

A Conversation with Joseph Waksberg

David Morganstein and David Marker

Abstract. Joseph Waksberg was born September 20, 1915, in Kielce, Poland; his family emigrated to the United States in 1921. Soon after graduating from the City University of New York (CUNY) in 1936, he moved to the Washington, D.C. area. He joined the Census Bureau in 1940, and stayed there for 33 years, retiring in 1973 as the Associate Director for Statistical Methods, Research, and Standards. Joe then joined Westat, a statistical research firm in suburban Maryland. He has continued to work at Westat for the last 26 years, serving as Chairman of the Board of Westat since 1990. From 1967 to 1997, he also served as a consultant to CBS and other TV networks for election night predictions. He has served the profession of statistics in many roles and received numerous awards, including the Department of Commerce Gold Medal and the Roger Herriot Memorial Award from the American Statistical Association. He has been active in the American Statistical Association serving on the Board of Directors as chairs of both the Survey Research Methods Section and the Social Statistics Section and on a number of committees. He has been president of the Washington Statistical Society and is currently an Associate Editor of *Survey Methodology*.

The following conversation took place in November 1998 in Rockville, Maryland. Besides Waksberg, the other participants were David Morganstein (DRM) and David Marker (DAM), both long-time colleagues of Joe Waksberg's.

DRM: Maybe we could begin by talking about some of your early life experiences as a survey statistician. Why did you decide to attend CUNY?

Waksberg: I decided to attend CUNY because it was a free college for New York residents, at least, for those who got quite good grades in high school. You have to remember this was during the depression of the 1930s. In most families, including mine, there was no money available for college, and scholarships were very rare. CUNY (at that time CCNY) was free, so I went there. I didn't go there to study statistics. At the time I entered I didn't even know there was such a subject. I was a mathematics major. When I graduated in 1936, it was the middle of the depression and things were really in a bad state. I took graduate courses at New York Univer-

sity in the evening. There wasn't very much work around New York. I apparently did well on a Federal exam for employment as a mathematician, and I received an offer to come to Washington to work for the government. When I first came to Washington, I worked a couple of years for the Navy Department as a mathematician. When that project was completed, a Census Bureau offer came up. I had taken one undergraduate course in mathematical probability as part of the math curriculum. I also had taken a course in statistics, although I'm not sure why. But, at that time, it was enough to qualify me to take the exam. It probably indicates something about the lack of sophistication of those creating the civil service tests and also about the vast expansion in statistical theory in the last 50 years. I got a couple of books, studied them, and apparently they were the right books. I think at least one of the authors of the books developed questions for the exam, and I did very well. Leslie Kish has reported that I got the highest grade in the nation on that exam. I'm not sure he's right, but I'm willing to go along with Leslie and not contradict him. But that's how I came to the Census Bureau in 1940. I remained at the Bureau for 33 years.

DAM: And who was there when you got there?

Waksberg: My first job was a job as a clerk. Those were the types of jobs being offered and

David A. Marker is Associate Director and Senior Statistician, and David Morganstein is Vice President, Westat, 1650 Research Blvd., Rockville, Maryland 20850-3129.



FIG. 1. *Joseph Waksberg, Chairman of the Board, Westat, 1998.*

that's the job I took. Other young college graduates came in as clerks also, people like Ben Tepping, who already had his Ph.D. in mathematics; Joe Steinberg, who had a Masters Degree in statistics; Ed Goldfield, Sam Greenhouse, Marvin Schneiderman, all came in as clerks at the same time I did and became distinguished statisticians. We first worked on operations related to the 1940 Census. There was a small research staff at that time with Morris Hansen in charge. Bill Hurwitz was his alter ego in many ways. The two of them worked so closely together it was impossible to give credit to either one separately. When World War II started, I worked on a project the Census Bureau set up to handle industrial production statistics needed for the war effort. When the war was over, I joined the mathematical statistics staff to work on preparation for the 1950 Census. The research was led by Morris Hansen and Bill Hurwitz. Joe Daly, who may have been the first, or certainly one of the first, to get a Ph.D. in mathematical statistics in the United States, was part of the staff. Ben Tepping, Joe Steinberg, Max Bershada and Marge Gurney, who also had a Ph.D., were also part of the staff.

In the late 1940s, Morris and Bill decided to add methodologists to the research staff and brought in Eli Marks and Leon Pritzker to work on evaluation

methods. The organization of the statistical staff at the Bureau was sort of a hybrid arrangement. Joe Steinberg led the work on sample design, sampling operations and related activities for the demographic studies. Jack Ogus and Ralph Woodruff had similar roles in the fields of manufacturing, and wholesale and retail trade, respectively. A central research group was directed by Bill Hurwitz. The presumed responsibilities were that the central group worked on long-term fundamental research, while the staff located in the subject areas handled their own statistical problems, including short-term research. The distinction between short-term research and basic research was not all that clear. Anyway, Hansen didn't care about that. He felt that whatever problems were important at that time needed to be addressed, and he assigned available staff, wherever they were located, to work on them. There was a lot of interchange among the mathematical statisticians, as well as a fluid and flexible procedure for getting things accomplished. I imagine management experts with a strong sense of what managers should be like would probably have been horrified by this kind of organization. But it worked at the Bureau, and, in fact, the Bureau still operates in essentially this way.

DAM: Where were you assigned?

Waksberg: I was in the Population Division for the most part, but was pulled out for two years when a new division was being set up for construction statistics. Statistics related to construction activities compiled by the Department of Commerce and the Bureau of Labor Statistics were taken over by the Bureau, and I was assigned to this new division developing these statistics from about 1959 to 1962. In 1962, Joe Steinberg, who was head of the sampling staff working on populations statistics and who brought me into the research staff, left, and I was asked to go back to the Population Division. Around that time, the statistics group was organized as a separate division with me in charge.

DRM: I'd like to return to the point you were making about the central group whose primary focus was on long-term research and the separate groups that were tied a bit more to the specifics of the surveys. You said that it worked very well at Census. Were there any particular mechanisms, some things that occurred that kept the communication strong between those people?

Waksberg: Oh yes, the statistical people in the divisions had dual reporting responsibilities. Administratively, they reported to the Chief of the Division; technically, they reported to Hansen and Hurwitz. In addition, there were weekly meetings for the central staff and the heads of the statistical staff from the divisions.

The weekly meetings were devoted to two kinds of issues. One was the statistical problems a division might be facing. The second one was the dissemination of new research methods. These meetings were a way of keeping the staff informed and up-to-date. There was actually a lot of interaction among the statisticians because they were personal friends as well as professional colleagues. The interaction took place both at these meetings and informal lunches among members of the staff.

DAM: That reminds me, I seem to remember Morris mentioning the fact that Census employees tended to use car pools to commute to the Bureau. Were you part of that crew as well?

Waksberg: Yes. The Census Bureau was out of the city with awkward and complicated bus transportation, and the only way to get there was to drive, resulting in a lot of car pools. Morris was in a car pool for many years. When I moved to Bethesda, one of the first things I did was look around for a car pool. I didn't car pool with Morris, and Bill Hurwitz was not part of his pool. Morris's car pool included Howard Grieves, who was an associate director at that time, and Max Conklin, who was head of the Manufacturing Statistics Division. I suppose they talked about work, argued, and made a lot of decisions in the car pool. Most of the other car pools were not at that same level of decision making.

DAM: One name you didn't mention was Ed Deming. Was he involved in what you were doing?

Waksberg: Not very much. He was at the Bureau during the 1940s, but left shortly after I joined the statistical staff. He may have had a stronger role earlier. He was at the Bureau and participated in planning the sampling aspects of the 1940 Census. While he was at the Bureau, Deming also developed some imputation methods. After he left the Bureau, he joined the Bureau of the Budget, what is now called OMB. So, he still had a lot of interaction with the Bureau, but no direct responsibilities, at least none I was aware of.

DRM: There were a lot of new young people who came to the Census at about the same period. As I understand it, in a matter of a few years, there were some pretty significant changes in the procedures for carrying out the census. What were some of the most important changes or innovations that modified the procedures that had been carried out for a couple of decades?

Waksberg: Let me divide my answer into two parts: the census methods and planning, and the periodic surveys. The issues are quite different. Let me address the census issues first. After the 1940 Census, there were a few studies on completeness of coverage that came about by accident. Males

between the ages of 18 and 45 had to register for selective service. The selective service organization had made an estimate from the 1940 Census of how many registrations to expect. They discovered that a lot more men registered than had been counted in the Census of 1940. It became quite clear that the census had missed a significant number of persons.

This led to a major evaluation program for the 1950 Census, both for coverage and content. Methodologists, principally Eli Marks and Leon Pritzker, were brought in to work on this evaluation. They developed different roles over the years but that's what they did initially. The important innovations in 1950 were evaluation programs and experimental programs that tested alternative methods of conducting a census. One method was a mail census. Another one also used self-enumeration, but with interviewer pickup. By the time I joined the statistical staff, a lot of this planning had gone on, and I wasn't too involved in this particular activity for the 1950 Census. I was involved in the estimation procedure and in special studies.

I would say there were no major changes in the 1950 Census, but the framework had been established for two things: (1) a method of determining the kinds of changes in census procedures that were necessary, and (2) what kind of research programs were needed to plan for the future census.

The 1950 evaluation and experimental programs revealed two major problems: the first related to undercoverage. For the first time, the large differential between black and white coverage was uncovered. At that time, Hispanics were considered a small part of the population and were not thought of as affecting the statistics appreciably. The difference between black and white coverage, however, stood out. The second item concerned one of the experiments in the 1950 Census.

Woven into the 1950 Census was an interviewer variance study in which randomized assignments were provided to interviewers. We were able to measure the total variance between the sample areas and within-area variance to measure the effect of interviewers. A large number of geographic segments were selected. Two interviewers had randomized assignments within each segment and the results showed a high level of interviewer variance for many items. Sometime in the early 1950s, an important paper was produced on the accuracy of the census with and without sampling. It described the response and interviewer variance with a 100 percent census, and what would be the total variance if you added sampling. The between-interviewer variance so dominated the total, it became obvious that the Census Bureau was wasting a lot of money by obtaining most of its data from the

whole population. There is a lesson there for the current congressional controversy about the role of sampling in censuses.

On the basis of these findings, Morris was able to convince the Bureau staff and the administration that they would do better to make two changes. One was to use sampling and the second was to take the money saved by sampling and put it into other aspects of quality control—procedures that we think of as typical quality control today, such as better training and supervision. That was very hard in a Census environment where 250,000 people are hired to work for about one month and the supervising staff is hired to work about two months. Supervisors were almost as unqualified as the interviewers. Real quality control was very hard to introduce into the census. But, at least with more complete training and more emphasis on supervision, these were steps in the right direction.

The second major innovation was to take control of the interviewing process from the interviewer and give it to the respondent. We started looking at the kinds of issues discussed in the training material and found that the interviewers were told over and over again, “Don’t invent a question,” and “Don’t embellish it.” “Ask each question just as it was worded.” It became clear that if you were concerned about the interviewer not using the correct words, then why not give the respondent the questionnaire to fill out? As a result, in 1960, there was a mail precensus operation. The respondents were sent advance copies of the census questionnaire to fill out and have ready for the interviewer’s visit. Not all households filled them out in advance, of course, but a substantial majority did. The interviewer effect was much reduced.

By 1970 other major changes were made. The mail operation introduced in 1960 worked well, and it seemed sensible to go one step further. Why bother to have the interviewer pick up a piece of paper and have to recopy the results? And so, the mail census was introduced, but not in the total United States. There were concerns over how to prepare good mailing lists in rural areas. The statistical staff, Morris, in particular, fought very hard to extend the mail census as much as possible. We thought we could solve most of the problems of the mail census, and our experience had indicated that better mail response rates were obtained in rural areas than in central cities. But the field division fought very strongly against a mail survey in rural areas because of poor mailing addresses. They may have been justified in whether they could carry out this operation with the resulting problems, and the Director sided with the field division. In retrospect, it was probably better that way, to avoid introduc-

ing revolutionary changes completely and so suddenly.

DRM: What sort of sources did you use for addresses? You were hardly automated at that time.

Waksberg: We weren’t automated at all. The Bureau started by purchasing a mailing list from Donnelley Corporation. This was 1970. I don’t know exactly what they did in 1990. In 1980, they were still working with the same system. They started with the Donnelley list, and then they had the post office verify the lists. At that time, the post office had the mailing addresses in little slots. Dummy mailing pieces were prepared for all addresses on the Donnelley list and the postal carriers put the mail into these little slots and checked for missing addresses, filling out a card for each missing address. This was repeated a second time. We did special checks in certain areas that were known to be difficult, focusing on areas with a lot of converted buildings without apartment numbers. Typically, such buildings had been mansions at one time and were converted into apartments, with no apartment numbers designated. We sent interviewers to list apartment numbers or other ways of identifying the unit. I believe this system is still the basis of what is used today, although, of course, it is much more automated.

In the 1970 Census, there were a few instances that stand out in my memory. In the original planning and pretests, we thought that a mail census would be impractical in areas with suspected low literacy rates. We decided we’d have the conventional census in these areas, with interviewers knocking on doors. We carried out a pretest in one city. Not surprisingly, such areas tended to be minority areas. The interviewers started knocking on doors, frequently accompanied by professional staff. At the same time there was a lot of publicity that the census was being pretested by mail. People were surprised when we knocked on their doors. A common reaction was “How come it’s not by mail? Do you think we’re too stupid to answer the mail questionnaire?” We dropped plans for an interviewer-conducted census in such areas.

I’d like to go back to some other major issues that arose in the 1970 Census, in particular, the issue of population coverage. I mentioned before that after the 1940 and 1950 Censuses, there were evaluations of quality. In 1960, many of the changes in procedures were for the purpose of improving coverage. The assumption at the time was that undercoverage was due to poor interviewer performance, so the changes concentrated on more and improved training and on closer supervision. The evaluation of the 1960 Census showed that there was an improvement, but there was still a long way to go. The

difference between the minority and nonminority coverage was still very great. Obviously, just blaming interviewers didn't do much good. Something fundamentally different had to be done. This was one of the main reasons for the mail census in 1970, because that would permit tighter control of household coverage.

There were additional research studies on how to improve undercoverage. We knew that part of the undercoverage resulted from concerns by people who didn't want their existence known and thought census data would be shared with other government agencies. So, we did a pretest stressing the confidentiality of the census. The pretest revealed that additional assurances on privacy didn't do a thing for undercoverage. Another program involved publicity, particularly in minority areas, where we talked about the census in churches and other places where people tended to congregate. A 15-million-dollar budget was set aside for this new program, which at that time was a lot of money. Right before the 1970 Census, I think it was 1969, Congress cut the budget for the census by 25 million dollars. Eliminating a new program being proposed for 15 million dollars seemed like the logical place to achieve most of the budget cut. I thought this was a mistake and proposed to the Associate Director for Demographic Studies, Conrad Taeuber, to retain the program, and instead cut the long-form sample from 25 percent to 20 percent. In some sense this was a continuation of looking at the total error of the system. I guess I must have been persuasive because my recommendation was followed. The coverage of the 1970 Census was much better than in 1960, but still far from the level we were striving for.

DRM: We've been covering some of the major changes that occurred in the census. Why don't we go into modifications in the methodology used in household surveys?

Waksberg: Although important changes were made in some of the economic surveys, in particular, going from area to list samples for the monthly retail trade survey, I wasn't very much involved in them. Thus, let me talk about the demographic surveys. The Census Bureau took over the Current Population Survey (CPS) around 1940. During the depression of the 1930s, the Works Progress Administration (WPA) was established to provide jobs for unemployed persons. The research staff of the WPA designed and conducted a household survey to measure unemployment. Although the statisticians at WPA were familiar with sampling theory as it existed in the 1930s, probability methods were used only for the first stage of selection. When the Census Bureau was going to take it over, Morris and

Bill Hurwitz really went to work on a theoretical basis for a full probability sample. They developed what is now probability proportionate to size (PPS) sampling, with unequal probabilities. The desire to improve ongoing methods in a practical situation was the impetus for a lot of the work they did which is now considered fundamental. By the time I took over the CPS, in the early 1960s, much of the structure was established, that is, PPS multistage sampling, using area samples. However, it became clear that area sampling had major problems. At that time, migration to the suburbs was in full swing and the census data used for measures of size of the segments were quickly becoming out of date. A few years after the census, I remember a segment was reported as having six farmhouses. When the interviewers got there, it was the site of a major suburban housing development. If unbiased procedures had been used, that segment would have counted for one third of the black population of the United States!

DAM: Because there were just so many houses there?

Waksberg: Yes. Within a few years after the census, the area had become a very large community with mainly black residents. In addition, many of the maps that were used for area sampling were completely out of date. Interviewers found roads and streets that didn't appear on maps. The boundaries of the segments of the maps were sometimes streets that had been torn down or were unrecognizable. What was inside the boundary and what was outside was impossible to determine. The Census Bureau wrestled with that issue without finding a satisfactory procedure.

I mentioned earlier that I spent two years working on construction statistics. One of the things I did at that time was to establish a system of tracking building permits. It seemed to me that this was a clue to solving the problem of poor measures of size. I proposed that we look at abandoning the use of area samples, and instead use a list sample consisting of addresses reported in the last census and supplemented by a sample of building permits. When you propose something new, people think of all kinds of objections and some of the statistical and field staff argued against it, but after I convinced Bill and Morris, we went ahead with list sampling and, essentially, it is still used today. A variant of this method, combining area samples and building permits is used at Westat.

In the 1960s, we carried out major research in connection with CPS, some of it on alternate rotation and estimation patterns; for example, can you get a better measurement of change by asking not only about the current month, but also the previous

month? We also investigated the problems of having a single respondent for an entire household. These investigations did not disclose any improved methods.

DAM: Moving beyond the CPS, what other major surveys were changed?

Waksberg: Let me mention an interesting aspect of another survey. In the early 1960s, we started the American Household Survey (AHS), originally called the Annual Housing Survey. It was carried out at the request of the U.S. Department of Housing and Urban Development (HUD), which wanted to measure changes in the housing stock. Because housing is more or less a local issue, the problems vary among geographic areas. HUD was therefore interested in getting data for the individual areas as well as national statistics. The original plan developed by the housing specialists was to conduct a national survey and surveys in about 14 large metropolitan areas. HUD wanted typical areas that reflected varying conditions and wanted data annually. When I began thinking of the sample design, it seemed clear to me that housing characteristics don't change strikingly over the course of a year. Also, some earlier work on the quality of reporting on some key housing items indicated serious problems arising from the difficulty of imposing clear definitions. A moderate size sample did not appear to be a good vehicle for measuring changes from one year to the next. I proposed to increase the number of areas by a factor of 4, and rotate the areas from year to year in a four-year cycle. Changes over four years were likely to be large enough to permit measurement with a reasonable level of accuracy and the plan would provide statistics on four times as many areas. The proposal was adopted.

It's interesting that this problem keeps coming up over and over again. A few years ago, Westat prepared a proposal for the National Household Survey of Drug Abuse (NHSDA). The RFP asked for annual surveys in a small number of areas. If there are problems of accuracy of response for housing surveys, my feeling was there are much more serious problems in trying to get people to admit involvement with drugs. Also, year-to-year changes based on a modest sample are a problem. So, I proposed that they do exactly the same thing we had implemented at the Bureau over 30 years ago for the housing surveys. The sample should be expanded to cover more areas, with the areas rotated over a four- or five-year cycle. Although Westat didn't win the award of the survey, I was informed later that the idea was picked up, and the survey is being carried out using this method.

DRM: Innovation?

Waksberg: I think it demonstrates something important for a sampling statistician to be aware of, which is to think not only about the specific questions that are asked, but the broader aspects of these questions: whether the questions make sense and can be solved, or whether they should be modified or changed. This is how I've tried to have people with whom I work think about the issues: Here's a question, how do you respond to this specific question? Is it the right question? What statistics will you get by a narrow interpretation of the question, and is there a better way to proceed?

DRM: Our role is more than just sample size and power and variances.

Waksberg: Exactly.

DAM: Let me ask you another question. During the same time period, the 1960s, you were involved with the issue of predicting election results. Can you tell us how that came to be?

Waksberg: Well you might say this election, 1998, is the thirtieth anniversary of my having been involved in the elections, but it's also the first election that I was not involved in the predictions. Let me first define your question. Definitions are very important.

DAM: That explains why they had all the trouble predicting what would happen. You weren't there.

Waksberg: Not at all. And that relates to the first point I want to make, definition of prediction. With some minor exceptions, I have never been involved in preelection predictions. What I've been involved in is predictions that are made on election night. These predictions were originally based on the official tallies in a sample of precincts after the precincts closed, but then evolved into exit polls as well.

My involvement started in 1967. With the availability of large computers in the 1960s, CBS and some of the other networks started doing this kind of election prediction. Their system worked pretty well in the 1966 election, but it was a disaster in a few states. An example of the problem occurred in the governor's race in Maryland. In 1966, a Republican, Spiro T. Agnew, was running against the Democrat, George P. Mahoney, who had made it through the primaries. Mahoney was a right-wing Southern Democrat who had been opposed by two liberal Democrats who split the vote in the primary. Open housing was a major issue at that time (passing laws to outlaw discrimination in housing so neighborhoods would be open to all). Mahoney was against open housing, his slogan being "Your home is your castle." What happened was that the



FIG. 2. Joe Waksberg with CBS staff in late 1970s (back row: Walter Cronkite, Charles West, Marty Plissner; front row: Warren Mitofsky, Joe Waksberg, and Murray Edelman).

precincts that were heavily black, which typically voted Democratic, went Republican. The same thing happened in the precincts with white liberal Democrats, who also voted for Spiro Agnew. Not much was known about him in those days.

DAM: He was more liberal in 1966 than 1968?

Waksberg: No. He just kept quiet about these issues. What had happened was that the staff at CBS was not aware of the changes in voting patterns in Maryland. Part of the quality control system was to compare the votes in each precinct to its past voting behavior. All of a sudden they got a bunch of precincts not matching their historical patterns, and decided either something was wrong with the data, or they had some outliers. As a result, they threw out all of these precincts.

DRM: Probably key indicators.

Waksberg: Yes, and then they ended up predicting Mahoney to win by a wide margin; of course, he lost. At that point, CBS decided they needed better statistical help, so someone gave them my name, and they contacted me. This was 1967; I was in-

involved in the planning of the 1970 Census, and I didn't want to leave the Census Bureau. Warren Mitofsky, who had been working for me at the Census Bureau, took the job and asked me to be a consultant on the project. He also brought in another person who had worked with him in the Census Bureau as his deputy, Murray Edelman. The first year we spent working on techniques and, having been trained by Hansen and Hurwitz, we said they had to introduce probability sampling and stick to it. Quality control was still necessary to detect data errors, but before rejecting data, you had to find out whether they reflected errors, flukes, or indicated something was happening. Also, if there were major problems, you should take a conservative position, and take this into account by increasing your estimate of the margin of error in the system.

In 1967, we first introduced probability sampling in a few places. It worked quite nicely, and it has been used ever since. At that time, exit polls were not used. People were stationed at the selected

precincts, and when the polls closed at the end of the day, the backs of the machines were opened or the ballots were counted by hand. When the counts became available they were called in and entered into a computer in New York. In a sense, Warren developed the first CATI system, because when interviewers called in, the telephone operators at CBS keyed the data directly into the computers so you got instant tabulations.

Later on, exit polls were introduced. I believe Warren was the first to use them. The predictions are currently based on three levels of information. The first decision to call the election as Republican or Democratic or too close to call is based on exit polls. Moderately small samples are used because this is an expensive operation. For those elections that are too close to call, the next level of information uses both information from the exit polls, and the totals at the end of the day from a larger sample of precincts. If it is still considered too close to call, the call is made on the basis of the official voting results (or close to final). How close to the final count depended on how close the election seems to be. With 50.5 percent versus 49.5 percent, a call is not made until virtually every last vote is in, partially to avoid surprises and partially because the earliest counts might not reflect the last handful of voters.

Congress's arguments with the Census Bureau about accuracy of sampling election predictions included concerns about the dangers of sampling. But the examples cited by Congress used preelection polls, and people can change their mind before voting. The election night predictions use actual voting data. There may be some problems in precinct-level nonresponse, but no problems from incorrect reporting. This is almost a textbook case of checking the sampling. The last time I looked at the numbers, Warren Mitofsky and Murray Edelman had made approximately 2,000 predictions covering elections: state-by-state presidential, governors, senators and some other state-level elections. The samples aren't big enough for House of Representatives. There have been over 2,000 state predictions with less than 10 cases where the wrong call was made. If you look at what it says in terms of the confidence interval, we've probably been too conservative. But at the same time, it is a striking confirmation of the power of sampling. The issues of the census are not really about sampling, but of adjustment of census counts.

DRM: One more topic may be related to exit polling and broadcasters. As the broadcast system became more commonplace, as the networks began to compete with each other trying to get the right

call, the earliest call, there was also a bit of a national debate over possible influence of these early results being made available on especially, say, presidential election results. I know that you had some thoughts and reactions about that whole discussion.

Waksberg: There are some data available on the issue, not a lot, but some. When the issue came up, some congressmen wanted to hold the networks back from reporting results of presidential elections until voting was over in every state. California closes at 11:00 EST. If you waited for Alaska and Hawaii, the evening would be over. Early calls would then be unnecessary. The networks' official answer was basically that such a law would interfere with free speech. Congress found it very difficult to argue against that. But at the same time, there was a small amount of research carried out by CBS. I don't know about the other networks, but I am not aware of any evidence that early results in some states affected voters in other states. For example, I looked at the turnout of registered voters in California and other western states versus the East Coast. At the time I looked at this, it was about 10 years ago, California and Washington had somewhat higher turnouts than the East Coast. The data did not support the idea that when people in the West see the results are already clear they won't or don't vote.

DRM: If anything, it works the other way; the results are coming in and it's not the way I want it, so I'd better get out and vote.

Waksberg: Possibly. I don't know if it works that way either. Mostly, I think, there may be an exaggeration of the impact that the media have on how people act as pertains to voting. Warren did get some money to do some intensive studies on the issue. As a matter of fact, Westat had a small contract with Warren about 10 years ago to do some experimentation. We did some pretesting and decided that the results would not be definitive, so the issue was dropped. But, there is no evidence that people hear the early returns and say, "Oh, I'm on my way to the polls, but now I've changed my mind. I'm not going to vote." It just doesn't happen that way.

DAM: Let's switch to your second career. You started working at the Census in 1940, and you retired in 1973, at which point you were the Associate Director for Statistical Methods, Research and Standards.

Waksberg: I'm not sure the title went in that order, but that's essentially what it covered.

DAM: At that point you decided to retire and come to Westat, where you've been for 25 years.

Could you tell us why you chose to come to Westat and what it was like when you first got here?

Waksberg: It's like the people say when asked why they climb a mountain: "Because it is there." Basically, when I decided to leave the Bureau, I had a number of offers. Some were from universities and some from other statistical research organizations. Westat was one of them, of course. I had lunch with Morris Hansen and Ed Bryant (one of Westat's founders), who I didn't know until then. I decided to come to Westat for essentially two reasons. One, I lived in Bethesda and our roots were in Washington, and most of my friends are here in Washington. My children grew up here. By 1973, they were no longer living at home, but, at the same time, they still thought of Bethesda as their home. It just seemed better for me to stay in this area. The second reason was the fact that Morris Hansen was at Westat. He was such a stimulating figure and working with him again was very attractive.

DAM: Who was here at that time?

Waksberg: At that time Westat's whole staff was probably under 100 people—Joe Hunt was here; he was Executive Vice President. Ed Bryant and Morris were here, as were Tom McKenna and

Steve Dietz, who are the only remaining staff at the executive level. A few other staff members are still at Westat, including David Wright and my son, Mark.

DRM: There are some rumors that Mark is the one who actually got you your job at Westat.

Waksberg: That's what he tells people. It's a family joke!

DAM: Well, in particular, tell us about the people who used to be at the Bureau who were here as consultants in the statistical group. I was wondering if they were here before you got here, or did you bring in Ben Tepping, Hal Nisselson, Walter Perkins or Bob Hanson?

Waksberg: Well, it was a combination of the two. Ben Tepping retired the same day I did. I think Morris had already spoken to him. Ben had always worked closely with Morris. I think I had talked to Ben about coming to work with us. Eli Marks was here when I arrived. Some of the other Census people who I brought in were Hal Nisselson, Jack Ogus, Bob Hanson and Dan Levine. Actually, I think outsiders exaggerate the role the Census people played at Westat. For a long time, Westat was called Census West, or something like that. In practice, the number of Census Bureau staff who



FIG. 3. Retirement from Bureau of Census (1973): Joe Waksberg with senior staff at the Census Bureau (left to right: Tom Jabine, Morton Boisen, Robert Voight, Joe Waksberg, Ben Tepping, Bob Hanson and Charlie Jones).



FIG. 4. Joe Waksberg with staff at Westat in early 1980s (left to right: David Morganstein, Joe Waksberg, Morris Hansen, Hal Nisselson).

became direct employees or consultants was rather low.

Let me just add to that, what has been more important to Westat was the *experience* that people like Morris, Ben Tepping, and the others brought to Westat that helped to develop Westat, not so much the people.

DRM: I don't think you were overjoyed at the point, I remember, when I made a comment. I looked around the room and I said, "My goodness, there are two centuries of experience at the Census Bureau, sitting here in this room."

Waksberg: Well no, I didn't mind.

DAM: How would you summarize some of the major accomplishments during your time?

Waksberg: I think that's something more appropriate for outsiders to say than for me.

DAM: Well, the things you are most proud of then? Please include some from both Census and Westat.

Waksberg: What am I most proud of? I don't know if you want to talk about general philosophical issues or specific methodologies or techniques. Let me talk about the latter first, because that's easier. I suppose in some sense, if you look at the published literature, my paper that is most referenced is the Mitofsky–Waksberg random digit dialing method (Waksberg, 1978). It has an impressive number of citations in the literature, and I am

pleased with that. I think that's a major accomplishment, even though it's no longer being used, or not much used, because of technological developments.

Let me talk about that for a minute. I mentioned earlier that I was not involved in the preelection polling business. However, Warren occasionally asked me to help out on other problems, and one of the issues was telephone interviewing for CBS. He wanted to do some research on a method that he had developed intuitively. It seemed like a sensible method, and he asked me to look into the mathematical development. I found that it was both an unbiased and an efficient procedure. My contributions were to provide the mathematical basis and show under what conditions it would work and when it wasn't efficient. That's what resulted in the paper.

At the Census Bureau, I think I did innovative work on oversampling for rare populations, the introduction of list sampling in demographic surveys and estimation procedures. The 1970 Census was the first census where the computer programs included automatic estimation of variance at the same time as the regular census tabulations.

When I came to Westat, one of my major goals was trying to get Westat statisticians to think in terms of the overall quality, as well as efficient sample designs. For example, to recognize the con-

ditions under which you could make some compromises with strict probability sampling. I helped introduce to the staff random digit dialing, which obviously excluded nontelephone households. Similarly, under what conditions can you afford to completely ignore the bias, or use weighting or imputation methods to adjust for that with reasonable confidence? When are you not doing too much damage, and under what conditions should you be worried about these biases?

Another accomplishment, more so at Westat than at the Census Bureau, was in working with junior statisticians, helping to train them. Not through a formal training program, but guidance in how to think about these problems. This is both an accomplishment and a source of a great sense of satisfaction.

DAM: You commented that you and Morris felt that part of your job here was to pass on the large store of knowledge you have.

Waksberg: I think that's very important. Passing on knowledge comes in a number of ways: one is by teaching, another by writing papers, and the third is just personal interaction in a way that produces useful effects.

DRM: We haven't mentioned anything about your work in small-area estimation. This is another topic where I think you made some important contributions. An issue that parallels this topic is where and when are models appropriate. I think it may have been George Box who said, "All models are wrong, but some are useful."

Waksberg: Who am I to argue with Box! Let me mention how I got involved in small-area estimation to the extent that I did have an involvement. In this and in most of my other work, I tried to develop theory to solve a specific real problem. My preference has always been to work on research that is directed to a specific problem rather than to sit quietly in a corner of a library looking through literature trying to think up problems to solve, although I don't intend to denigrate achievements by statisticians who prefer to work in that way.

My interest in small-area estimation arose from a specific problem that occurred in the conduct of the 1970 Census. As I mentioned earlier, the 1970 Census was carried out by mail in two-thirds of the country, with interviewers collecting the required information in the remaining, mostly rural or small town, areas. The early census returns in the non-mail areas, probably in May, showed more vacant residential units than expected, and it seemed likely that errors were being made. We decided that this needed investigation and quickly developed a program to check the accuracy of the classification of

units reported as vacant. It took quite an effort to design and carry out a new and unplanned survey in a few weeks, but we wanted the results back in time to modify the population counts if that seemed advisable. The study was completed in August, and indicated that about 10 percent of the "vacant" units were really occupied. My instincts were to adjust the census by transforming every tenth vacant to an occupied housing unit, using imputed neighbor information. However, I thought that assurances were necessary that this would improve the population counts, particularly for subnational areas like states and large cities.

I developed some rudimentary model-based small-area estimation theory, and my staff carried out the necessary computations of sampling and estimation variances. The conclusion was that areas under 50,000 population were more likely to be hurt than improved, and areas over 50,000 were likely to be improved. Improvements were quite clear for areas as large as states. The decision, then, depended on one's priorities, accuracy of data for large or for small areas. To me the answer was very obvious, because reapportionment of Congress only required state data. But I suppose someone more concerned about block or tract statistics would come to a different conclusion. I proposed that we adjust the data for the census, and the Director of the Bureau agreed. I subsequently did more work on the theory, together with Maria Gonzalez. We wrote a paper on more general features (Gonzalez and Waksberg, 1973). The question of priorities in terms of large areas versus small communities is basic to the issue of adjusting the census based on an evaluation study. There are some prominent statisticians who are against the adjustment. Most survey statisticians working with census data, although not all, are for adjustment. In my opinion, the few who are opposed are choosing the wrong priorities. Not that their mathematics and understanding are wrong, but their priorities are wrong.

DAM: The paper you mentioned, Gonzalez and Waksberg, was presented, I believe, at the first meeting of the International Association of Survey Statisticians in Vienna, Austria.

Waksberg: This is right, 1973. After that, I always wanted to go back and do more work on the topic, but once I got involved with Westat, I never had time.

DAM: Well, you helped me in my research on the topic (Marker and Waksberg, 1994). But that's a nice lead-in to another area of involvement that we haven't touched on, and that's your involvement with professional societies: Washington Statistical Society, ASA, the ISI, and so on.

Waksberg: I think it's important for professional statisticians to be involved in such societies from the point of view of passing on information to the next generation. I spoke earlier about the importance of this. A second reason is to learn from others about advances that are going on. Third, it's a very collegial thing to do. I've developed many friends that way. I've been a member of the American Statistical Association since about 1950. I've been on committees, served on the Board of Directors, was Chair of two Sections, and President of Washington Statistical Society, and so on. The International Association of Survey Statisticians was organized in 1973. I was a member of the first council of the IASS. I think it's an important way of interacting with other people, giving you an opportunity to both learn and teach.

DAM: You've contributed to the methodology of surveying rare populations: Hispanic communities, Jewish communities, maybe some others that I can't think of. Does it present some interesting and unique challenges that are different from other large-scale surveys?

Waksberg: Well, each subarea of statistics presents its own issues. It's particularly important to those government surveys that need to get statistics about selected subgroups of the population. Actually, my first paper on this topic was in 1973, at the ASA annual meeting. I had a paper on oversampling rare populations (Waksberg, 1973). It only dealt with some simple situations, but it was a first attempt to look at conditions under which it pays to oversample and what are the optimum oversampling rates. The issue also came up from a specific problem that the Census Bureau faced.

The National Center for Health Statistics was planning the first cycle of NHANES. The agency wanted to oversample minorities and the low-income population. I found this an interesting problem and started looking into the design of a sensible oversampling program. Many other surveys were affected by oversampling strategies, so I developed a more general theory and reported it at the 1973 ASA meetings. One of the things that surprised most people was that conservative oversampling rates help you the most. NCHS didn't quite accept my recommendation and oversampled targeted groups at a rate of 8:1 instead of 2:1, which I thought they should use. Of course, it didn't work. Or, I should say it worked getting data, but there was a large increase in the variances for the total population and only marginal gains for the specific population they were oversampling. That's usually an important issue, how to reconcile the oversampling for the specific rare population with

the effect on other parts of the population. The issue keeps coming up. Just a few years ago, we did some analysis of 1990 Census data examining the optimum oversampling rates when census tracts or blocks are stratified by proportion of minorities. Dave Judkins of Westat and Jim Massey of NCHS worked on that with me, and we published an article in *Survey Methodology* (Waksberg, Judkins and Massey, 1997). It sounds counter-intuitive to say "Don't put all of your money into the blocks that are very heavily black or Hispanic." However, a fair proportion of blacks and Hispanics live in areas that are more integrated. The variance that you get from undersampling the integrated areas will seriously increase variances on the total population and even reduce the value of oversampling for blacks and Hispanics. The situation is even worse when the targeted group is low-income households.

DRM: It seems to me your contributions are to balance principles and theory, while making sure you examine things in detail. In Hispanic HANES, there was a coverage issue. The best design excluded 80 percent of the geographic area of the United States. The optimal design was restricted to the strata that had high concentrations, as well as the majority of Hispanics.

Waksberg: The Hispanic HANES presented an interesting choice of two alternatives. Let me first describe Hispanic HANES. NCHS wanted to have separate samples of three Hispanic groups: Mexican Americans, Cuban Americans and Puerto Ricans. When they started looking at the cost of national samples that would include representation of the few Mexican Americans in states like Montana and Wisconsin, it was obvious the cost would be prohibitive, and they were right. So, they decided to restrict this sample to areas that had moderate-to-high concentrations of Hispanics, and the rules they had were sensible ones. At that point they came to us, described the situation, and we planned the survey. It covered about 80 or 85 percent of the designated Hispanics. NCHS's first inclination was to publish the results, with a statement that the data only reflected the situation for Hispanics living in areas with high concentrations.

One of the comments I made earlier was the importance of examining not only what you are asked, but also what you think the analyst has in mind. My reaction was that the data would be used as a representation of all Hispanics in the regions of the U.S. covered by the survey rather than in places with high concentrations of Hispanics. I didn't disagree with the proposal to restrict the sample. But once the data became available, it

made sense to produce the best possible statistics for the covered areas as a whole. I proposed to use census data on Hispanics in the areas included in the sampling frame and in the rest of the covered areas as the basis of weighting procedures that adjusted for the excluded Mexicans, Puerto Ricans and Cubans. NCHS concurred with my recommendation.

I think this example illustrates two general principles. One is to think in terms of what is best for the program, not just to answer the specific questions that were asked. Secondly, in fact, there is a role, at least partially, for nonprobability methods. Under some circumstances you just cannot afford to pay for a completely unbiased probability sample. You will need to spend most of your money screening millions of households to get adequate representation of the rare population—in this case, Hispanics. The cost of doing that with the budget you have will permit only a small sample. I think it's a good example of the fact that you have to be somewhat flexible in what you consider acceptable and how best to meet the needs for the study.

DAM: Let me ask an overview question. It is now 58 years after you joined the Census Bureau and you are still actively thinking about these issues. I'm not sure how to word the question, but what has kept you working so long or what do you think the secrets are to staying involved, active professionally, for so long?

Waksberg: I'm not sure I have any secrets, but I can tell you there is at least one fundamental requirement, having pretty good health. I'm perfectly serious about that. If my health weren't good, I just wouldn't be able to do what I do, so that's a necessary but not a sufficient condition. There is no simple answer to the question of why I'm still at work. There are a complex of reasons as with any decision that human beings make. One is that I find it intellectually stimulating to work on these problems, and I enjoy them. Secondly, the problems are ones that are important to the welfare of the nation. I don't think I would have stayed this long if they weren't. Third, I like the interaction with the younger statisticians in both passing on information and learning from them. Notice I haven't said "the money." It's not because I discount the money; the money is nice to have, but it's basically not what's kept me at it. As I said in the ending to my Hansen lecture (Waksberg, 1998), I've really been fortunate. Almost by accident, I strayed into statistics. I found something I enjoy doing. I've met so many people and made friends for life. And, I hope other statisticians get the same satisfaction out of their work as I've gotten out of mine.

DRM: I'm not going to let you end yet. I think I remember hearing you say that you don't mind making predictions for the next 5 minutes or 50 years from now. But you don't want to make predictions about events 5 to 10 years from now. But if you were to make a prediction about the next 5 or 10 years do you see the Internet, for example, as something that might cause a new paradigm shift to the way surveys have been conducted in the past?

Waksberg: I have not been heavily involved in automation, although I might say I was one of the first persons in the Census Bureau to work with computers. I helped develop the first replication variance estimation program. This was for CPS. I also set up the basis for the weighting and variance estimation system for the 1970 Census. I've frequently regretted not being more involved, but I've found that I've enjoyed working on other statistical issues more.

As far as the issue of surveys being done on the Internet, I think it's very likely, but I think the prospects are in the more distant future than some of the enthusiasts believe. Let me repeat a remark I made in testifying before Congress on the possible use of administrative data for a census instead of the current system. I said administrative census is the wave of the future and is always likely to remain in the future. It sounds good until you look at the nitty-gritty of what has to be done, what are the issues and what are the problems. You say, "Oh well, we will have to solve this later. We're not going to do it this next census; we'll do it the following one." I suppose at some point in time, maybe there will be an administrative records census in the United States, and I suppose at some point in time, maybe reliable complex surveys will be done over the Internet, under controlled conditions. But I suspect it's going to be quite a way down the road. I'm not really close enough to be considered a spokesman for this.

DRM: When you were at Census you may have been involved somewhat in the issues about how data are assembled, made available to the public, and the impact on the public dialogue as well as information just generally available to the community. Is that a place where maybe this new electronic world of the Internet is likely to play a role?

Waksberg: It's a great place. Nowadays, if you want census data, you don't go to the volumes, you go to the Census Web pages, or you buy a CD. So I think that's the precedent for the future. Automation of the data collection will lag far behind data distribution or data processing. I'm referring here to population surveys. I wouldn't be surprised to

see rapid conversion of data collection techniques for business surveys to methods that rely to a greater extent on the Internet.

DRM: I think we've pretty much covered all the topics we wanted to discuss with you. I thank you very much.

Waksberg: It was a pleasure.

REFERENCES

- GONZALEZ, M. E. and WAKSBERG, J. (1973). Estimation of the error of synthetic estimates. Presented at first meeting of the International Association of Survey Statisticians, Vienna, Austria.
- MARKER, D. and WAKSBERG, J. (1994). Small area estimation for the U.S. National Health Interview Survey. *Statistics in Transition* **1** 747-768.
- WAKSBERG, J. (1973). The effect of stratification with differential sampling rates on attributes of subsets of the population. In *Proceedings of the Social Statistics Section*, 429-434. Amer. Statist. Assoc., Alexandria, VA.
- WAKSBERG, J. (1978). Sampling methods for random digit dialing. *J. Amer. Statist. Assoc.* **73** 40-46.
- WAKSBERG, J. (1998). The Hansen Era: statistical research and its implementation at the U.S. Census Bureau, 1940-1970. *J. Official Statistics* **14** 119-135.
- WAKSBERG, J., JUDKINS, D. and MASSEY, J. T. (1997). Geographic-based oversampling in demographic surveys of the United States. *Survey Methodology* **23** 61-71.