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A Conversation with Morris Hansen

Ingram Olkin

Morris Hansen was born on December 15, 1910 in Thermopolis, Wyoming. He entered the University of Wyoming in 1928. After a working leave for a year he returned to the University in 1930, and received his BS degree in accounting in 1934. After graduating, he joined the Bureau of the Census in Washington, D. C. He studied part time for several years at the graduate school of the Department of Agriculture and American University, and received his master's degree in statistics from American University in 1940; he became Chief of the Statistical Research Division at the Bureau of the Census in 1947, Assistant Director for Statistical Standards in 1949 and Associate Director for Research and Development in 1961. He retired from the Bureau of the Census in 1968, and joined Westat, Inc., then a small statistical research, consulting and service organization, as Statistical Advisor and Senior Vice President. In 1986 he became Chairman of the Board. He received an honorary Degree of Doctor of Laws from the University of Wyoming in 1959, was President of the Institute of Mathematical Statistics in 1953, President of the American Statistical Association in 1960 and President of the International Association of Survey Statisticians 1973–1977. He is Honorary Fellow of the Royal Statistical Society, and a member of the National Academy of Sciences.

The following conversation took place in late May 1986 in Washington, D. C.

"I THOUGHT I KNEW SOMETHING ABOUT STATISTICS AND LEARNED LATER THAT THAT WAS A MISCONCEPTION"

Olkin: Morris, I notice that you received a bachelor's degree in accounting from the University of Wyoming and a master's degree in statistics from American University. Can you tell me about how you got to American University from Wyoming and how you got into statistics?

Hansen: Well, in Wyoming I didn't know what I wanted to do and finally decided to take accounting after one false start. And in accounting I was exposed to courses in economic statistics by a professor in the Commerce Department. He was a really fascinating teacher and got me interested in statistics. When I finished those courses, I thought I knew something about statistics and learned later that that was a misconception. But I knew a little and decided that I would like to go into statistics.

Olkin: How did you get to Washington?

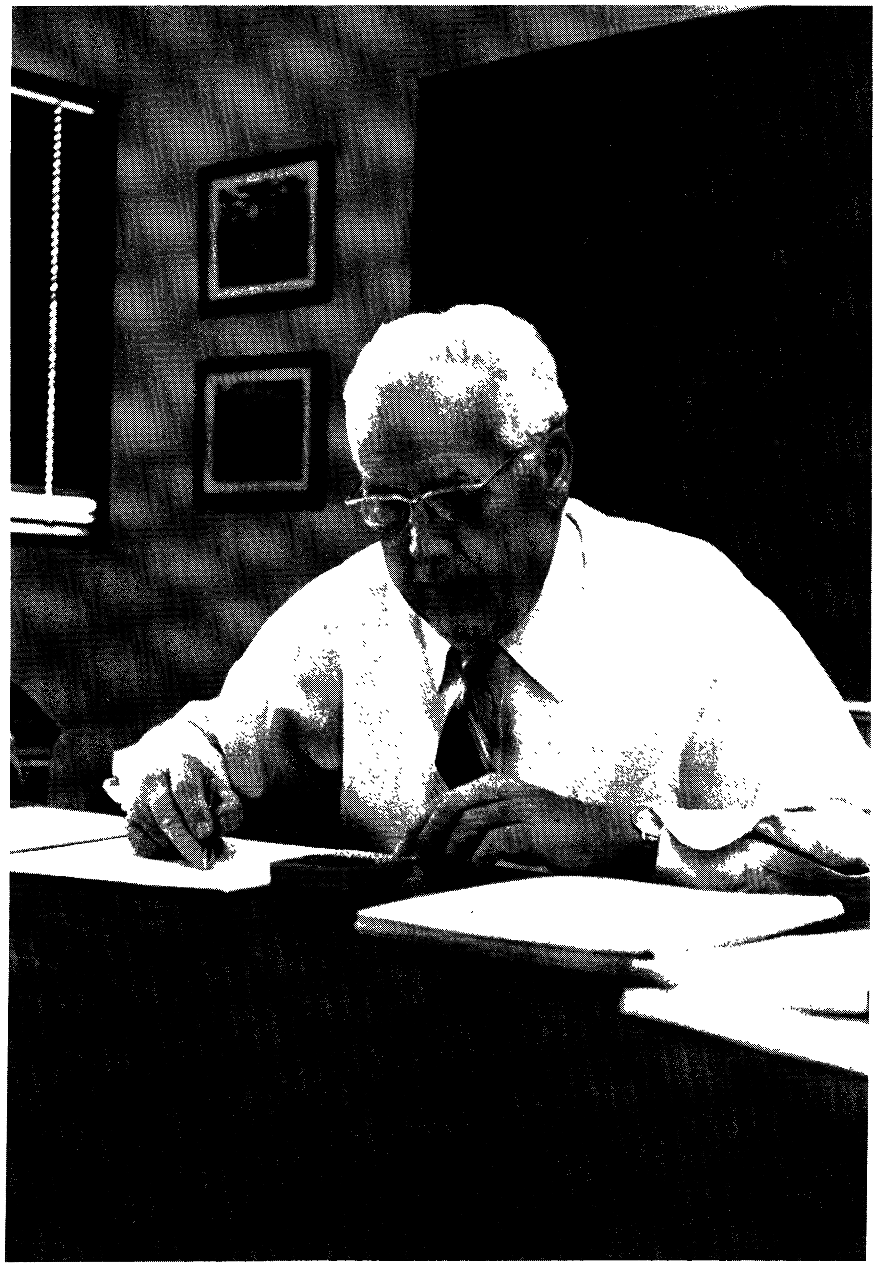
Hansen: I had seen some output from the Bureau of the Census, and I thought that I would like to work there. I took a batch of Civil Service exams. Wyoming was below the state quotas and I rated high on the exams, so the combination put me high on the list. No one would call me because I was from Wyoming, until a professor of mine who had a contact with the Bureau of the Census went back and soon thereafter I was there.

The contact at the Bureau brought me into the Personnel Division with the expectation that I would learn more about the Bureau and move out later. In Personnel I worked on classification problems and learned a lot about the Bureau that way for a year. Then Cal Dedrick, who was head of the Research Division, which at the time was a very small division, arranged for me to join him and start working in statistics.

Olkin: I was surprised by your resumé in that I did not think that a master's degree was available at American University in 1940. Apparently I was wrong.

Hansen: Well, most of the statistics courses I took were in the Graduate School of the Department of Agriculture. I'm not sure if any statistics courses actually were given at American University, but they cooperated in a program. I took other courses at American University, and they did indeed grant a master's degree in statistics. The faculty at American University responsible for statistics were applied statisticians; they didn't know much theory of statistics.

Olkin: So the main courses were at the Graduate School of the Department of Agriculture.



Morris Hansen, about 1983

Hansen: Yes. I believe Ed Deming was responsible for the statistics program. I took courses from him, from Abe Girshick, and from several others.

Olkin: Tell me about your progression in statistics after that. We are talking now about the years 1941, 1942, 1943.

Hansen: Let me go back a little before that, because I'll tell you how I got into the statistics business in the Census Bureau after my year in Personnel. I had been taking these courses from about 1935–1936 on. The Census Bureau, or Cal Dedrick and some others, thought that there was a future in sampling in the Bureau and assigned me and one or two others to begin to explore the field of sampling. A number of sample surveys were going on in Washington, including some large scale ones, but mostly without theoretical foundation.

Then a job came along, working in what was called the 1937 Unemployment Census. Someone conceived of a national registration of unemployed to be handled through the Post Office, and the President was behind it. Unemployment was the national problem at that time.

Olkin: Do you remember the approximate unemployment figures at that time?

Hansen: Estimates in the newspapers varied from

3–15 million. The 15 million figure may be high, but there was a wide range of guesses on unemployment by various methods. And it was really, as you probably recognize, a crisis problem.

I don't know who conceived of this national registration of the unemployed. It was authorized and appropriated for by the Congress and put into operation. There was a registration card, signed by President Roosevelt, to be delivered by the Post Office to every family in the United States. I suspect that it is the only statistical survey questionnaire, at least in recent history, signed by the President.

Some people, mostly sociologists, including Cal Dedrick, Fred Stephan, Stuart Rice, Sam Stouffer and others, felt that here was an opportunity to try to understand what unemployment was. They felt that a national voluntary registration with much nonresponse would yield numbers no one could interpret, and that we could do a check census in a sample of areas. And indeed, we did.

I was brought into that project by Cal Dedrick from the beginning and helped design it. Fred Stephan took a lead role in it. The Post Office personnel went out and actually served as census enumerators to enumerate a sample of postal routes.

Olkin: Were there special, interesting aspects to the design at that time, or was it relatively straightforward?

"ONE OF MY FIRST CONTRIBUTIONS WAS STIMULATED BY WHAT JERZY NEYMAN WAS SAYING"

Hansen: It was relatively straightforward—a systematic sample of, I think, 2% of the postal routes and a straight, complete census in those sample postal routes.

The interesting feature, though, was that the Post Office Department with its tremendous resources said that they would be able to classify the registration returns by postal routes and give us separate counts for each postal route, including the sample postal routes, which gave us the potential for an independent variable to use in estimation.

When the sample results were in, no one really knew how to use them at first. One of my first contributions was stimulated by what Jerzy Neyman was saying (at that time I had begun to have some exposure to Neyman), that we didn't have to know and match all the cases and understand all the relationships to take advantage of that independent variable.

We generated estimates of unemployment and estimates of the standard errors of unemployment that were basically very valid measures [Dedrick, C. L. and Hansen, M. H. (1938). *Census of Partial Employment, Unemployment and Occupations: 1937. IV. The Enumerative Check Census*. U. S. Government Printing Office, Washington, D. C.]. We also projected sample estimates to smaller areas through regression relationships.

Olkin: You mentioned that you began to be involved with Neyman. Where was Neyman at that time?

Hansen: I wasn't involved with Neyman in any personal way. He was here in Washington and was invited by Deming to give some lectures, frequently referred to as the Neyman Lectures published by the Graduate School of the Department of Agriculture. We didn't understand his ideas too well at the time, but they began to stimulate and guide us.

That whole experience was not only a demonstration of what area sampling and probability sampling methods could do, but it was also basically a demonstration of the Neyman philosophy of randomization, as distinguished from modeling using purposive sampling. It was a demonstration of what could be done, and was accepted and believed.

Before that, the Census Bureau had the idea that it couldn't do sampling because that would discredit the results; they had to have complete coverage.

Olkin: So this was really a major innovation in the philosophy of sampling and censuses.

Hansen: Yes. A major innovation in philosophy and understanding of how you do sampling and how you can measure precision from sampling. It exposed me to a lot of new procedures that I hadn't done before.

Olkin: Also, it appears to have been a training ground for a group of individuals who then became the leaders in sample survey.

Hansen: The staff who became leaders in sample surveys were not there then, but arrived later. A few of them had bachelor's or higher level degrees in mathematics and were recruited as clerks in the 1940 census—jobs were scarce. Ben Tepping, with a Ph.D. in mathematics, came in at a little higher level. Others, some of whom came in still later, came from other areas. The leading group at the time of the 1937 unemployment survey included Sam Stouffer and Cal Dedrick, who were sociologists, and Fred Stephan, who was a sociologist and also a statistician. This was my training area, and I'm sure that we all learned a great deal from it too. It helped us take the next major step in the Census Bureau, which was to create a role for sampling in the 1940 Population Census.

Olkin: What was special about that particular census?

Hansen: Well, the special thing was that for the first time—anytime you say "the first time," you're going to be wrong—sampling was used as a means of collecting data as part of the Decennial Census. No one trusted it enough to displace any questions on the questionnaire, like putting them only on a sample, but we were able to add a series of questions to the questionnaire in which there was great interest, and sampling made it possible financially to add these questions to the census. One of them was a question on wages and salaries, incidentally, that hadn't been in the census before. This was a 5% sample.

Stephan, Deming and I took the lead in the design work. I was the third member of that team.

Olkin: What came out of that effort in terms of the impact professionally or internationally? Did other countries take notice?

Hansen: I can't really say what came out of it internationally at that time. Some things that we did later certainly had an impact internationally, but I'm not sure about that particular stage.

But this was the beginning of a recognition that sampling had a role in the Bureau of the Census. The war came on in 1941, and we used that sample, incidentally, to obtain early estimates for many complete census items and to help serve the war needs as well as to obtain tabulations of the new items.

The 1937 Unemployment Census was the precursor of something else, at the Works Project Administration (WPA), initiated by Lester Frankel, Steve Stock, John Webb and Howard Myers. They had also participated in the design of the 1937 survey. Following this unemployment census, they decided that they could design a national labor force survey and keep track of unemployment on a national basis, using essentially the questionnaire procedures that had been developed in the 1937 Enumerative Check Census. These procedures were new and helped in clarifying conceptual problems of employment and unemployment.

They designed a national sample survey, a multistage sample, for which they deserve a great deal of credit. They did a first-stage sample of counties, and then they sampled within counties. In urban areas they sampled blocks, using what I would call reasonable probability sampling methods where you knew the probabilities of selection. In rural areas they felt they couldn't do that, and they used short-cut methods that got them into trouble later. They took a sample of random points and the households nearest those random points.

Olkin: That must have been an interesting idea at the time.

Hansen: It was not feasible to unravel the probabilities at the final stage of sampling, of including a household in the sample. But when the economy and population movements are relatively stable, you can use these kinds of methods to make acceptable estimates of change. If the whole state of the economy stays more or less static, they will give good measures. But if the state of the economy goes through a major upheaval, such as it did during World War II, with, among other things, all sorts of people being pulled off the farms and enlisting or going into available urban jobs, then there may be serious biases from such a sample.

The WPA was abolished shortly after the beginning of World War II, and the unemployment census was transferred to the Bureau of the Census. We continued that survey in the same way for about a year or so.

We soon went to work on redesigning the sample by putting it on a probability sample basis. We were now beginning to understand probability sampling and its implications, and we developed some new methods that helped facilitate the redesign.

The new sample survey was instituted in 1943, I believe. We obtained important and rather strikingly different results from it than had been coming out of the old survey. The new results were accepted as valid because we were able to project from the sample some other aspects that were known to have occurred and represented important population shifts from the prior decennial census. We were also able to argue that the results of probability samples were demonstrably acceptable within the range of measured sampling variability. In particular, we were able to estimate some substantial population shifts that were pretty well understood from other sources.

Olkin: One point that struck me is that your published papers appeared in the early 1940s, and I don't believe there was much published literature prior to the early 1940s. This suggests that the techniques being developed were completely new, or were some of the techniques already available?

Hansen: Well, there were all sorts of things around. As we agreed a minute ago, there's little that is brand new. Much of what we were doing in the Bureau of the Census that was new was contained in internal memoranda, technical reports or manuals, and was not published. Mostly we were operating somewhat independently of others. At this stage, though, Neyman's paper was available, and we were impressed and influenced by it. We ended up with a design that was a real improvement, as should be the case, over what had been done earlier. We learned a lot, and what we learned resulted in the publication of our 1943 paper ["On the theory of sampling from finite populations," *Ann. Math. Statist.* **14** (1943), 333–362].

Olkin: That was with Hurwitz.

Hansen: Right.

Olkin: What about sampling procedures in other countries, say, India or England? Were they developing sampling methodology?

Hansen: Yes, but that was primarily in agriculture.

Olkin: Was this being done by Frank Yates?

Hansen: Yes, Yates and Bill Cochran, before Bill came to the United States, and also some others working with Yates. They were using an analysis of variance approach to finite population sampling. This approach is not that different, but although both methods assume randomization, the philosophy differs from the Neyman philosophy which is based on random selection from a finite population.

"THE FIRST PAPER WE PUBLISHED—"

The first paper we published, incidentally, was a paper in 1942, which discussed the role and impact of intraclass correlations in cluster sampling [Hansen, M. H. and Hurwitz, W. N., "Relative efficiencies of various sampling units in population inquiries." *J. Amer. Statist. Assoc.* **37** (1942), 89–94]. Also it showed a defect of the analysis of variance approach to finite population sampling in the sense that it would give no opportunity to observe a negative intraclass correlation, and you could arrive at wrong conclusions.

Olkin: Was the notion of optimum allocation, that is, the idea of optimization, an inherent part of the development at that time?

Hansen: Neyman's basic notion, in his classic 1934 paper, was an optimum allocation of units to strata for a fixed sample size [Neyman, J., "On the two different aspects of the representative method: The method of stratified sampling and the method of purposive selection" (with discussion 607–625), *J. Roy. Statist. Soc.* **97** (1934), 558–606]. It turned out that the same result was obtained earlier by Tschuprow, but Neyman didn't know it. He acknowledged the priority later.

Then a year or two later, when he visited Washington in 1937, some questions were raised about double sampling. Some people were intuitively working with double samples, but they didn't have the theory for it.

Olkin: Neyman wrote a paper on double sampling.

Hansen: Yes. He came up with the idea of including cost functions and then optimizing with respect to cost. It then became obvious that this is the right approach. We later carried out a variety of extensions of that approach.

We incorporated in that basic 1943 design the use of sampling with probability proportionate to measures of size in ways that simplified multistage sampling. It gave you equal workloads where you'd like to have equal workloads instead of unequal workloads, and it also reduced the variance simultaneously.

Olkin: How did you come to consider sampling with probability proportionate to size, and what have been some of the developments from it?

Hansen: In the redesign of the Labor Force Survey, we were sampling large first-stage units followed by two or more subsequent stages of sampling. The first stage units were counties or groups of counties that varied widely in size, even within strata. The idea struck us that if the first-stage units were sampled with probabilities proportionate to measures of size in terms of people or households, and an approximately constant number of households subsampled, the overall probability of selection of households could be uniform, and at the same time an important source of variability would be eliminated or reduced. The method was especially convenient for multistage area sampling.

We developed the variance for multistage sampling with varying probabilities, with or without subsampling. However, in order to estimate the variance, we assumed sampling of the first-stage units with replacement. We had been unable to get the variance without this simplification. The theory was an approximation for sampling without replacement. In 1949, we prepared a paper on optimum probabilities, with or without subsampling, again assuming sampling with replacement in obtaining the variances.

Later, Horvitz and Thompson solved the problem of variance estimation for sampling two units per stratum without replacement. The estimator they used, widely known as the Horvitz-Thompson estimator, was the same as we had used earlier—it is the same whether sampling is with or without replacement. Their important accomplishment was to obtain variances for sampling with variable probabilities. Their paper, and especially the Horvitz-Thompson estimator, stimulated a great deal of interest that resulted in papers by a number of authors. A considerable number of later developments and papers of both practical and theoretical interest have resulted. One important topic that has received attention is a procedure for selection with varying probabilities, without replacement, and estimation so as to achieve stable variance estimates.

We also introduced other ideas in the redesign of the labor force survey: multistage poststratification, for example. At first, when we introduced probability proportionate to size, we thought we wouldn't have to use poststratification. We thought the problem was solved until we saw that those sample estimates were bouncing around month by month, a variation that was far too large. Multistage poststratification was needed, and we introduced it and worked out the theory.

An important point should be mentioned here. We adopted a philosophy that if theory and practice were to conform reasonably, it was necessary to restrict design features and operations to those that were operationally feasible. Only then could specifications be reasonably followed. It was also essential to take steps to assure that actual performance was closely in accord with specifications.

Olkin: When did ratio estimation come into use?

Hansen: Neyman talked about it in his 1934 paper, and they did some early work in ratio estimation at Rothamsted. Our introduction to it was the 1937 national sample survey of unemployment that I mentioned earlier, in which registration figures were available for the sample units and also for the total population. Ratio estimation made it possible to get useful results out of it.

Olkin: What happened after that?

Hansen: I think that the 1943 introduction of a whole redesign in the Labor Force Survey, which we took over from the WPA, was a major advance again, similar to the major advance that took place when we did the 1937 survey. We began to see that sampling had a role in the Bureau of the Census and started working to make it operational, especially when optimum allocation was important in some of the applications.

We went into the subject area where business establishments were being sampled, using judgment mail samples, and the reaction we got at first from people in that area was that sampling is fine for populations where the units are more or less homogeneous, such as households, but you can't do it for business establishments that vary widely in size. We were able, by working with them instead of trying to impose it upon them, to get them to be partners in the job, to get them to work along with us. Fortunately, we had complete support from top level management in the Bureau.

Morris Hansen at age 3

Olkin: What was going on during the World War II period in the Bureau?

Hansen: We were introducing sample surveys into quite a few areas by then.

Olkin: Were you involved with Army personnel in sampling or in providing manpower estimates?

Hansen: We obtained estimates of the availability of manpower and manpower shortages in major industries from the point of view of general population estimates. This was a point I was trying to make with respect to the redesigned Labor Force Survey. It provided estimates of agricultural employment and nonagricultural employment and showed that the employment in agriculture was far below what had been assumed.

It led to a new manpower policy with respect to deferrals in agriculture, for example. It was a very important instrument in manpower policy.

Olkin: How did you get policy changes as a result of statistical information? That's not so easy anymore.

Hansen: It's not so easy, and we did not have a policy role, except by providing the data. But the data were kind of sensational, and they did indeed lead to policy changes. It showed the foresight of the people in other agencies. There were a lot of good people in federal agencies during the New Deal and war period.

Olkin: Was there a receptivity toward statistical information and results?

Hansen: Yes, a receptivity and a demand for statistics, a great pressure on the Census Bureau. You asked earlier where I was in World War II. There were great pressures on us to do statistical services for other agencies, for example, for the WPB.

Olkin: The War Production Board.

Hansen: Yes, and the OPA, the Office of Price Administration, and others. But those were two very important ones. The WPB pushed industrial statistics in many ways, and we provided surveys for them as well as for the OPA.

Olkin: You then stayed on at the Bureau. Tell me about some of the people. I know that you and Hurwitz had a close relationship, certainly professionally; I don't know about it in other ways.

Hansen: Both, yes, personally and professionally.

Olkin: Where did Hurwitz come from and what was his background?

Hansen: Bill Hurwitz joined us in 1940. He was in the Department of Agriculture before he came to the Census. He came from Storrs, Connecticut with an undergraduate degree in mathematics and then studied with Harold Hotelling at Columbia. He did not earn a doctorate, but he received some good training under Hotelling, and then joined us in Washington in 1940. He and I started working together initially on some plans for a national sample census that we thought would be the way to go, a sample population census. We did a lot of work on how to design and do a national sample census to make it efficient. A lot of things could be said about it that are too detailed to try to say here.

But then we got this transfer of the Labor Force Survey early in the war, in 1942, and started examining and working on it. We really began to work together as a team. And as a team, we were certainly far stronger than the sum of the two of us working separately. We stimulated each other very substantially.

Olkin: There seems to have been a sense of community, camaraderie and excitement at the Census at that time.

Hansen: All of those. Bill Hurwitz was certainly one of the prime stimulators. He and I, working as a team, were a prime stimulator. Phil Hauser in the Population Division was a guy with foresight. Some of the early ones in addition to Bill Hurwitz and Bill Madow, who worked in sampling were Hal Nisselson, Ben Tepping, Joe Steinberg, Joe Waksberg, Joe Daly and Eli Marks.

Olkin: Was Deming in the Census Bureau at the time?

Hansen: Yes, Deming was brought in by Phil Hauser to serve as Mathematical Adviser on the 1940 population census. He was a prime mover in that census and one of the early people in developing and applying quality control to census operations.

Olkin: In what way did he bring that in?

Hansen: One of the major tasks was card punching. One part of card preparation was keying. Later we also concentrated on various other operations, such as editing and coding, and also other large-scale operations of the census. I'm sure he must have initiated some of that.

Olkin: But the main point was the recognition of quality control as an important ingredient in censuses and surveys.

Hansen: Right. Before that, we depended on 100% verification. The tradition up to the time of the 1940 census was to collect the data with really pitiful quality control and with inadequate supervision in the field. We tried greatly to strengthen the supervision in the 1940 census beyond anything that had existed before, and we did.

But still, you have a massive census in which you recruit about 400 supervisors almost overnight and train them in a few weeks to recruit and train on the order of 150,000 to 200,000 enumerators, and they're supposed to do their job in two weeks in urban areas and a month in rural areas. They don't adhere to this schedule literally, but that's what the law calls for. You can imagine the supervisory problems and quality control problems with one supervisor to about 200 enumerators. The quality was remarkably good, considering the circumstances under which it was being done.

But in 1940 we introduced for the first time an intermediate level of supervision called a crew leader. A crew leader supervised only about 20 people. That was still a limited supervisory staff, and they had to be trained overnight, too.

What I'm trying to build up to is that the quality of that data as it came in was not particularly good. But the Census Bureau, after it got the data, attempted to achieve 100% verification and to make sure that every process was carried through with near perfection. We tried to bring a balance into it and said it wasn't worth paying that price for 100% verification for the quality of data we had; we can control that quality very well with sample verification and a process control system.

Olkin: So that was quite innovative.

Hansen: Yes. The initial work was introduced by Deming. We carried it on and extended if after he left. He left shortly after the 1940 census and went to the Bureau of the Budget where he spent some years before moving into private consulting.

Incidentally, as far as working with Deming is concerned, after he went out into private consulting, I suppose some years after, he felt the need for someone to argue with, to interact with, and he contacted Bill Hurwitz and me to work with him from time to time. Pretty soon every other week or so for years I was going over to his house and spending an hour or two with him Saturday or Sunday morning. We are still carrying it on but on a more limited scale since he's such an international celebrity and traveling around the world all the time now.

Olkin: That's true. I'm glad you didn't say his vigor has diminished, because it's amazing what he's doing.

We were talking about Hurwitz, and I gather at some point here in the story, Bill Madow also came to the Bureau?

"THIS WAS THE PERIOD WHEN THERE WAS A LOT OF DEVELOPMENT IN SAMPLING"

Hansen: Bill came to the Census Bureau as Mathematical Adviser approximately when the Labor Force Survey was transferred to us, and we took responsibility for sampling for this survey. But he wasn't there very long. Bill left the Bureau and went to South America during the war.

This was the period when there was a lot of development in sampling. Before Bill left we decided to write a book jointly which turned out to be many years longer in the doing than we anticipated [Hansen, M. H., Hurwitz, W. N. and Madow, W. G. (1953). *Sample Survey Methods and Theory* **1**, **2**. Wiley, New York]. I don't know, I'm sure the other pastures always look greener; but it seems to me that writing a book when you're working full time, doing the things we were doing, and having to do it on weekends and nights—it was a much bigger job than we anticipated. It took us about eight years.

Olkin: How did that project start? Did the three of you decide at one point to write things down?

Hansen: I think that's probably so. I don't remember the details right now. I think it was probably Madow who took the initiative and made the initial contact with Walter Shewhart, who was Wiley's consulting editor—I'm not sure—and found that they were interested in getting something written. We had become somewhat known by then. And sampling was now an interesting area in which a lot was going on. Also it was impacting other areas a lot. Our 1943 paper on sampling from finite populations, I think, had an impact around the world.

Olkin: I never talked to Bill about his involvement in sampling. I do know that he received his degree from Columbia University about 1939. His dissertation field was multivariate analysis. I was wondering how he got into the Census Bureau.

Hansen: I'm sure that the census was an area that was receiving attention and interest, and he got interested in it. I believe he told me that applying multivariate analysis was difficult with the computing facilities then available.

Olkin: Also, there were not that many departments of statistics if people wanted to go into academia.

Hansen: That's right. I'm not sure that he was doing what he thought was interesting work. I think he was in the Department of Agriculture, but I'm not sure. That's where Bill Hurwitz was and I believe he had met Madow there, or at Columbia, and influenced him to come to the Bureau.

Olkin: Coming back to your own career, consulting was also a major part of your activities. Was it private consulting that got you involved in thinking about Westat? How did that come about?

"THERE WAS A CONSIDERABLE OPPORTUNITY FOR PRIVATE CONSULTING"

Hansen: Westat is a private statistical research and consulting organization with a strong emphasis on design, data collection, processing and analysis of sample surveys. However, its activities are not restricted to this area. For example, Westat staff is active in training and consulting of statistical process control.

At that time there was a considerable opportunity for private consulting. Since our youngsters were getting through school and going to college, Hurwitz and I found that we had to supplement our income. We were constantly getting offers that were far more financially attractive, but we weren't anxious to leave the Bureau since we were enjoying what we were doing. We discussed with the Census Bureau management whether we could stay in the Census Bureau and still carry on a limited amount of outside consulting so long as there was no conflict in what we were doing.

The Bureau wanted us to stay and agreed to let us engage in some private consulting. We made a continuing consulting arrangement with a group called National Analysts, later called Westat.

Olkin: Was that in surveys or polls?

Hansen: No, it was in a wide range of areas. I guess what we were doing was more operations research than sampling. For example, we had been in the forefront in early computer applications, and we began working with Eli Marks and Ben Tepping on computer applications to subscription fulfillment for the *Saturday Evening Post*. This came about because at that time National Analysts was owned by the Curtis Publishing Company which published *The Post*. We did a lot of innovative work in setting up a subscription fulfillment system, involving all sorts of matching problems.

Olkin: This actually was a period when operations research was in its infancy and beginning to be developed.

Hansen: Yes. We also did work with them on estimating the consequences of their promotional efforts, applying a Markov process. We carried through a Markov process to show that the consequences of certain classes of promotional efforts were far different than they had anticipated, for example. Those were areas in which we spent a lot of our time.

We went out and did some other consulting, through National Analysts, with companies such as John Deere and *Reader's Digest*, on operations research and subscription fulfillment.

Olkin: So in a certain sense, Westat was a natural extension of your consulting.

Hansen: In a limited sense, yes. And we continued, I guess, that consulting until we became involved with the telephone companies in connection with financial settlements for their use of each other's facilities for routing toll calls.

Olkin: Was this happening even at that time?

Hansen: Yes, indeed. They had developed a system for dividing funds from intercompany toll calls between the Bell System and the independent companies. They asked us to work with them on the sampling aspects of this. This was strictly a sampling problem and was not in conflict with census type of activity. We did a fair amount of work on this project, and I continued that consulting even after I joined Westat. Ultimately, I got tired of working overtime on consulting and incorporated it into a Westat activity.

Olkin: You have been at Westat, now, about 18 years. What was your involvement in Westat and what kinds of projects did you pursue there?

Hansen: Well, a lot of them are similar to what we were doing in the Bureau of the Census: the design of sample surveys. We have always looked upon sample surveys as a total system design problem. That's what gives us this operations research element: looking at how you put a whole system together that will be effective.

One of the things that I personally did with Ben Tepping and Joe Waksberg (both of whom are retirees from the Census Bureau and who are working with us in Westat) was consulting with the Bureau of Labor Statistics (BLS) on the Consumer Price Index. The general philosophy on the Consumer Price Index had been, I think, pretty much like I mentioned earlier with respect to sampling from highly skewed distributions for business establishments in the Census Bureau. The philosophy was, "You can't do probability sampling; it applies to other areas but it doesn't apply here."

We indicated to BLS that we thought probability sampling could be applied, and came up with proposals and suggestions for some innovative aspects in the design of the Consumer Price Index: how they get their samples of establishments and their samples of items to price within establishments. I guess I have to say that we applied a probability sampling philosophy. We proposed methods for selecting these samples using procedures that would produce probability samples. However, because changes are taking place constantly and you have to keep this same sample in operation continuously, you can't say it's completely up to date in a probability sampling sense. Nevertheless, it was a substantial advance in the selection of establishments and of the items to price within establishments and on the estimation of variances from the samples.

When we started suggesting the kinds of methods we were going to propose to them our attitude was

Morris Hansen, the trumpet player, fourth from left, about 1947 pretty much that it was too late because they were already well along in the cycle of redesigning. However, they thought that our suggestions would solve some of their problems, and they were receptive to the ideas. They scared us a little because they were jumping into things that we weren't sure should be adopted before more adequate testing.

They did preliminary testing and adopted the methods, liked them, and have been extending them into other areas such as the Wholesales Price Index—now called the Producers Price Index and the International Trade Price Index.

Olkin: Morris, there were exciting developments in those early census days. Are there exciting developments in sample surveys nowadays, or since then, that you've particularly taken note of?

"THE AFDC PROGRAM WAS BEING ATTACKED BY MEMBERS OF CONGRESS—"

Hansen: Well, there are still interesting developments. I'm currently working with Ben Tepping on what's called quality control of welfare programs, especially the Aid to Families with Dependent Children (AFDC).

I had suggested earlier when I was on a committee advising them that AFDC introduce quality control. They were being attacked by members of the Congress who sent investigators out who were finding ineligible people on AFDC. So in a committee that I was on, we suggested they set up sample evaluation studies. They reacted favorably and set up a quality control system especially to measure and try to find sources of error, and to do feedback to improve the administration of welfare. And in about 1983 a member of the administration got the idea that since the federal government subsidizes about half of AFDC, depending on the state, they shouldn't subsidize the states for the overpayment errors made by the states in administering AFDC, above an allowable tolerance. They came to Westat to find out how to estimate overpayments from the quality control samples. We proposed that they set up a federal monitoring program using a subsample from the state quality control sample. The quality control system then was a state evaluation program in which they took a sample of AFDC cases, and investigators went out and intensively reviewed them. We suggested that they use a federal subsample to evaluate the state evaluations, along with a double sampling regression estimator, in order to get the maximum information we knew how to get from the data available, to estimate the overpayment error rate. Also, it made the sanctions depend on the federal investigations, not the state's, so that the states weren't being asked to hang themselves. The sanctions, now called disallowances, amounted to many millions of dollars that have been assessed against some of the states for estimated overpayment errors beyond the tolerance levels that have been established by Congress.

Olkin: How about theoretical work, such as probability sampling and quality control?

Hansen: That's what I'm building up to. The states sued AFDC, and AFDC asked me a year ago to come in and look into their system, evaluate it and be available to give testimony for the Government in these suits by the states. They gave us as much money as we needed to look seriously into this and to do some more theoretical work as needed.

The kind of theoretical work we were doing especially was based on Monte Carlo type simulations. This was because asymptotic approximations are involved and exact theory is not available for much of what we were doing. We were examining and learning a great deal, and it was fascinating. We're sampling from distributions that are remarkably difficult to sample from, distributions where for a high fraction of the cases the payment errors are zero, with a highly skewed distribution for the rest of them. And we've been getting into some very interesting theoretical and simulation type activities.

Olkin: Have you become involved with Bayesian statistics or other techniques developed within the last ten years?

Hansen: Not really. I guess I endorse and approve the kind of thinking that Don Rubin has been doing.

Olkin: With respect to missing observations?

Hansen: Yes, in missing observations. Sometimes it's necessary to do modeling in sample surveys, where probability sampling methods aren't applicable as in the case of the imputation for nonresponse. We certainly have been involved in such methods. In general, I can't say that we have been working in that area very much. However we are interested in the potential in that setting.

Olkin: Now, Morris, to switch topics somewhat, I would like to know more about you and the scene in Washington. Did you travel much? Did you go to England or India? In the early days, what was your connection with, say, Iowa State, or various other universities? In general, what was happening during that period?

Hansen: Well, our early travels were to Canada and to Iowa State during the war. Canada was interested in the Labor Force Survey as we had redesigned it and invited me to visit. They had set up a somewhat similar survey. We formed an interactive relationship that has continued ever since. They had some very good people, such as Nathan Keyfitz early on, and Ivan Fellegi later, and we had a good deal of interchange.

Currently, I happen to be chairman of an Advisory

Committee to Statistics Canada on statistical methods and have been visiting Canada about twice a year or so.

We also interacted a lot with people at Ames, Iowa, such as Bill Cochran, Ray Jessen, Arnold King and others, where a lot of original work was being done in sampling for agriculture, and a lot of interesting thinking and interactive development went on.

Olkin: How about England or India? Was there a lot going on in the 1930s or 1940s?

Hansen: Right after the war P. C. Mahalanobis came to the United States and visited the Bureau of the Census among other places. We were impressed with what he had been doing. There was quite a lot of parallel work going on in India more or less independently.

Olkin: Was that all at the Indian Statistical Institute?

Hansen: There were two places where those things were going on: one was Mahalanobis at the Indian Statistical Institute, and he was the pioneer, but P. V. Sukhatme and others were also doing pioneer work.

Olkin: And he was where?

Hansen: He was at the Indian Council of Agricultural Research. Mahalanobis and Sukhatme were rather bitter competitors in many ways, but they were both doing good work.

A lot of the focus was on how to design a sample survey to control nonsampling errors. In general, nonsampling errors are errors of measurement or response including improper responses, recording and processing errors, etc. The nonsampling errors in this case involved errors in the identification of the plants to be included in the sampled plots. Mahalanobis had been doing sampling where he harvested plots for estimating crop yields, using fairly small plots. Sukhatme did some experimental work and showed that as the size of plot was increased, the aggregate estimate decreased, and substantially. Boundary errors tended to result from including some plants on the periphery that should not have been included, and the periphery was bigger relative to the area for a small plot than for a big plot.

"I HAD LUNCH WITH NEHRU"

Olkin: I understand that Mahalanobis introduced you to Prime Minister Nehru. Perhaps introduced is a strong word.

Hansen: No, that's correct. I had lunch with Nehru.

Olkin: What were your impressions of that meeting with Nehru? Do you recall that luncheon?

Hansen: I recall the luncheon very well, but it was lunch with about half a dozen other people, so that it was not an opportunity to have more than a very casual conversation. Mahalanobis was interested in having me advocate his plans and positions, and I thought they were good, so I did support him. I may have had some small positive impact for him.

Olkin: Was Nehru receptive?

Hansen: He seemed to be, but he was already highly receptive to Mahalanobis. Mahalanobis was a powerful influence on Nehru. He was Nehru's principal statistician and economic planner and took the lead in developing five-year plans in India.

After Mahalanobis visited the U. S. we became close friends, and he and his wife, Rani, came to the house numerous times, and we stayed at his house in India. He made a point that surprised me very much at the time, and I came to believe it was true, namely, that the two statistical organizations in the world that were remarkably similar in their sample survey work, and like no others, were the Bureau of the Census and the Indian Statistical Institute. Each was looking at total sample survey design and using innovative approaches and developing theory as needed. None of us was developing theory to write papers. In fact, we didn't write many papers. But when you have a problem to solve and you need some theory, you develop the theory to solve the problem.

Both organizations had the same philosophical approach and both were large scale organizations doing large scale work, although ours was much larger. Both were doing a lot of innovative developmental work.

Olkin: How about in England?

Hansen: We visited England and talked to people there and had interchanges with some of them from time to time. There was not much interaction with Yates at that time. We worked closely with Bill Cochran when he came over from Rothamsted to Iowa State College in 1939. We never had much interchange with the British, although we did interchange some with the organization that was responsible for the population census. They were responsible for demographic statistics, and I think they were influenced by what we did in some ways. The British started a sample survey called the Social Survey that we had some involvement with.

But there were no strong, personal or continuing relations such as with Canada or with Mahalanobis, though we certainly benefited from the early developments at Rothamsted. I think we took the lead in many ways when we got into pushing sample surveys into area after area, innovating in many ways. But they were applying sampling to agriculture much earlier, and we owed them a great debt.

Olkin: What about the scene in Washington; what was happening here at universities such as George Washington or American?

Hansen: George Washington had a statistics program. Solomon Kullback was there, and he was a strong researcher, though not particularly interested, as far as I know, in the kind of problems we were working on.

Olkin: So most of the statistical activity was really centered around federal government agencies.

Hansen: When I came in and did most of my early studying of statistics, I told you it was in the Graduate School of the Department of Agriculture, even though I was getting my degree at American. And most of the people teaching there were in the government, teaching part time, people like Girshick and Deming.

Hurwitz and Madow started a course in sampling there. I replaced Madow when he went to Brazil. Joe Steinberg and Joe Waksberg from our staff soon replaced us and continued this course for a number of years.

Olkin: Were the professional societies very active in Washington?

Hansen: The Washington Statistical Society was active. In those days many times we would have a meeting on one aspect or another of sample survey methodology and it wasn't unusual to have a turnout of 100 or even 200 people.

Olkin: Really?

Hansen: I don't mean that was the everyday meeting, but it was not unusual either. I was definitely impressed: there was a lot of interest.

Other agencies of the federal government were also active. Jerry Cornfeld was in the Bureau of Labor Statistics and was a participant. He was very strong and did some good work there. When he left, they were relatively slow to pick up what we thought of as the methods that were being developed then. Of course, they used these later on.

Charles Sarle at the Department of Agriculture was providing leadership in research on sampling and its possible applications in the field of agriculture. He established the Statistical Laboratory at Ames and they set up another center or two. But they were not incorporating research results very fast into their operations. I think that was one of the major differences between the Bureau of the Census and some of the other agencies. We were pressing, pushing, getting sampling methods and statistics into every area we were working in.

COMPUTERS IN THE CENSUS BUREAU

Olkin: We had talked a little bit about computers at the Census Bureau. Perhaps you can review or discuss some of the innovations in computing that went on there.

Hansen: A very fascinating area.

Olkin: Were von Neumann, Eckert, Mauchly or any of the other movers in the early days of computers involved at all with the Census Bureau?

Hansen: Yes. The ones that you named who were especially involved were Eckert and Mauchly. They had developed the idea that vacuum tubes could do switching a lot faster than relays. I am not sure if they actually developed the idea, but they recognized it could be applied in the computer area. They built the Eniac during the war, which was used for computing firing tables and maybe other wartime applications. The Eniac was a vacuum tube machine with a mammoth number of vacuum tubes.

They then got the idea that they could build a machine that had the logical design of a modern computer with vacuum tube switching. They didn't want to use vacuum tubes for the memory because it took too many vacuum tubes and had too many reliability problems.

So for memory they used mercury tanks, circulating impulses at the speed of sound through mercury tanks and regenerating them and keeping them circulating. They used vacuum tubes for the computing part of it, and tape input and output so they could get large amounts of data into and out of the machine. With the old Eniac, they moved dials and read dials to get data in and out. Now they could get data in and out at what we thought were tremendously high speeds at the time.

They came to us in 1945 and said they could design a machine that will do these things and build it if we would give them financial support. They knew that the Census Bureau had done pioneering work in punch-card equipment and had need of large scale applications. If they could get the Census Bureau to support them, they would be in business. We looked at it and were just impressed and amazed; it was unbelievable to us at first.

We went to the Bureau of Standards, and they told us this was feasible. We were able to get Congress to authorize us to use some money, that we would have otherwise lost, to put into a contract and support the design of the first Univac, which was designed and built along the lines Eckert and Mauchly had originally proposed. The contract was placed and monitored through the Bureau of Standards, and they exercised strong leadership.

We interacted in some aspects of the design phase. For example, things were moving so fast that by the time they had built a mercury tank memory for the Univac, it was quite clear that electrostatic tubes would provide a faster and more effective memory. The question was: do we go ahead with the original plan of using mercury tanks, or do we hold up and put in a changed design with electrostatic tubes? Well, with the participation of the Bureau of Standards, we made the decision to go ahead with mercury tanks.

We concluded that we'd never get a computer if we wouldn't accept one that wasn't obsolete by the time it was built.

Our people were also doing programming on applications that had some impact on the code structure they set up. The machine was delivered to us on March 31, 1951. We accepted it on that date and thought that we had the world by the tail. We found that we still had a lot to learn. It seemed obvious to us that if you could build a computer that had these marvelous features, it would be no problem to build a machine that would convert cards to tape.

They built a couple of card-to-tape converters. We had to initially record the data on punched cards to get enough equipment to key the massive amounts of data that we keyed for a census. The cards were then recorded on magnetic tape through a card-to-tape converter. However, we found that the card-to-tape converter made occasional errors. The data wouldn't meet the odd-even or the redundancy test. These occasional errors caused us lots of trouble in getting the system into use in the first place, but we managed to surmount those problems and did a small but significant part of the 1950 census on the computer.

As soon as we ordered the Univac, we began to perceive that we wouldn't have a balanced system, unless we had a way of avoiding all that keying. Since mark-sensing was around, being used on small scale work, we went to IBM to see if they could design equipment that would do the massive mark-sensing operation that we would have to do to key in the 1950 census. We worked with them on it for about a year, and they came to us and said we weren't going to get there. So we did key in the 1950 census and went through a card-to-tape transcription for that part of it on the computer, which, though it was an important part, was still a relatively small part of the total census.

Then, immediately after IBM said they couldn't do it, we went to the National Bureau of Standards (NBS) and started working with them on methods of mark reading. This is position and not character reading. NBS conceived of the idea that they could do it reliably through microfilm, microfilming position-marked questionnaires. We did a lot of testing on the feasibility of position-marked questionnaires and later did a lot of testing on the feasibility of getting the public to fill in position-marked questionnaires and came up with the answer that it was both feasible and economical. So they designed what was called the FOSDIC (Film Optical Sensing Device for Input for Computers) and built the first one for us in time for the 1960 census.

We also did a number of other things on the design of a new system for the 1960 census, so that it would become sort of a model of advanced methods for taking a census. We had done some work on response error models and experiments that we incorporated into the 1950 census, and from these we obtained some interesting empirical results.

RANDOMIZED EXPERIMENTS IN THE CENSUS

In the 1950 census we randomized the assignments of about 700 enumerators out of perhaps 200,000 in such a way that we could measure the variance between and within the enumerators. Each enumerator received two random assignments within an assignment area. By computing the variance between and within enumerators, we were able to demonstrate that the enumerators were an important source of correlated response errors. These errors especially affected small area data involving the work of one or only a few enumerators. With a big enough area you have a lot of enumerators, but with a small area there were only two or three enumerators.

We were able to show that self-enumeration would reduce correlated response errors, and also that with self-enumeration and the use of a sample we could collect a lot of these data with essentially the same or improved accuracy as for a complete census. We therefore took a lot of the important items of the census and moved them over to a sample, including all of the items that involved manual coding or intervention. Then we could put the complete census through microfilming and Fosdic fast, without any manual coding and with relatively little editing on those units rejected by the computer editing.

While we were putting the complete census through microfilming, Fosdic and the computer, we could be doing the manual editing and coding on occupation, industry, income and other items and then put those sampled items through Fosdic and the computer.

Olkin: Sounds like an exciting era.

Hansen: It was really an exciting operation, a very successful one.

Olkin: I know you have learned shorthand and often see you using it. Have you also learned to use computers?

Hansen: Not really. People on my staff did all of the work on the computer. I only recently bought a little personal computer, a Macintosh, and I'm having some fun learning about it. But I'm far from being a master of the computer. I guess part of the problem, as far as my learning is concerned, is that there's always someone there to do it for me.

And the remarkable guy there to do it for me is Ben Tepping, who has had a Wang minicomputer in his home for some years. Then in addition, not too long ago he got a Macintosh Lisa. That's what he persuaded me to get. He now has an additional Macintosh Plus which he's got hooked together. And he is a powerhouse not only as a statistician but in computing. Anything we want, we talk about and he comes in with an answer. I don't have need to do much work on it.

Olkin: To switch gears from computing, Morris, you have received many honors. Would you tell us about some of them. The Rockefeller Public Service Award, what was that for?

Hansen: I think, formally, that was for the book that we published. Although we wrote it outside of the census, we got all the support from the Census Bureau that you could reasonably expect and still avoid a conflict of interest.

The book was a report on census work basically, and the award was an acknowledgment of the contributions made in improving census methods.

Olkin: When did you become a member of the National Academy of Sciences?

Hansen: I think it was about 1972.

Olkin: Have you been active in the Academy?

Hansen: I have been active in certain aspects of the Academy. I was a member of the Committee on National Statistics for several years. I was one of the first members. You don't have to be a member of the Academy to do that, of course. But as a member of the Committee on National Statistics, I was involved in a number of the projects they did, more or less intensively. Then I was asked to serve on the Report Review Committee where I did a lot of work for the Academy, hard work, basically reviewing reports that involved statistical work of various sorts.

Olkin: Are you in the social science section or in the mathematics section of the Academy?

Hansen: I'm in Section 33, which is applied mathematics. I have some doubts as to which place I ought to be, but there's sort of an agreement with the statisticians on applied mathematics. I'm less a mathematician than some of the others. I think it would be more appropriate for me to be in the social science section, but I fit in applied mathematics too.

Olkin: Are there other topics that we haven't covered, Morris, that you want to talk about? What's your goal for the next 25 years? You've only had two major careers; that's not so much!

Hansen: You know, it's been 50 years since I joined the Bureau of the Census, a little bit more. And really, I've enjoyed almost every day of it and worked with a lot of enthusiasm in most things.

We haven't really said as much as might be said about the response model that Bill Hurwitz and I put together—really, it was Bill Hurwitz, Eli Marks, W. Parker Mauldin and myself back in the late 1950s. We published a paper on it in 1951 [Hansen, M. H., Hurwitz, W. N., Marks, E. S. and Mauldin, W. P., "Response errors in surveys," *J. Amer. Statist. Assoc.* **46** (1951), 147–190]. Later, there was a paper, with some further developments by Hurwitz, Max Bershad and myself, published in 1961 [Hansen, M. H., Hurwitz, W. N. and Bershad, M. A., "Measurement errors in censuses and surveys," *Proc. Internat. Statist. Inst.* **38** (1961), 358–374].

Olkin: Tell me about that.

Hansen: We did some more work, in which we took some of the results from the 1950 census experiments with randomized enumerator assignments. The attitude used to be that the only way you can get correct answers in the census is to train the enumerator so that he can ask the questions properly and answer the respondent's questions. But training enumerators adequately, and to be able to control their work is a problem when there are a couple of hundred thousand to be trained in a few days, and complete their work in two to four weeks without any reasonable opportunity to review seriously their work in time to take corrective action. If they've misunderstood something, it impacts on the enumerator's whole assignment. Sometimes it isn't a misunderstanding, but a kind of personal interpretation.

When we saw the effect of correlated errors within enumerators we asked ourselves: How can we reduce interviewer variance? The answer was by reducing the role of the enumerator and by having the respondent respond without interviewer intervention. Then you won't have correlations between the responses for different households except as interviewers have to intervene. But they intervene much less. They edit the returns, and intervene only when the results are not acceptable in some sense: items are missing, things like that, or total nonresponse.

We also ran some tests in a few metropolitan and other areas as a part of the 1950 census, and after the 1950 census, on the use of self-enumeration on a large scale. With these results we were able to get acceptance of the idea of using self-enumeration as the major vehicle for taking the 1960 census, along with the other changes I had mentioned.

We found that for a lot of subject items, though not all of them, we could reduce the response biases as well as response variances by using self-enumeration. There's a paper by Con Taeuber and myself that summarizes some of the results. There are a number of papers by others, too, but that's one I can mention right now.

So that was an important area that I thought we hadn't covered.

I might add that we use interviewers successfully in continuing current surveys without the same level of correlated response errors. For these we have

Senior statisticians at Westat, about 1985. Left to right: Joseph Waksberg, Edward Bryant, Harold Nisselson, Morris Hansen and Benjamin Tepping.

permanent part time interviewers, so we can successfully set up quality control systems, process control systems, with training and retraining, editing of returns, observation and re-interviewing, all with feedback. We were able to avoid having large correlated errors and biases: I don't mean they're zero, but they're not of the same magnitude as in a census.

Olkin: One of the questions I had, which is not on the specifics of the census or sampling, is a little more of a philosophical question. How do you see the health of the statistical profession, and what do you see as the future? What are the things you would advise the newcomer to do?

Hansen: Well, I don't feel very expert in trying to answer that question. But I'll make a few comments. I feel that it's just wonderful what you're doing in education—interacting in a subject area with statistics. Some of the people I've seen coming out in mathematical statistics with Ph.D.s are more interested in writing papers than in solving applied problems. If they're interested in applied work and have a reasonably good statistical background, they can learn to work effectively in almost any field. For example, Bill Cochran moved from experimental design to sampling to biostatistics, back into a more general sphere. I am sure that advancing theory as a basic research activity, and publishing papers, is exceedingly important, and has made major contributions to applied work. But there is also a need for people who have learned to want to solve applied problems and not just write papers.

Olkin: How do you get people interested in the applications and learning enough about the substantive area to become involved, to be able to be productive?

Hansen: Well, in the first place, they work with people that know the substantive area, a team approach. In the Bureau of the Census, incidentally, we had a system, that Deming talks about as the model of the way it ought to be done.

The approach was one in which we set up an administrative structure that some of the management people say shouldn't work. We assigned a statistician from central staff to take the leadership on the application of statistics and operations research in a certain subject area. The statistician reported to Bill Hurwitz and to me, technically. He reported to the chief of that division, administratively. The chief of the division gave him his work assignments, but his promotions depended on us as much as they did on the chief of the division and had to be mutually agreed upon between us and the chief.

Also, we set up central staff meetings in which we had an interchange between the people in all of these subject areas. We regarded that statistician as transferable from one subject area to another. By and large he went into a subject area and stayed there a long time and became a specialist and proficient in that subject area. But we regarded his statistical competence and his willingness and interest in applied problems as his prime qualifications. And we sometimes took him out of one subject area and put him in another to get some new ideas in the other subject area.

Olkin: Doesn't this require the receptivity on the part of the substantive area for statisticians, which is not always the case?

Hansen: It requires that, or it requires top management who insist on that receptivity. Ordinarily, when we wanted to sell something new in an area, we would go in and get the chief of the division or one of his top people working with us. But we wouldn't go in and say this is what you ought to be doing.

We jointly developed with them what they ought to be doing. We found that we could often end up writing a memo over our joint signatures saying what we proposed in dealing with certain problems. We educated them and sold them on the application of statistics in their area instead of going in and saying, "You've got to be doing this."

We rarely went to top management, of which I later became a part. Still, the director is the one who would have to make the decision in the event of disagreement. But, generally, we got acceptance. If we did, we didn't write a memorandum taking credit. But rather, it was something joint.

Olkin: It was collaborative.

Hansen: It was a collaboration. On a very few occasions we had a major conflict and had to go to the director. Ordinarily, though, if the division chief wouldn't back us up, the associate director would.

Olkin: But Westat primarily, is a statistical type organization; whereas, for example, in the FDA or any of the other agencies, how do statisticians infiltrate and work on the applied problems, if they're not even recognized by the agency?

Hansen: This is a problem for the agency. I think that they have much to learn on how to get statistical work done. I indicated already that I think a good statistician who is interested in applied work can work with a person in a subject field. They can work together and each one of them has to learn some of the other's lore and theory. But they can work together and do a very effective job.

But very often there will be a request for a proposal for bidding on a project from some agency that says, "In order to bid on this, you've got to have someone who's a specialist in this subject matter, and he also needs to be a statistician." I think that's the wrong philosophy. When they do that, we sometimes are able to bring in consultants, and show that we can qualify on both sides.

But by such a requirement an agency often limits itself to people who may know the subject area but they bar people who can do the job right. Sometimes we have been able to persuade them that what they need, if they want to do a sample survey, is people who know how to do all aspects of sample surveys including the design, organization, management, data processing—the works. They also need people who are willing to come in and learn the subject area sufficiently by working with their people or by bringing in subject area people to fill in that gap. You certainly have to have the team approach on it.

Olkin: I'm inclined to agree. But what I've seen is the fact that other agencies and other groups are not sensitive to the power of statistics and to some degree, perhaps, even resentful at times, certainly not receptive.

Hansen: Yes, indeed.

Olkin: Even at a university where we have a number of joint appointments, it's not a very easy arrangement to effect.

Hansen: Well, I would prefer, if I had the choice, to bring in an Ingram Olkin to work on solving a problem in a subject area that he does not know, rather than to bring in a subject matter specialist without adequate statistics, or a highly trained mathematical statistician who isn't interested in applications and doesn't have the innovative approach to solving applied problems. Problem solving is the heart of this. Systems design is also involved.

Olkin: Should we be teaching more problem-solving and applications?

Hansen: I think so. I guess it's what I was saying earlier: I don't think it should be exclusive. You need the whole gamut from people who are interested in theory and want to write papers to the people who want to solve applied problems. If you're interested in solving applied problems, you often don't get to write papers on the new theoretical developments that result. We often found that someone else published a result long after we had solved the problem and been applying the solution.

Olkin: In your perspective, have you seen a change in emphasis in statistical theory from the applied to the theoretical in any particular way? For example, would you characterize the 1940s or the 1950s as a theoretical or an applied era? Are we now in an applied era at the moment?

Hansen: I really don't know how to answer that question, but I believe that recently there is increased interest in applications. I indicated my great pleasure that you're working in an applied field as well as in theory. And I see you around advising on problems. I think that's great. I think this new publication, *Statistical Science*, that you helped start, is a wholesome move toward dealing with areas of applications.

Olkin: I think there is a recognition of applications just in the fact that there are journals such as *Technometrics*, *Biometrics*, *Psychometrika* and *Econometrica*. That already indicates that there are special areas, though I must say at times they become very theoretical.

Hansen: There's a tendency to become more theoretical over time.

Olkin: Is there anything else, Morris, that we should talk about?

Hansen: I would like to add one more comment on an experience that was especially rewarding and stimulating in the Bureau of the Census. In 1955, at Bill Cochran's suggestion, we created a Panel of Statistical Consultants to serve as advisers on the statistical aspects of our work. This was a special panel in addition to the usual advisory committee structure. Bill Cochran served as chairman. Other members included Fred Stephan and Bill Madow for the full time period, and somewhat later Ivan Felligi from Statistics Canada, H. O. Hartley and others. All were exceedingly able, but we did not look to them as experts whose advice would simply be sought and followed. Instead, we operated on an interactive basis. We discussed specific issues or problems as well as all phases of total survey design for a particular survey, experiment or census. We received much useful advice; they also learned from us. We benefited, also, from being able to report that such a group had reviewed and advised on the various aspects of our program. We had close and continuing personal as well as official relationships with the panel members. It was a highly stimulating group from whom our work benefited greatly.

Just one remark, if I haven't made it clear, which is that what I've done has really been the work of a team: Bill Hurwitz and I, alone or with others. I'm not really a very effective performer alone, but I'm effective in developing ideas and in working with a team, stimulating them and getting them to develop ideas and then in selling those ideas. An important part is statistically formulating a problem that comes in. Someone says what he wants and you find out that you can formulate it in a useful way. But it's a team approach to solving. It has been all the way through my life.

Olkin: I think that's one of the interesting and remarkable contributions, in a sense. Some people work alone or in small groups: Fisher or Neyman or some other great statisticians. But I think one of the things that came out of the Census Bureau is indeed the fact that there was this contagious process that generated a team effort and really made sampling a successful mode.

Hansen: A lot of the people have come out of the census using this broad design approach. You think of it as sample design, but it's a lot more; it's total systems design, total census design or the design of something to solve a statistical problem.

I think the team approach in the census has been enormously effective.

Olkin: I know I speak for all the other statisticians. We thank you for that team approach and for doing as much as you did in the Census Bureau and for developing all the techniques that I think will have a profound effect for the future. It's been a pleasure having a chance to talk with you. Thank you.