A., Bross, Geisser, Good, Kempthorne, Neyman, Zelen; and many others. Practically all the foundational topics of statistical inference were discussed. Each theory had supporters as well as opponents. All views were forcefully put forward. One I particularly remember was by Bross, that the subject "foundations of statistical inference" did not exist. This paper, naturally, was put at the end of the conference.

Taking into account the quality and quantity of discussions generated at different sessions, I think the conference was a great success. But the efforts put in were also tremendous. The publication in 1971 of the proceedings of the conference received very exciting reviews. It hardly would be an exaggeration to say that no other conference, since the one held at Waterloo, has discussed foundations of inference with comparable thoroughness.

Thompson: At Waterloo, you carried on with your work on foundations.

Godambe: For about the first five years at Waterloo, I worked exclusively on survey sampling. In 1970 I published a paper, "Foundations of survey-sampling," in *The American Statistician* (Godambe, 1970). This article was an attempt to provide background and an interpretation of the controversy at the Chapel Hill conference. There was a rebuttal by H. O. Hartley and J. N. K. Rao, and there ensued a rather heated debate. This debate calmed down with my publication of "A reply to my critics" in 1975 (Godambe, 1975) and a note in *Sankhyā, Series A*, by C. R. Rao in 1977 supporting me in the controversy. The controversy, apart from producing heat, also brought about enlightenment. Many statisticians, specialists of survey sampling and others, started having a fresh look at the subject. In fact there was a movement to try to reconcile the various approaches to sampling inference. In 1971, we read a paper before the Royal Statistical Society called "Bayes, fiducial and frequency aspects of inference in survey-sampling." A book entitled *Foundations of Inference in Survey Sampling* was published in 1977 by Cassel, Särndal and Wretman in the wake of the controversy. Our paper, "Robust near optimal estimation in survey practice," presented at the Delhi ISI meeting in 1977, showed that much of survey practice could be explained and justified in the unified theory framework I was proposing.

After 1975, although I still occasionally worked and published on survey sampling, most of my researches concentrated on estimating functions.

FIG. 3. *Receiving the Statistical Society of Canada Gold Medal from Statistical Society of Canada President Martin Wilk, 1987, Université Laval.*

ESTIMATING FUNCTIONS

Thompson: I remember that your interest in estimating functions was revived by a lecture of Barnard.

Godambe: Yes, I think of 1974 as having special significance in the development of estimating function theory. A little earlier, Barnard had given a series of lectures on statistical inference at Waterloo. In one lecture he briefly mentioned estimating functions and the associated problems of nuisance parameters. Stimulated by the lecture, we established (Godambe and Thompson, 1974), using the criterion of optimality that I had published in *The Annals of Mathematical Statistics* (Godambe, 1960), optimum estimating functions for a general nuisance parameter case. As an illustration it was shown that, for the usual normal distribution with unknown mean (the nuisance parameter), the optimal estimating function for the variance was obtained by replacing n (the sample size) in the maximum likelihood estimator by $n-1$. The methodology ultimately led me in 1976 to a satisfactory resolution of the well-known Neyman–Scott problem (Godambe, 1976). In our series of joint papers on the topic, the one I particularly would like to mention is our 1986 paper where estimating function theory is brought to bear on survey sampling problems (Godambe and Thompson, 1986).

RECENT RESEARCH

Thompson: On what kinds of problems have you been working more recently?

Godambe: For the last ten years or so, I have been occupied with biostatistical problems. Here, we have indeed a wide variety of literature on such topics as observational studies, randomized experiments, causation and so on, and response dependent sampling is quite common. It is axiomatic in some observational studies, whereas it can be operational (or actual) in some planned randomized studies. I believe it is important here to try to distinguish the response from the individual who responded. This may not always be possible, but when it can be done, one can build a framework

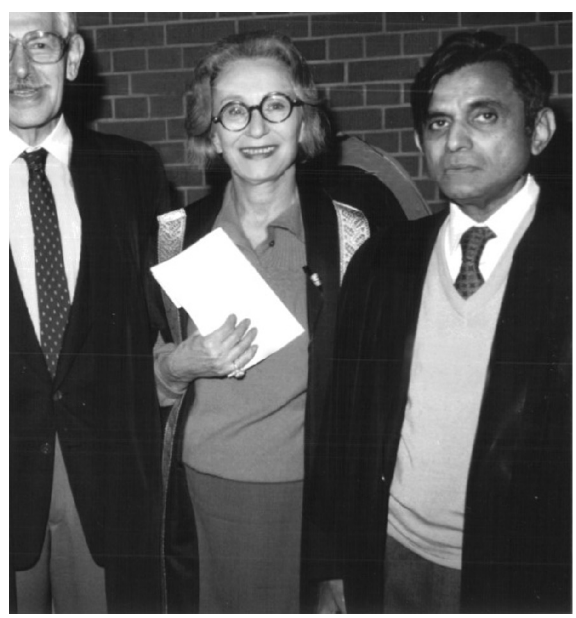

FIG. 4. *With University of Waterloo Chancellor Sylvia Ostry, 1991.*

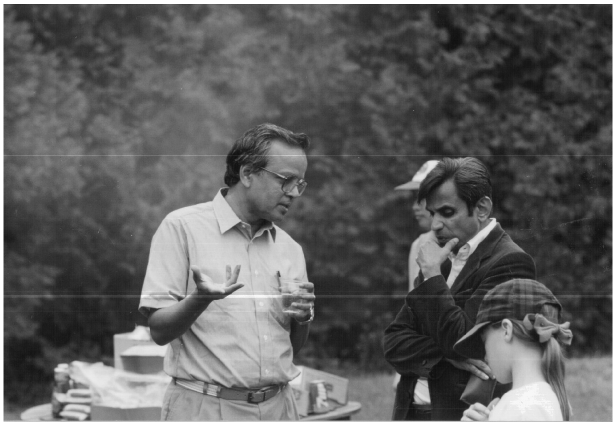

FIG. 5. *With Professor Jon Rao of Carleton University, Statistics and Actuarial Science picnic, 1994.*

of a survey population from which the present sample has been actually or axiomatically drawn. Now all the technology of survey sampling can be brought to bear to study estimation of two levels of parameters, one of the survey population and the other of the hypothetical population. Both have practical interpretations, particularly when investigating association (Godambe and Thompson, 1997).

Because the topic of survey sampling is generally neglected in our universities, it is not surprising that this perspective is generally ignored in the biostatistical literature.

Thompson: What motivates you to do research? What role is played by the desire to be first with a discovery?

Godambe: Since I got into research activity (I don't know how and exactly when), I have always wanted to do conceptual and fundamental work. I also spent considerable time on technical details, but generally only when they were required for practical verification of some fundamental theory. All this made me strongly inclined for conceptual clarity. In this respect the statistical literature in general, and survey sampling in particular, lacks considerably compared to some older disciplines—such as, for instance, physics.

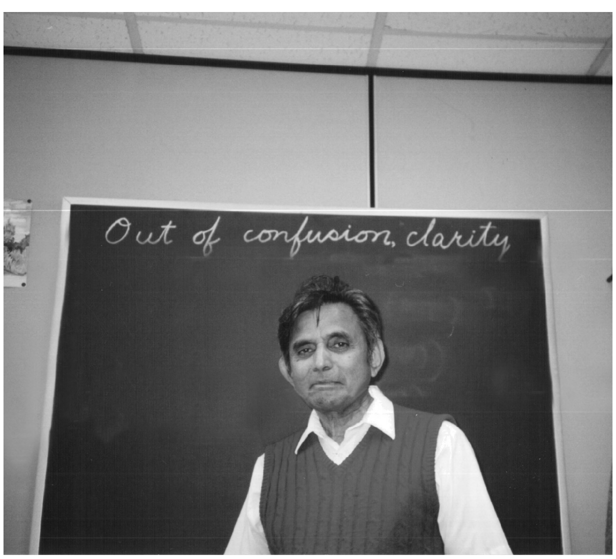

FIG. 6. *At the University of Waterloo, 2001.*

Let me give you an example: my very first paper "On two stage sampling" (Godambe, 1951) was written to clarify the following confusion prevalent in the literature. A survey population consists of N units i, $i = 1, \dots, N$. Further, each unit i is divided into a number N_i of subunits. The sampling procedure consists of two stages. First a random sample, size m, of units is selected. Then from each selected unit i a random sample, size n_i, of subunits is selected. It was generally agreed that n_i should be proportional to N_i. But should the constant of proportionality depend only on the sizes N_i of the selected units, or on the sizes of all units in the population? In the latter situation, which is generally the practice, the total sample size ($\sum n_i$ over selected units) becomes a random variable. To hold the total sample size fixed, the constant of proportionality has to be adjusted properly. My first publication clarified this point. So you see it was not so much a desire to be first as it was a desire to clarify confusion that was the basic motivation of my research. This becomes all the more apparent from my subsequent publications.

Thompson: You have stimulated many others to do research on specific problems. How have you managed to do this?

Godambe: I do not think I have consciously made many efforts to stimulate anybody to work on any problems. True, I like to discuss my ideas with colleagues. But that is primarily to clarify the ideas to myself or to see if I have committed any blunders. So, if people worked on some ideas like (for instance) estimating functions, it must be due to the fact that they liked the idea and found it useful in their own work. The credit goes entirely to them.

GODAMBE'S PARADOX

Thompson: Do you think there will ever be agreement about Godambe's paradox?

Godambe: The origin of the paradox is deeply rooted in the statistical practice of randomization with special reference to survey sampling. A primary purpose of randomization here is to eliminate nuisance parameters from the assumed superpopulation model.

In other words randomization provides justification for use of some simple superpopulation model (i.e., one with no nuisance parameters) in practice. Up to this point, I suppose, there is considerable agreement. What people reject or find difficult to accept is the following statement: "The assumption that randomization eliminates nuisance parameters" is "an instance of statistical inference." People's rejection of the statement may be because of their subconscious awareness that if the assumption is an inference, this inference does not agree with something very basic in statistics. This something very basic can, I believe, be explicated by the "ancillarity principle" (see my 1982 *Journal of the American Statistical Association* paper "Ancillarity principle and a statistical paradox"; Godambe, 1982).

So you see the answer to your question, how the paradox would be settled, is not easy. Perhaps that is not important either. It would be unfortunate, however, if the paradox is ignored. Unfortunate, for the paradox throws a new light on the old controversy concerning the role of individual labels in survey sampling. This controversy, it seems, in the course of time has contributed significantly to the clarification and extension of survey theory and practice.

Thompson: Is statistics a science, a technology or a discipline?

Godambe: Or is it largely just a collection of tools whose common use is derived from a *consensus*, mostly dominated by tradition?

Consensus also plays an important role in the older sciences such as physics and chemistry. But in these sciences the consensus, in addition to being traditional, also has theoretical and factual foundations. This, unfortunately, is not the case in many areas of statistics—in general, statistical methodology consists of a probabilistic model, and the data at hand are assumed to have come from the model or are actually drawn from the assumed model or population with a suitable sampling design. Conclusions about the parameters of interest are obtained using some estimation or test procedures. These latter procedures are often theoretically and practically rather well investigated. However, in many involved situations the assumption of an elaborate model is a matter of consensus, not necessarily well founded. This, for instance, is the case in small area estimation in survey sampling. Other illustrations of similar elaborate models from biostatistics, ecology, survey sampling are not difficult to find. The model diagnostics are at best at a very rudimentary level.

Of course there are important exceptions to the above rather pessimistic picture. For instance, in agricultural experimentation and in quality control, linear models and least squares estimation are well founded. Similarly in genetics and the related biological areas, Gaussian models and maximum likelihood estimation are firmly established. Further, of late, in large areas of biostatistics and survey sampling, the use of semiparametric models, including generalized linear models, and estimation based on the theory of optimal estimating functions is found compelling. These semiparametric models, because they assume only two moments of the distribution, are far easier to check for their validity than the elaborate models I mentioned. And the theory of optimal estimating functions provides a unification and extension of the two traditional methods of estimation—namely, least squares and maximum likelihood.

Thompson: I believe that you are saying that statistics as a technology is the object of study of statistics, the discipline! Thank you very much for sharing your thoughts and experience.

Godambe: It has been my pleasure.

REFERENCES

BERAN, R. J. and FISHER, N. I. (1998). A conversation with Geoff Watson. *Statist. Sci.* **13** 75–93.

CASSEL, C.-M., SÄRNDAL, C.-E. and WRETMAN, J. H. (1977). *Foundations of Inference in Survey Sampling*. Wiley, New York.

GODAMBE, V. P. (1951). On two-stage sampling. *J. Roy. Statist. Soc. Ser. B* **13** 216–218.

GODAMBE, V. P. (1955). A unified theory of sampling from finite populations. *J. Roy. Statist. Soc. Ser. B* **17** 269–278.

GODAMBE, V. P. (1960). An optimum property of regular maximum likelihood equation. *Ann. Math. Statist.* **31** 1208–1211.

GODAMBE, V. P. (1966). A new approach to sampling finite populations, I and II. *J. Roy. Statist. Soc. Ser. B* **28** 310–319, 320–328.

GODAMBE, V. P. (1970). Foundations of survey-sampling. *Amer. Statist.* **24**(1) 33–38.

GODAMBE, V. P. (1975). A reply to my critics. *Sankhyā Ser. C* **37** 53–76.

GODAMBE, V. P. (1976). Conditional likelihood and unconditional optimum estimating equations. *Biometrika* **63** 277–284.

GODAMBE, V. P. (1982). Ancillarity principle and a statistical paradox. *J. Amer. Statist. Assoc.* **77** 931–933.

GODAMBE, V. P. and THOMPSON, M. E. (1974). Estimating equations in the presence of a nuisance parameter. *Ann. Statist.* **2** 568–571.

GODAMBE, V. P. and THOMPSON, M. E. (1986). Parameters of superpopulation and survey population: Their relationships and estimation. *Internat. Statist. Rev.* **54** 127–138.

GODAMBE, V. P. and THOMPSON, M. E. (1997). Optimal estimation in a causal framework. *J. Indian Soc. Agricultural Statist.* **49** 21–46. (Golden Jubilee Number.)