

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with C. Horace Hamilton PAA President in 1960-61



This series of interviews with Past PAA Presidents was initiated by Anders Lunde
(PAA Historian, 1973 to 1982)

And continued by Jean van der Tak (PAA Historian, 1982 to 1994)

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C. HORACE HAMILTON

Joint interview of C. Horace Hamilton, Joseph Spengler, and Clyde Kiser by Harry Rosenberg at the Carolina Population Center, Chapel Hill, North Carolina, December 15, 1976.

CAREER HIGHLIGHTS

Charles Horace Hamilton was PAA President in 1960-61 (No. 24). He was born in 1901 in Waco, Texas, and died in 1977 in Raleigh, North Carolina. He received his A.B. degree from Southern Methodist University in 1923, and received a master's degree from Texas A & M in 1925. He then spent a year teaching at Lon Morris College in Jacksonville, Texas, followed by a year completing a teaching fellowship at the University of North Carolina. He then researched the rural church while at Virginia Polytechnic Institute, and completed a Social Science Research Council Fellowship at Harvard University from 1930 to 1931. He received his Ph.D. in sociology from the University of North Carolina in 1932. He was a rural sociologist at North Carolina State University, Raleigh, from 1931 to 1936 and again from 1940 to 1977. From 1967 to 1971, he was also Associate Director of the Carolina Population Center at the University of North Carolina in Chapel Hill. His research and publications focused especially on internal migration in the U.S. and the demography of the South.

ROSENBERG: Horace, I would like to start with you and ask you what your first association was with the PAA.

HAMILTON: I first joined the PAA when I was at North Carolina State University. I was doing research mainly with the agricultural experiment station, the agricultural economics department and rural sociology. Among other things, I was studying some of the state's social problems and needs connected with the Depression. But I had this interest in population. Publications from the 1930 census were just coming out--in 1932, 33, and I guess even 1934. The first word I remember getting about the Population Association was a letter from Frank Lorimer, who had written a book with Fred Osborn on the Dynamics of Population [1934]. He was secretary of the Association [1934-39] and asked me for 75 copies of a little book I had put out for the experiment station at North Carolina State on rural-urban migration in North Carolina to be distributed to PAA members. I called up the experiment station and asked them to ship them off. I was tickled to death that somebody was going to read the damn thing.

I see from the member list that Joe Spengler has that my name was on there in 1935 and I guess that's when I joined. They had a meeting in 1935 in Washington and I probably attended that one.

Among my earliest memories, of course, are some of the people who are here, especially Clyde Kiser. He was our old buddy in the department of sociology here [University of North Carolina at Chapel Hill], back in 1926 and 1927, working on juvenile delinquency in [Garrett?] County, North Carolina.

I was joining other organizations about this time. My first, of course, was the American Sociological Society; I joined that about 1927. There were some population papers being published there and the rural sociology section. Later in the 1930s, 1936, we began publication of the rural sociology journal; we called it Rural Sociology. About the same time the American Sociological Society--they called it Society at the time--dropped the American Journal of Sociology and published their own journal, American Sociological Review. So there were a lot of things happening about that time. I became conscious very quickly that if I was ever going to get any work done I'd have to quit joining organizations--or quit going to all of them. We also had an organization called the Southern Agricultural Workers Group, not farm laborers but professionals working in colleges and universities. We had meetings of that in the South, usually in Atlanta or Birmingham or New Orleans.

The big thing that impressed me about the Population Association is the same thing that

impressed me about other organizations that I had joined during the early 1930s and that was the opportunity to meet with people that I knew only by name and at a distance. I got most of my satisfactions out of the personal and professional contacts. I see the first president was Henry Pratt Fairchild. I remember an article he wrote in Harper's Magazine on population, the 30 or 40 percent increase in births in one year, 1920 or 1921, after World War I. I remember Sorokin of Harvard talking about that. He got the idea that we were going to have overpopulation because of this increase.

Henry Pratt Fairchild was president from 1931 to 1935. It must have been an informal organization in those early years, possibly centering pretty much around Fairchild. Following Fairchild, here is Dublin (1935-36), Warren Thompson (1936-38), Lotka (1938-39), Truesdell (1939-40), T.J. Woofter (1940-41), Whelpton (1941-42). I met all of those people. I knew of their work before the Association started and I utilized their methods, particularly this method of computing a stable population, true rate of natural increase.

SPENGLER: The record probably would bear out that Fairchild was the prime mover who put this thing over. I mean he was the type. I remember when we were together at the first big meeting on birth control, organized by Margaret Sanger in 1934, with Amelia Earhart and Katherine Hepburn and so on, and Fairchild was the real pusher. I don't mean this is the pejorative sense; good organizer. I don't know if we would have had an organization if it hadn't been for Fairchild, who had done work those many years on immigration, this, that and the other. What always struck me was that his brother was a very conservative economist at Yale, wrote on income distribution. He had this book called [Furnace Fairchild?], all his students used to alliterate this thing. I think you really more or less felt this way--the importance of Fairchild as the organizer, pusher. Osborn may have gotten, put up more money and so on, since he was interested.

HAMILTON: I never did know Frederick Osborn. Evidently he was one of the great men in the early history of the Population Association. The man I knew first about in this field was Warren S. Thompson. He had written his doctoral dissertation on the population of China, I guess. I had first met and heard him talk about China.

SPENGLER: I think Warren ran into Scripps while he was making a trip to China and that's how Scripps got interested in this thing and put up the Scripps Foundation [for Research in Population Problems].

KISER: Notestein in one of his articles says that Scripps was interested in world population. He went up to Columbia University and was going through the card catalogue and ran across a reference to Thompson's book [dissertation on Malthus] and got impressed with Thompson and induced him to go with him on a trip to the Far East, in his yacht. They were gone for about a year and when they came back they had the plans for the Scripps Foundation.

HAMILTON: I think the Scripps Foundation played a big role coordinate with the Population Association of America. There were Thompson and [P.K.] Whelpton at Scripps, Clyde Kiser at the Milbank Memorial Fund. Whelpton was an agricultural economist and taught down at Texas A & M College, where I had gone to school. I didn't meet him while I was there.

There were interlocking relationships. In the early 1940s, I was offered a job by the commissioner on hospital care of the American Hospital Association. One of the first people they checked me out with was Warren Thompson--wanted to know if Hamilton would do any good as a demographer on their staff. Warren evidently gave me a good recommendation, so I got the job. It moved my salary from about \$4500 up to \$8000 just within one year. Is Warren living?

KISER: No, he died about nine years ago. He'd been out of his mind for a couple of years.

HAMILTON: T.J. Woofter is another man I would meet at all the meetings and enjoyed contacts with him. In fact, Woofter asked me to write a paper, do a study on rural-urban migration in the Tennessee Valley. So we did that. Conrad Taeuber [PAA president 1948-49] sort of alarmed me. Back under the old FERA [Federal Emergency Relief Administration], he wrote a series of bulletins on the rural population and collectivization. [Something about Ohio State.]

SPENGLER: I played volleyball at Ohio State with [Charles] Lively. He was an exceptionally good volleyballer. If you got first choice, you got him. The ag economists went to Ohio State. I thought the ag economists were much more realistic than the rest of them.

KISER: I went to the population conference in Paris in 1937 [IUSSP] and Lively was there and he told me that Thompson had offered him the job in Scripps before he offered it to Whelpton.

ROSENBERG: Joe, can you tell about your first association with the PAA?

SPENGLER: I was interested in this thing [population] very early, because when I lived in the country we had a very good library in my hometown. So I'd take the stuff home, particularly if it looked like the weather was going to be bad, because I wouldn't have to go to school, for which I was always grateful. I got hold of Malthus's Essay one time and you know he has a couple of formulas in there, forecasting population. I told my father that and he said that was a lot of malarky; nobody could forecast the population. I guess he was righter than I was.

So I had this interest from then on, which was back when I was in--I must have been about freshman high school. I went to Ohio State and I wasn't aware yet of [A.B.] Wolfe's things, but I had a very good professor of ancient history and he liked me and took me to lunch and was telling me about Wolfe. So then I got in with A.B. Wolfe immediately and I think he was one of the charter members of the PAA. So my interest in this was very early. But I cannot remember the first meeting I went to. See, I was at Duke in 1932-33 and then I went back to Arizona in 1933-34. I got one trip from Arizona to Cleveland in 30-31. Times were hard and people were being paid in [warrants?] the year I was at Duke.

I clearly remember going to a meeting when we were all invited to the White House. This must have been the Washington meeting in 1935. What I remember then was there was a man named Frank--L.K. Frank--in the chair. There was some argument--I never felt that social sciences should be pushing anything or propagandizing, though I did think that all issues are subject to examination, and I felt a certain hostility to concern with birth control matters. Of course, Norman Himes was an exception to that. I think that upset Frank a little. I had the feeling that we were enraging everyone.

A lot of my work was with Kuczynski, because I was at the Brookings Institution in 1926-27--the Institute of Economics--and Kuczynski was there working on his two books on Europe. I was studying the movement of fertility in New England, mainly, because that's where the data were best. So that's what I wrote on, and migration and so on. Then I went back to Ohio State. I think Wolfe and Brookings and all got me involved in the PAA. So as I had an opportunity to go to the meeting--maybe the Washington one was the first, or the previous one. So I got in around the beginning.

KISER: The first annual conference [organizing meeting; May 7, 1931] was in New York and the following three meetings, 1932, 1933, 1934--at the Town Hall Club. Fairchild was a sort of resident manager of the Town Hall Club; his office was there. In 1935 we came to Washington.

ROSENBERG: I'm interested in this reception that Eleanor Roosevelt gave. What was the occasion?

SPENGLER: It wasn't much of a reception. We just marched down, arrived, Fairchild stood next to Mrs. Roosevelt and introduced each of us and we shook hands. I thought the White House very stuffy. At the Brookings Institution we'd look out and see the White House and I always referred to it as "your inferiority complex."

KISER: I remember that while we were in session, Eleanor Roosevelt came over and made some remarks to the group. She sat right behind me. Fred Osborn brought her. I wanted a good look at her, so I just turned around and looked and she gave me this stern look. She sat there and knitted while Carter Goodrich was giving his paper on migration and economic opportunity.

SPENGLER: Rupert Vance was there. She was especially solicitous with Rupert; he was crippled by polio.

KISER: At this White House reception, a young guard saw us all starting up the stairs and saw Vance come along on his crutches, so he crooked his finger and took him into a little elevator.

SPENGLER: One thing that struck me early in the game was how many people were interested in population. I was just looking at something I'd forgotten about--a chap named Punke, he was down at Georgia State Women's College in Augusta. I don't remember him from Adam, but the thing that struck me was he had an interest in this. We had some correspondence. Like Horace, I had the feeling that there were a large number of people interested and you had an opportunity to get acquainted.

I bought Lotka's book [Elements of Physical Biology, 1924?; Analyse demographique, 1939?, or other?]. I thought that was the greatest book I read in the first 50 years of my life. What it showed me was the rich variety of matters that [centered on population]. This was what I got out of Wolfe's course on population; you had to study geography and this, that, and the other. As an economist, when people asked me what I was doing and I said I'm working on population. "Well, what's that?" I just let the fools suffer in silence and never paid attention. My wife said I was really rude. The thing that caught my eye and what led me to introduce a course on it just as soon as I could was that you had a tremendous range of materials; all kinds of things emerged here. As far as an economist was concerned there was nothing else.

I first took the course with Wolfe. Then when he went away once, I took it over for him, I think for a term. I went to Arizona and introduced it there even in my second semester or second year, when I had some freedom to put something in. And when I came to Duke, I immediately introduced it there. I was supposedly a labor economist. What I got from Wolfe was a wide range. Check the journals; he wrote a tremendous lot on this. I've been tempted to write a lengthy paper on his contribution. But that's how it came.

Then things just kept enlarging and you had more and more angles. And down here in the South--as long as we were poor in the South and it's hell to be considered other than poor--we were the scheduled classes, so we got a certain amount of money from the SSRC [Social Science Research Council] without being questioned.

ROSENBERG: What's that you have there?

SPENGLER: That's the proceedings of one of our meetings, "The Third Annual Southern Social Scientists Research Conference in New Orleans, 1937." We had a regional meeting every spring; Raymond Thomas was the chairman. Rupert Vance went every year and we would get a car--four or five would go. We got the money. There was nothing to splurge on, of course. This was about the

South--a major theme was always the population problem in one character or another, much of it on agriculture and migration and so on. Simon Kuznets came down one time.

ROSENBERG: Was it always focused on population?

SPENGLER: No, that was a component. But you had a number of people in the South who worked in this and then you got tied in with the population. In addition--as I remarked to Horace--Rural Sociology came into being and my own feeling was that up to the time of World War II that was the best sociological journal in the United States if you were interested in demography.

So there was a general kind of syndrome of concerns. And this fed the Population Association. I think we brought a considerable impact from the South into it, because these urban characters from up in New England didn't understand anything about agriculture anyway. We were able, I think, to give a certain orientation it might not otherwise have had.

ROSENBERG: Do you think you can tell that when you look through the officers--the impact of Southerners on the Association?

SPENGLER: Well, you've had Rupert Vance [PAA president 1951-52], who's a distinguished demographer from the very start, continuing somewhat, I think, Odum's concerns with regionalism.

KISER: T.J. Woofter, Jr. [president 1940-41], too. Odum's Southern Regions was published in 1936.

SPENGLER: Yes, Woofter was particularly interested in what you call the labor force replacement ratio or something. Several people here in the sociology department. These things all touched upon the population excess here in the South. We had a particular orientation when we were working locally and we also had a good empirical orientation, because there was so much to go on.

ROSENBERG: Was a major concern at that time with labor excess in general, let's say, during the 1930s--as a population issue?

SPENGLER: Well, the high rate of fertility among the least privileged agricultural people, particularly the blacks. Whether it was white or black, you had a very heavy fertility and they therefore didn't have the opportunity to elevate their kids upward.

You had the shortage of education and the South spent their small budgets relatively more on education than a good many places, but you couldn't get any federal help on education to speak of, as I recall.

You had mobile human capital and you were here and it's all growing up in the South. That was beginning to move North, because we'd had the previous experience of the heavy movement during World War I. I think that comes out with Kuznets and Dorothy Thomas and those studies. So that was mobile human capital. We didn't turn it into technical terms. The economists talked about the "learning effect." Well, every damn dog is subject to a learning effect. So we didn't fancy it up; we just looked at it in the country. So there was a real sense about what was going on. We had Social Forces too and then Rural Sociology and these fugitive publications. Of course, Horace knows a lot more about these than I do. But that's what made an impression on me.

You had another thing that could have been tied in; you had the National Resources Planning Board set up in Washington, about 1932 or 33. You had two books on trends in the U.S. First there was one during Hoover's time, in the 1920s [William Ogburn?--headed the staff of Hoover's Research Committee on Social Trends], and then there was one in which Whelpton and Thompson had their first projections, that was published by McGraw Hill [Population Trends in the United States, 1933]. And

the sequel to that, the National Resources Planning Board report [Problems of a Changing Population, 1938]. There were all kinds of inquiries. A good deal of this stuff grew out of Southern demographers, I think. Some of the work that had been done sort of fed into that, stirred it up.

I was always interested--as I think Warren Thompson was--in city size. My point was that a big city ought to be burned out, and that's now been confirmed. I had an argument with the New York Times editor on that. I did a piece on city size and migration. It never got published because we went to war just as I was finishing up. This is by way of saying that the interest ranged fairly widely in the National Resources Planning Board.

I think in the history of empirical social science, say from 1929 on, you'd find a whole network of things that fed into each other, to which in a way the demographers contributed a great deal. Not so much in technological terms, although they did very good empirical statistics, I thought, but in the sense of pointing to empirical issues they felt affected man's material welfare. I've been trying to interest one or two graduate students over at our place [Duke] to do something like this.

ROSENBERG: That would be somewhat an intellectual history?

SPENGLER: Yes, it would. But I think it would also throw light on the strengths and weaknesses of how you organize.

This is a little aside from the Population Association, but my point is that we had a whole network of things, because we had a network of interrelated problems. I know that Horace had a lot that he was working with on this. And you, Clyde, up there where you were [Milbank Memorial Fund in New York], you were working at you might say the other side, except for the measurements.

INTERRUPTION. Talking about Rupert Vance as tape resumes.

SPENGLER: One thing that always struck me about Rupert, you never could do anything whatever to help him. We were at VPI and he went up three flights of stairs to do a radio program. The only time Rupert would accept any assistance from me was when we were in Philadelphia, maybe when I was president [1957 PAA meeting, when Spengler was president, was in Philadelphia]. It was windy as hell; just bitter on that elevation. And I sheltered him because his circulation couldn't contend with that. That's the only time it was ever possible for me to extend any assistance.

Rupert introduced me when I made my presidential address ["Aesthetics of Population," published as a Population Bulletin of the Population Reference Bureau, June 1957].

HAMILTON: He gazed out over the audience and said, "I know this audience is a typical demographic table--all broken down by age and sex." That's funny. I can't remember your speech but I remember that. I never was much of a joke-teller but one of the great joys I always had was at these meetings and hearing Vance tell some off-color jokes.

SPENGLER: Two people that always beat anybody else. One was T. Lynn Smith. Clyde Kiser would always have three or four up his sleeve; he'd try them out on me first. T. Lynn always used to have several on blacks.

HAMILTON: Woofter was awfully good too. I thought a lot of T.J. He was very good on methodology--undercounting of blacks in the 1940 or 1930 population census. He pinned down that they'd undercounted 150,000. He was very much interested in that.

When I talked to Woofter about one problem I had, he suggested using another method and he was right, but I wanted an argument at that time. I later recognized that he knew what he was talking about.

On the Southern regions, T.J. Woofter had a special methodology for delineating regions, special kinds of regions, sort of out of character with Odum's generalized approach. But I wonder if it wasn't good to take off from some of Woofter's work, because he adapted Hotelling's method of factor analysis of central components, first components.

KISER: If I might make a serious remark, someone once defined the South as that part of the country in which if you wanted something good to eat you had to go to a Jewish delicatessen.

ROSENBERG: Clyde, could I ask you how somebody from Gaston County developed an interest in population? Do you come from a big family, for example?

KISER: No, there were five of us--three boys and two girls. And my own household is four.

Well, I came to the University of North Carolina as a freshman in 1921. I dropped out and taught in high school a year. Then I heard about Odum's work here, through Jennings Rhyne. Jennings told me he would support me in an application for a fellowship. I didn't get a fellowship, but I got a scholarship that first year when I came back to work on a master's degree. Then after I took the master's degree, I planned to go on but I got hooked up with the study of St. Helena Island, a project Woofter was running there.

SPENGLER: What was your master's?

KISER: My master's thesis was on liquor law violations in Durham and Person counties.

Frank Ross--he used to be editor of the Journal of the American Statistical Association--he and Jack Woofter were doctoral students together at Columbia and they were good friends. So they hatched up the idea of this study of St. Helena Island. I got hooked in with that. We ran into the fact down there that a lot of these Negroes had moved north, although this was not a place where Negroes were downtrodden so much. They were poor but they owned their own little plots of land. And they've had very little in the way of racial tension because there are very few whites there. But still they were moving. So they hatched up the idea that it might be well if I transferred to Columbia, if they wanted me there, and collect some data on Harlem Negroes and then come back to North Carolina for my degree.

Well, I got up there and I liked the group up there pretty well. I became good friends with Frank Ross. As a matter of fact, I gave blood to his infant daughter--I was her blood type--my first year up there; so I happened to fit in very well with him and his family. So I stayed up there for the degree. I was already initiated into demography more or less--Negro migration--and I took Chaddock's courses in vital statistics and population.

Then in the summer of 1931, Chaddock showed me a letter he had from Frank Notestein. Frank Notestein said they had a lot of 1900 and 1910 census data on children ever born that needed analyzing and they had a fellowship for a year, for which I applied. So I went to the Milbank Memorial Fund in 1931. And I learned pretty soon there about the PAA. My immediate supervisor was Frank Notestein and Sydenstricker was above him. So I learned that a young organization, the PAA, was just beginning. They had had their organizational meeting on May 7th, 1931.

I might say a word or two about the circumstances of that organization. In my PAA presidential address, "The Population Association Comes of Age" [1953, published in Eugenical News, December 1953], I mentioned predisposing causes and immediate factors responsible for the Association. Among the predisposing factors was the increasing interest in demography during the 1920s. The Scripps Foundation started in 1922. Pearl and Reed had developed their logistic curve and Pearl had written Biology of Population Growth and at Scripps, Thompson and Whelpton were starting work on their projections. At the Metropolitan [Life Insurance Company], Dublin and Lotka had come

out with "On the true rate of natural increase" [1925]. And in 1928, the Milbank division of research was started to do research on population.

The immediate factor I saw was the stimulus given by the formation of the International Union [for the Scientific Investigation of Population Problems/IUSSP]. The International Union was formed in 1928 and that grew out of a World Population Conference held in Geneva in 1927. Margaret Sanger organized that. Margaret Sanger did more for getting population started than we give her credit for. She approached an anonymous source for money for that 1927 conference and she got it. She approached the Milbank Fund for money for the International Union and she got it. She approached the Fund for money for this first little organizational meeting of the PAA and she got that. Now the amounts were relatively small. For the PAA it was just \$600 to pay Thompson and Whelpton's fare and a few people coming up from Washington, things of that sort. But she got the money on behalf of Fairchild. Fairchild did the legwork in getting the organization started. The Fund supported the International Union the first three years of its existence almost in full. It gave, I believe, \$10,000 a year, or \$30,000 for the three years 1928-31, and the Rockefeller Foundation chipped in with additional support.

The PAA had its organizational meeting May 7th, 1931. In that article I mentioned the first meeting of the American National Committee. The International Union was organized not on the basis of individual members but of national committees, so we had to have a national committee. The first meeting of that American National Committee was held February 4th, 1931. Lotka was the secretary. He gave me the minutes of that meeting and I quoted from that in that article. The first paragraph read something like this: " Louis I. Dublin opened the meeting by stating that he had been asked by the president of the International Union for the Scientific Investigation of Population Problems to become chairman of the American National Committee, in accordance with the organization of its executive committee. Dr. Dublin had accepted and in conjunction with Professor Fairchild and a group that had met at the latter's invitation, he had invited a small group to constitute the American National Committee. Those attending were: Louis I. Dublin, elected chairman; Alfred J. Lotka, elected secretary; and then H.P. Fairchild, C.E. McGuire, Lowell J. Reed, Clarence C. Little, and P.K. Whelpton."

Now last night I was digging through some old files and one of the things I read was a letter from Lotka to Edgar Sydenstricker, March 3, 1931, just about two weeks after that February meeting of the American National Committee. He wrote:

"Dear Mr. Sydenstricker,

In accordance with a motion carried at a meeting of the American National Committee, held in New York on February 4th, 1931, the chairman has prepared a draft of statutes, of which a copy is enclosed. The several members of the committee are hereby requested to communicate to the chairman any comment or suggestion that they may have to make regarding this draft in order that he may be able to send the statutes in final form to Dr. Carr-Saunders for publication."

Raymond Pearl was the first president of the International Union. Lotka gave the list of the original members of that American National Committee: Louis I. Dublin, C.E. McGuire, vice-chairman, and Alfred J. Lotka, secretary-treasurer. And members: O.E. Baker, Department of Agriculture; H.P. McGuire; James W. Glover, the life table man; George W. Kosnak, editor of the American Journal of Obstetrics and Gynecology; Dr. Clarence C. Little of Harvard Club--he was a biostatistician and he once was the president of the University of Michigan--and Dr. Raymond Pearl and Lowell J. Reed, both from Johns Hopkins, and Mr. Edgar Sydenstricker of the Milbank Fund, Warren Thompson and P.K. Whelpton.

Now, I might just say a bit more about that first meeting of the organization [May 7, 1931]. According to Fairchild, there were about 38 there. Notestein in one of his articles recently spoke of the overlapping membership of the American National Committee and the Population Association. Fairchild was the first president of the Population Association; Dublin was the first chairman of the American National Committee. I wonder if there was some jockeying between those two for a position as the arm of the International Union. But the American National Committee was the first on the scene. It stimulated the formation, I think, of the Population Association.

SPENGLER: Did you know anything about C.E. McGuire. He was at Brookings; a Catholic. I had lots of arguments with him when I was at Brookings. He was a brilliant man, very sharp and all, but a profound Catholic, and, of course, I had run-ins with him periodically on this. He was the correspondent to the London Economist too. I didn't know how he happened to get in [the PAA], because he had an ideological concern, I thought, rather more than a substantive concern.

KISER: Frank Lorimer said that Margaret Sanger wasn't concerned too much by science but she stressed the importance of having the backing of science in her movement and she didn't get very far with the International Union. She set up the Union, but it was not going to be an activist organization. She couldn't get very far with Dublin. Dublin opposed birth control on moral grounds. So probably for that reason, I thought there might have been some jockeying for position. Or maybe Margaret Sanger thought she'd take a second chance on the PAA and on Fairchild, because she hadn't been able to get very far with the International Union. But she didn't get very far there either. Notestein says here--he spoke of the fact that she had managed to get some money from the Fund to help set up the PAA. And he said--this is from his article in my book ["Reminiscences: The Role of Foundations, the Population Association of America, Princeton University and the United Nations in Fostering American Interest in Population Problems," in Clyde V. Kiser, ed., Forty Years of Research in Human Fertility, Milbank Memorial Fund, 1971, pp. 67-84]:

"It was expected that she, Mrs. Sanger, would be elected first vice-president. Largely because of Frederick Osborn's influence, her name was withdrawn. Osborn, a great admirer of Mrs. Sanger, persuaded the meeting, and I think Mrs. Sanger, that the fortunes of the field would be advanced if the new Association were to guard its scientific character and keep free from attachment to the birth control movement." [page 70]

Now, from the very beginning, they adopted a scheme to guard the scientific character. They formed what they called a College of Fellows. These were the purest of the pure. Notestein describes it:

"We went to organizational lengths beyond all lengths to keep out all but the purest of the academically pure. I still remember when about a dozen of us would meet in Dublin's office at the Metropolitan as members of the American National Committee of the International Union for the Scientific Study of Population Problems and draw up a memorandum to the new Population Association of America. We would then adjourn our meeting and quickly travel to the Town Hall Club, where the same group would assemble as the College of Fellows of the new Population Association of which were were the creme de la creme. As such we received the memorandum from the American National Committee, pondered its merit, and passed on the results of our superior wisdom together with notice of action taken to the body of the Association. The College then hastily adjourned to reconstitute itself as the Association and receive with gratitude the result of the College's mature wisdom. It really took us an incredible time

to realize that the birth controllers and other action groups were probably less eager to capture the academics than the academics were to avoid capture." [pp. 70-71]

SPENGLER: In recent years, there's been a great resurgence of interest in fertility and family planning, which is really birth control, and all of these new people that have come in on family planning have now joined the Population Association and almost swamped it. That's gone full cycle. But I think the group interested in the scientific approach is so large now and dominant with the journals--Population Studies in England, Population in France, and our Demography and Population Index. By the way, do you know anything about this new World Population Society?

KISER: They're people in Washington. It's sort of an alarmist group, I guess. I couldn't make much out of it.

SPENGLER: I couldn't either; I didn't join it. Funny thing, I belong to the International Union and their dues now are up to \$40. They haven't followed all my other societies, like American Sociological Association, Population Association, Southern and so on, which have reduced the rates for elderly people down to practically nothing. The members of the Union get sort of a double rake. Your dues to the International Union are supposed to include subscription to Population Index along with Population Studies and Population. But we American members are paying for Population Index twice, that is, we get it along with Demography as PAA members.

KISER: I heard that American members of the Union would get a reduced rate because of that. [IUSSP members get a reduced rate in PAA because their subscription to Population Index is covered by IUSSP dues.]

KISER: I joined the Population Association before that first annual meeting [1932] but I didn't get into the College of Fellows. I was just a young squirt. John Innes and I attended that first annual meeting together in the spring of 1932 at the Town Hall Club. I think we were about the only two there--besides the birth controllers, Mrs. Sanger maybe--who were not members of the College of Fellows. The College of Fellows was just a sifting device. It was made up of the very first charter members, the professors and the bigshots. They were to guard the scientific character and keep the birth controllers out.

HAMILTON: The fact that Frank Lorimer wrote me for the 75 copies of that bulletin--it was published in 1934 and he wrote me right after it came off the press--there must have been a relatively small number of members in 1934. [PAA records show "around 100" members in late 1934-early 1935.]

SPENGLER: What about Walter Willcox?

KISER: Willcox was never president of the Association. He attended the early meetings all right. He even attended that [IUSSP] conference in Paris in 1937. I remember he attended a meeting at the Shoreham Hotel in Washington. He must have been well over 80 then, but he walked all over Washington. I went to his 100th anniversary at the Cosmos Club.

SPENGLER: He was over 100 when he died [103]. Got into Ripley [Believe It or Not] for that. I remember going to the Columbia Club for dinner around 1950 and he was sitting there eating with two or three people. I had corresponded with him on something and I went over and shook hands. He seemed quite spry; it was amazing, he was 100 years then.

KISER: After he got to about 100, he offered to be examined periodically by any medical group that might be interested and he did go several times to be examined. Notestein said he enjoyed being thumped by the doctors who examined him.

ROSENBERG: What was the secret of his long life?

SPENGLER: Good parents. I think a combination of genetics and culture factors. Individualistic factors too. I think nutrition has a hell of a lot to do with it and he probably got started on that. Then I think that genetically, some people do not generate excessive cholesterol or something like that--other factors that kill people off.

ROSENBERG: You say that Margaret Sanger was instrumental in generating a lot of resources and enthusiasm but that she somehow was kept out of the mainstream. Did she accept that role?

KISER: I think so. She must have seen what was happening. After all those things happened, she was quite willing to cooperate with a study of her birth control patients--Regine Stix and Notestein followed up 5,000 patients of the Margaret Sanger Clinic. She would have conferences with Notestein and Stix and take issue with them when she thought they were not showing birth control up in as good a light as she thought it should be, but she did things pretty good.

SPENGLER: Do you remember the journal called The Birth Control Review? I wrote things for that. Got invited to the first conference. You had old Charlotte Perkin--all these suffragettes--most entertaining conference I ever attended in my life. There was some pretty good stuff in that review and a lot of nonsense too. But it was in some degree a medium at a time when there was no other journal rather closely oriented [to population].

HAMILTON: I remember the year before I got married, I decided I ought to know as much as I could about birth control techniques. I wrote a personal letter to Margaret Sanger and asked her to send me any information she might have. She sent me an article and wrote me a letter and gave me the brand and name and everything which I could buy. I thought it was very instructive and to the point. As a matter of fact, I sent it to my prospective bride before we married. I wanted her to be sure of what was going to happen.

SPENGLER: She married a wealthy man, Mr. Clee, who lived in Arizona. She moved to Arizona not long after I came East. She was closely associated with the university people; very highly respected in Tucson. She was very comfortable in her older years. I remember one time I made a speech to women on how much wealth they owned and how much more they would own and how it got concentrated when they had smaller families. The Catholics hopped on me--"this stinking, vicious. . ." The president of the University of Arizona, he was a man of parts, he never paid any attention to people who hopped on his faculty--obviously it was a scholarly argument that I presented. I was just describing how family size affected how much wealth they had. There was quite a bit of writing in those days on wealth--this was about 1933.

You remember General Francis Walker? Well, a student of mine was writing on the significance of legislation by our Congress having to do with Indians. He had grown up on a reservation himself. He told me--and I hope I'm remembering this correctly--when Walker was made superintendent of the 1870 census, he didn't have an office. So they made him head of the Bureau of Indian Affairs and that way he got an office. He had a young yellow-haired military s.o.b. named Custer and he told Custer, "Well, if you want to make a name for yourself. . . . [go after Indians?]"

This was an easy way to do it.

KISER: Did you know that Willcox was the Census Bureau's chief statistician for population about 1912?

SPENGLER: He didn't have much in the way of high-powered techniques; he had simple methods. But he made a big impact. I got my vital statistics from E.B. Wilson, Harvard.

KISER: I wrote a chapter for Frank Lorimer in Problems of a Changing Population with a part in it on factors underlying group differences in fertility. I gave the biological factors and then went on to show that biological factors are not so important and most differences were due to differences in contraception. Without asking me, someone cut out the contraception part and it just ended on biological factors, attributing all these fertility differences to biological differences. Who did this? Old E.B. Wilson. [This incident is described more fully in the Notestein interview, above.]

SPENGLER: He was a mean bastard. He and Pearl had some way of ascertaining whether the other planned to go to a certain meeting and if he was to be there, the other wouldn't go. You know why? Old Pearl received an appointment at Harvard once and, according to the story, E.B. Wilson blackballed him. Blackballed him so that for a day or two Pearl was out on a limb. He'd already resigned from Johns Hopkins and then his appointment at Harvard was canceled because of E.B. Wilson's objection. But they took him back at Hopkins.

HAMILTON: Raymond Pearl [roused] a good deal of controversy on the logistics curve and that method of projections.

SPENGLER: Wolfe wrote a critique of it. Whelpton, in his first set of projections, his components method, he criticized the logistics curve.

HAMILTON: I lucked out pretty well on trying to make projections--a short method of projections of population from one decennial census to another for small groups. Came out in Social Forces in 1962. I wrote more papers and did more work with a practical interest after I retired as head of department and also when I was away from the department, like I did in Chicago with the American Hospital Association.

SPENGLER: I think Ed Hutchinson married E.B. Wilson's secretary. What did Wilson say to her?--"Keep your mouth shut." He had a first-rate mind and he was a son of a bitch.

HAMILTON: Getting back to what PAA was like in the early days of my association with it and some of the central characters, I think two people that contributed, man and wife, as much as anybody else to PAA were Conrad Taeuber and Irene Taeuber. Irene, you know, edited Population Index for years.

I remember we had a meeting in Charlottesville [1954] and there was going to be a Negro invited, George Roberts, Jamaican, and Conrad Taeuber made arrangements to have him put up at the [Jefferson] Hotel. When he came to the hotel, he just assumed that arrangements had been made, but they wouldn't register him. Conrad Taeuber went through the ceiling on that. I think he got it worked out, but it was a nasty situation.

It's too bad Irene had a premature death. She had an offer to leave Princeton and go to Georgetown as professor at a very good salary. Princeton found out so they upped her salary and kept her. But she didn't live there; she lived in Washington.

ROSENBERG: Horace, you said you had some thought on your older recollections of PAA.

HAMILTON: I was a generalist on nearly everything--rural sociology, agriculture--and for that reason, I decided at one time that I was going to quit fooling around with population. I was going to stick to rural sociological surveys. I remember telling Dan Price or Rupert Vance that I was going to let the university over here do the armchair research and I was going to do my surveys. Well, I couldn't stick with that because the guy after Truesdell in the population division in the Census Bureau advised me to read a paper at one of the PAA meetings on net migration, techniques, and so on. I did it reluctantly. But gradually--you know, a man writes a paper or series of papers on a certain area and he begins to get tabbed--"Well, Hamilton is the guy to do that paper or say something about that."

ROSENBERG: Joe, what are your recollections of some of the early issues the Association was concerned with?

SPENGLER: I can't remember the issues too well. The thing that sticks best in my mind--something that sticks best in my memory respecting other organizations with which I was associated in the early salad days--was you had a sufficiently small group so we could hear most of the things that were presented. Furthermore, we knew each other, sort of speaking acquaintances, and there were people there with whom we had deeper common interests and it was easy to make contact and carry them on. You didn't have to plan ahead, because you played these things by ear. So what I liked was the smallness of the organization, rather than the conflict. The opportunity I had to get little views on things or lines on things that I could possibly make use of in my own research and teaching. Not so deep, but something I could jot down and stick in my head and go home and it would fester in my brain, even help me to teach or scholar or both.

Now you get these damn big markets. You can't have that anymore. There it was easy since you were all members of the same bark. You could go say, "I'd like to chat with you about this for a moment," and that was just standard protocol. That's what I remember. We got rid of most social scientists of all sorts and if we could restore that situation, I would be all in favor of it--provided I could survive the process.

HAMILTON: The universities would not pay traveling expenses to a faculty member unless they were delivering a paper. I got into trouble on that at Texas. I wanted to go to Detroit one year and deliver a paper on the social effects of the mechanization of agriculture, but they wouldn't pay my expenses. I said, "I'm reading a paper there." They said, "We don't pay people's expenses just because they've got a paper. You have to have some other reason to go." So I paid my expenses that time, several hundred dollars, and I had to take it out of my meager salary and I didn't like that at all. I published it in I think it was Rural Sociology. Had a lot of fun writing it; Lynn Smith got me in on that. Incidentally, a lot of papers I've written have not been papers that I thought of myself, but papers other people persuaded me to write.

Later on, Everett Lee was chairman of the social science section of the American Association for the Advancement of Science, which was meeting in Cleveland in about 1961 or 62. He wanted me to write a paper at first on the Negro deserts the South. I didn't like that word "deserts" so put it "The Negro Leaves the South" [published in Demography, Vol. 1, 1964]. I got more requests for reprints on that paper, and also for it to be reprinted in other books and in other connections. At the same time, I got a request from Duke University--Edward Thompson and McKinney, I believe, were publishing books on the South. I had written this paper on the Negro and found it very easy to expand the same subject matter to whites and blacks and they thought so highly of it they put it Number 3 in the book.

I got some more mileage out of that paper. I was invited to address the alumni of North

Carolina State University--they come back once a year--and the dean wanted me to make an address. I told him, "I have a paper which is going to be published and if you'll let me read it, I won't have to do any extra work." I did. The chancellor was there and all the other bigwigs and little wigs. I got a lot of mileage out of that paper, because people that I had met and known through the Population Association knew that this was something down my alley.

ROSENBERG: Clyde, could you say something about your feeling about the issues the Association was involved with and how it changed? The whole question of size is something I'm somewhat sensitive to, even over the brief period I've been associated with it. It has grown so rapidly. Last year for the first time, I went to the meeting of the Southern Regional Demographic Group, which is quite a small group. They have about a hundred people at their meetings. You get to know people over the two days on a face-to-face basis and it's much more personal. Leaves you feeling much better. People really talk to you about your presentation and you see them again at dinnertime and it's a very pleasant thing. I believe the Association size has been a real factor in changing the nature of it--at least as I hear you talking about all the people you interacted with through the Association.

KISER: Well, as I said, the original membership was around 38, according to Fairchild. I did a little spot map by state of them and most of them lived either in or around Washington or New York. The first three meetings, as I said, were held in the Town Hall Club and they were pretty small rooms. But then in 1935, the attendance did jump, because of the interest of the government workers. That topic in Washington was the relation of population to some of the New Deal programs. [Conference on "Population Studies in Relation to Social Planning," May 2-4, 1935. PAA fourth annual meeting was on May 3, 1935.] The organization was different too. It was in some respects more loosely organized in those days.

HAMILTON: They didn't have sectional meetings; just one big program.

KISER: That's right. Everyone heard the same thing; they had one session going. It wasn't a three-ring circus. Now it's about 24 varieties.

HAMILTON: Clyde, you worked in the field of fertility--the tremendous Indianapolis Fertility Survey--we migration people got to feel maybe the fertility people were running off with things. Dudley Kirk talked a bit about that in his presidential address ["Some Reflections on American Demography in the Nineteen Sixties," published in Population Index, October 1960].

SPENGLER: Yes, I believe he said--well, Warren Thompson used to say, "You can only be born once, but you move a hell of a lot."

KISER: And in another respect it was more tightly organized than now. The members voted on the Board of Directors and the Board of Directors appointed the officers. Furthermore, the Board of Directors was self-perpetuating. They would reappoint themselves. They didn't have this rule about not serving consecutive terms. So there were lots of complaints about a tight little group running things.

HAMILTON: I remember when that fight came to a head; I don't remember what year it was.

KISER: I'll tell you. It happened--not because of anything I did--but it happened in 1953, when I was president, that they did amend the constitution to put officer election on a membership basis. It was Con Tauber who wrote and said he was going to propose this to the members. I rather resented it,

because I had enough to do anyway getting ready to leave and then I had to read up on Robert's Rules of Order. But I think the thing worked pretty well. Phil Hauser was a bit pugnacious, so I wanted him on my side. He was the one I chose to introduce the motions one by one and then we'd vote on them. After the meeting was over, Irene Taeuber said, "You practically had that memorized" [Robert's Rules]. I sure did.

HAMILTON: Phil Hauser was president in 1950-51.

SPENGLER: He was president when they met here in Chapel Hill in 1951. We had two meetings here--1940 and 1951.

KISER: At the 1940 meeting here, Truesdell gave his presidential address on the 1940 census.

SPENGLER: Let me tell you what O.D. Duncan said about Truesdell's address. You know how exciting a speaker Truesdell is. O.D. Duncan [Senior], you remember him, says to me, "Joe, I had to listen to Buckshot [Fall-in-a-trap governor]; damn sight more interesting than this."

KISER: I remember Odum got pretty fed up too. I believe he presided that night.

HAMILTON: But Truesdell was a great man. He's still living too, isn't he?

SPENGLER: He is; he's 90 years old. He's a good man, but he's a hell of a poor speaker. He was the man who introduced me to Phil Hauser. Phil was his assistant for a while [in the Population Division of the Census Bureau].

KISER: Well, it's really a lot of scope for interesting relationships and experiences. I think the trouble with all of our social science organizations--just like the AMA almost--they have a House of Delegates and they no longer attempt to have a mass meeting of everybody.

SPENGLER: There's one thing I miss now compared with the early days and this is not so much a function of change in size, it's something else. People were very knowledgeable about the facts of life in the early days. Now you have a bunch of young men--paralyzed monks or whatever you want to call them--who don't have much real interest in the world or any sense of it at all. So as soon as you shift away from a methodological feature you're apt to be pumping in a dry well. In the early days, people had a pretty wide range of knowledge of economics [and other things]. It might not have been scientific, but at least you had a basis. Now they don't know about any of those things and therefore you get these peculiarly circumscribed notions of [concomitants of] human behavior, despite the improvement in methodological techniques.

HAMILTON: I was thinking that too, Joe. We've had the computer and all the improvement in technology, methodology. Yet I don't believe we can predict any better now than we could 25 or 30 years ago. The facts should be in the computer. What makes a difference is not the computer; it's that they speed things up. And the stuff comes out of that thing. . . you get a stack of computer output that thick. What the hell are we going to do with it? Straight into the garbage can.

ROSENBERG: I wrote to Science magazine--there was an article by Sklar and Berkov on the trends in U.S. fertility and it said something about a resurgence in U.S. fertility. I thought there were some problems with their analysis and I wrote Science at the end of last week and they told me they would accept [my article]. Basically, I challenged what they said; it was on the basis of one year's data in

California.

HAMILTON: The latest month that the monthly vital statistics came out of Washington shows a tremendous drop in births, both in the state and in the nation. The childbearing women, they've had their two kids and this depression, the housing [costs], is developing and beginning to show an effect.

ROSENBERG: But I was thinking about what you said about the use of the computer and refinement of methodological techniques. And at the same time you see a great narrowing in the capabilities of understanding and specialization. It's fragmented the Association, I think. PAA is just enormously fragmented now into small groups.

HAMILTON: There's one thing they do now in most big organizations which helps the little man trying to get a start, give a paper. They have a section called contributed papers. Anybody that can't get on the regular program, the bigtime, he can always send in a contributed paper.

SPENGLER: I hear a lot of people at the Census Bureau watch that very carefully. They use that as a basis for getting to the meeting.

HAMILTON: There's a similar thing. You know this magazine of England called Nature. Very early they adopted the principle of open submission. Anybody who had an idea, no matter what [could be published]; they didn't submit it to a bunch of referees. One of our statisticians out here now at RTI [Research Triangle Institute] worked with me on a problem and he got an idea while he was working on it and wrote it up--just a short couple of pages--and sent that to Nature; got it published. And I never did get my paper published. The damn referees in the American Statistical Association, they wanted to be snooty about it.

SPENGLER: This would make an interesting study. Nature has had a profound influence over the years on scientific thinking. If one could compare the role that Nature has played in England over, say, 50 years with some comparable journal or two or three, I would think this would demonstrate Milton Friedman's point that if you've got a lot of liberty without imposing arbitrary rules, you get more product. I've often skimmed Nature to see if anybody's written something new in there.

ROSENBERG: Clyde, what are some of the books and materials you think might be useful in this project? For example, the history of the Milbank Fund, Forty Years of Fertility Research [Proceedings of a Conference Honoring Clyde V. Kiser, New York City, May 5-6, 1971, edited by Clyde Kiser, Milbank Memorial Fund, 1971]. Obviously it's a central kind of thing.

KISER: That's right. There are three historical articles in here. My own is, "The Work of the Milbank Memorial Fund in Population since 1928." Then Notestein's piece, "Reminiscences: The Role of Foundations, the Population Association of America, Princeton University and the United Nations in Fostering American Interest in Population Problems." And then Frank Lorimer, "The Role of the International Union for the Scientific Study of Population."

Then this other little red book of which I've given each of you a copy. It's called, The Milbank Memorial Fund: Its Leaders and Its Work [by Clyde Kiser, Milbank Memorial Fund, 1975]. There's a little history of population here but not too much. I devoted this mainly to public health work.

There is another good book, the proceedings of the World Population Conference in 1927, edited by Margaret Sanger and published in London by Arnold. Another book is the proceedings of the 1931 IUSSP conference in London; that was the first annual meeting. It's edited by G.H.L.F. Pitt-Rivers. He was an erratic guy but he was pretty bright. He was the one who really caused a ruckus in

that Paris meeting in 1937. He was something of a Nazi and he wanted to kick the Czechoslovakians and several of the other Communist countries--or those he thought were on the verge of Communism--out of the Union. There was a lot of argument. The Germans and Nazis there wanted to give papers in which they talked about the master race and all of that. In planning the congress, according to Lorimer, they put all those papers in the same session and let Frederick Osborn be the chairman. At the end of the session, Osborn quoted Voltaire: "I disagree with everything you say, but I would give my life for your right to say it."

SPENGLER: Gini organized a meeting earlier and Sorokin invited me to give a paper there, which I gave on state and population, or something like that. Later on Gini had another one; I gave a paper to it too. [The IUSSP'S 1931 first conference was planned for Rome but transferred to London when it was learned that it would be used as a platform for the proclamation of Mussolini's theories. Gini, however, organized an international population conference in Rome, securing the "attendance of a considerable number of foreign scholars." Frank Lorimer in the article cited above, p. 89.] This brought together a good many demographers. One thing that struck me was the small knowledge Americans had of the degree of population study in Europe. There were a tremendous number of pretty good workers and fairly good statistics. The Hungarians had done pretty good work way back. But except for people like Wolfe who were well educated, there was small comprehension of the nature of the work that was done there. We were somewhat provincial, except for the connections that we had with the British.

End of Interview



Some Problems of Method in Internal Migration Research

Author(s): C. Horace Hamilton

Source: *Population Index*, Oct., 1961, Vol. 27, No. 4 (Oct., 1961), pp. 297-307

Published by: Office of Population Research

Stable URL: <http://www.jstor.com/stable/2731789>

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C U R R E N T I T E M S

SOME PROBLEMS OF METHOD
IN INTERNAL MIGRATION
RESEARCH

It may be that most population research workers not only think that their own specialty, in comparison with other specialties, is more interesting, more challenging, more important, but also that its methodological problems are more complex. If such professional sectionalism does exist, your speaker cannot say that he is an exception to the rule. But he does have an "out." He has never considered himself as a member of any particular research clique. Although he has done more work in the field of internal migration than in any other specialty, he has ranged far and wide in other divisions of population analysis, including fertility, mortality, health, welfare, medical care, distribution, ecology, and population growth.

But even if most of us are overenthusiastic with respect to one division of population research or another, we need not be apologetic about it. Specialty pride and enthusiasm are important sources of motivation. They are justified provided the research worker has a sense of humor and is broad-minded enough to concede the same feelings and enthusiasms to fellow workers in other fields.

Although it might have been more appropriate to discuss some topic of more general interest to the membership of this organization, I have chosen a subject in line with my major and current interest. I am convinced that it is not only of great importance, but also timely and appropriate for this occasion. It is timely because our new Census is now in the process of publishing a vast amount of new data which can be used in migration research. Its importance can be measured in terms of the recent appropriation by Congress of \$394,000,000 for the relief of distressed areas, whose ills are to some extent related to the failure of migration to bring about a balance in the supply and demand for labor.

Some of the things that I wish to say have a broader application than to the subject of internal migration. Many of the problems in method that migration researchers have are similar to those of all social scientists and particularly to those of other demographers.

May I hasten to add that it is not my intention to cover the entire field of research method in migration studies. Such a comprehensive approach is not needed here, and if it were, there would not be time for it. For those of you who would like a systematic and thorough review of general methodological problems of internal migration research, I recommend Don Bogue's excellent chapter on "Internal Migration" in The Study of

Editor's Note.—This is the text of the address delivered by C. Horace Hamilton, President of the Population Association, at the banquet on the evening of May 5, 1961, at the Barbizon Plaza Hotel, New York City, as part of the annual meeting of the Association. It is hoped that in future we may each year publish the Presidential Address in the October issue of Population Index.

Population, edited by Hauser and Duncan. Bogue's bibliography includes some 66 reference to other social scientists who have made substantial contributions in method.

The Problem of Data

The movement of a person in space from one point to another is, like the tossing of a coin, a simple physical event. It is a common experience that can be objectively defined and observed. The number of people who move from area A to area B and from area B to area A during period T can be counted, and their characteristics can be observed and recorded. Likewise, the characteristics of areas A and B can be observed and recorded. Other relevant events in the lives of the people, movers and non-movers, and in the areas can also be observed and recorded.

With these few facts at hand, the study of population mobility should proceed in a simple, straightforward manner. Rates of mobility, proportions of people who move in, out, and between two or more areas can be computed, and comparisons can be made between people having different classes of characteristics. The volume and character of in- and out-migration can be determined, and the net effect on the population of the various areas can be observed. The interchange of populations between large numbers of places and areas can be determined, and related to such factors as distance, numbers of people in and between the various places and areas, incomes, levels of living, employment and unemployment, housing, taxation, transportation, institutions, and services.

From these simple facts, types of mobility, or of migration streams, can be differentiated, and intensive surveys and case studies made within types for the purpose of discovering the operation of psychological, social, and economic factors in the migration process. Thus, as any one can see, the study of internal migration should be relatively simple, and a set of reliable generalizations covering the field should be accumulated in no time at all.

Statistical Complexity. But about here we must quit dreaming and begin talking about realities. In spite of the apparent simplicity of the statistics of human mobility in physical space, the study of such phenomena, like the study of probability, is fast becoming, if it has not already become, a complex mathematical science. The little event, the movement of an individual in space from one point to another, has been transformed into an elusive, complex, statistical monster. It can no longer be studied by the simple methods of seventh grade arithmetic. Now we must develop hypotheses, construct logical and mathematical models, use set and game theory, draw multistage stratified random samples of clusters, test null hypotheses, accept or reject alternative hypotheses, control errors of the first and second kind, make parametric and nonparametric tests, and perform complex computations involving analysis of variance and covariance, factor analysis, and multiple correlation and regression—linear and curvilinear. To top it all: even if an individual researcher can perform all these operations with skill and interpret his results wisely, he is wasting his time. The electronic computers have taken over. All the

research demographer needs to know now is what to tell the programmer to tell the computer!

Quality of Data. And then, of course, there is the little, almost forgotten problem of getting enough appropriate, accurate data to feed the hungry electronic brutes. In North Carolina we have partially solved this problem by tabulation of the daily milk production records of all the certified cows in the state! It may or may not be true that many city people think that milk comes from the supermarket, or from cartons delivered by the milkman; but there is danger that the modern demographer will think that the sources of his data are electronic tapes and census volumes. No scientific study is any stronger than its weakest link, and in social research that is the problem of collecting accurate, reliable data. The interviewer and the enumerator should be as well trained and as capable in performing their functions as the statistician is in performing his function.

Many Kinds of Mobility. But I would not have you think that I attribute the growing complexity of migration research to the development of new skills, techniques, and instruments. The apparently simple elementary event of the movement of a body in space is actually not so simple as it looks at first glance. There are a great many different kinds of movements. They vary in terms of distance, frequency, purpose, cause, consequence, and meaning, and in social significance.

Movement which involves a change in usual place of residence from one community to another is classed as a migration. Communities vary in size, type, and complexity—and are difficult to define and delineate. A movement between two small communities within a few miles of each other is not considered to be nearly so significant as one between two large cities, or between a small rural community and a large distant metropolitan center.

The Real Variable. The fact that a person may move more than once within a specified time period, or that he may circulate during the period between two or more places, adds to the complexity of migration statistics. Consequently, the classification of people into a simple dichotomy, movers and nonmovers, or migrants and nonmigrants, is not exactly accurate. The true migration variable consists of 0, 1, 2, 3, or more moves and is very likely a natural Poisson type. In some studies, and for some types of data, it may be convenient and profitable to use the real variable rather than a simple dichotomy. This would be the case, for example, if one were studying migration behavior of the same individuals over a long period of time, as is possible in longitudinal studies and in the use of OASDI data, i.e. social security records.

Variety of Conditions, Characteristics and Causal Factors. The heterogeneity of migration statistics is further complicated by the potentially large number of characteristics of individuals, of communities of origin and destination, and of other variables which in one way or another are involved in various types of migration. Donald Bogue, in his chapter on "Internal Migration," gives a list of fifty situations, factors, and conditions which likely have some relation to migration./1

Age, sex, color, marital status, employment, are all so highly related to migration that multiple cross-tabulations and large numbers of cases are needed in order to establish reliable uniformities and generalizations. Such heterogeneity of data results in difficult and intricate techniques of analysis and in making an excessive number of minor generalizations: the researcher can hardly see the forest for the trees. His worst enemy is the nature of his data. This problem can best be met by: (1) the limitation of research projects to a small number of important hypotheses, or to selected types of areas and streams of migration (such as rural-farm and nonfarm, rural and urban, metropolitan and nonmetropolitan, central city and fringe, east and west, north and south); (2) limitation of studies to specific population categories (such as college graduates, professional people, construction workers, aged people); (3) concentrating research on specific factors (such as educational selectivity, distance, income and employment, population pressure).

Variety of Scales. The analysis of migration and related statistics is also complicated by the different kinds of scales or forms in which the variables are expressed or measured. Some are simply sets of discrete categories, i.e., nominal scales; some are simple dichotomies; some are ordinal; some are cardinal; and others are hybrids, such as the ordered metric scale. Incorporation of different types of scales in the same multivariate model creates messy, if not impossible, computational problems. Unfortunately, neither the college textbooks in social statistics nor those in population analysis provide the demographer with analytical tools needed for handling these diverse forms of data in the same problem.

The Survey Method

The cost and difficulty of collecting primary data on migration by the survey method is a factor seriously limiting the amount of scientific internal-migration research. Over the past fifty years, hundreds of small field surveys that included some questions on population mobility have been made. The limitations of these studies are well known: they are limited to small samples representative of only a small segment of the nation's population; they are usually limited to certain population categories or to either in- or -out-migration; and they are not large enough to permit detailed analysis and the making of many generalizations. Nevertheless, the accumulation of the results of these surveys over the years has increased our insights, and has led to the testing of new hypotheses and to the designing of better surveys. There is still a great opportunity for sound migration research by the survey method. Repeated surveys in the same communities and areas—the longitudinal approach—would increase greatly the effectiveness of the method.

Census Data as a Source

The use of Census data presents both opportunities and problems. Their coverage, in terms of time, area, and population, makes it possible

to discover trends and uniformities broadly applicable to historical and cultural epochs and to the life cycle of great nations. Such generalizations are of value to other nations which are following, or may yet follow, similar patterns of social and economic development.

Underuse of Census Data. It is to be regretted that more use has not been made of the extensive migration data of the 1950 United States Census. Possibly too few social scientists are aware of the research potentials (and limitations) of these data, and even less aware of the methods available for blasting scientific truth out of them. (Perhaps it would help the researcher if the Census Bureau would provide a simple set of instructions with each table—much on the order of the instructions for cooking found on frozen foods—a sort of Census research cook book. For example, “How to Digest Table 19, Special Report P-E No. 48, in Three Easy Lessons.”)

The meticulous research worker, accustomed to handling small amounts of sample data, tends to be overcome with the sheer mass of Census statistics which can be used to study internal migration. I suspect, too, that the average sociologist and economist, being heavily burdened with teaching and other duties, is quite willing to “let George do it” when it comes to analysis of Census migration statistics. The trouble with this attitude is that there are too few Georges—too few Donald Bagues, Dorothy Thomases, and Henry Shryocks. The profession needs more research workers giving full time to migration study.

Area Size and Tabulation Problems. I am, of course, aware of other attitudes toward Census data, their quality and adaptability for research in internal migration. On the one hand, we hear that there is not enough small-area data and, on the other, that there are not enough detailed cross-tabulations of social characteristics with migration. The fact is that these two wishes are not entirely compatible. We do need as much small-area data as we can get; and for large areas (such as regions and divisions) we also need as many cross-tabulations as we can get. However, it is unrealistic to expect any significant amount of multivariate cross-tabulations of characteristics with migration data for small areas, small migration streams, or even those of intermediate size. I am convinced that multivariate cross-tabulations of characteristics with migration data reduce the need for small- and intermediate-area data where cross-tabulations can only be made with one variable at a time. For example, a table showing the relationship of education to migration for state economic areas (or for any other small areas) is of little value unless age is controlled. We can afford to be indifferent to small areas where adequate multivariate cross-tabulations are provided for large areas. In order to get cross-tabulation detail we can also afford to be indifferent to color and even sex in some cases.

All that I am saying here is, as Karl Pearson said long ago, that space and time have no essential substantive reality.² They serve only as convenient frames in which to arrange and correlate distinctive cultural, social, and economic patterns with migration phenomena.

Migration Streams. The study of specific migration streams presents peculiar and difficult problems. From the standpoint of multivariate

detail we have the same problem with small streams that we have with small areas: the smaller the stream, the fewer cross-tabulations can be economically provided. Cross-tabulations of all counties and cities, or of all state economic areas, can be used in relating migration to distance and are useful for that purpose. However, we can afford to be indifferent to the individual identity of the thousands of small migration streams, like the minute branches of a great river system. What is needed, and what we already have to some considerable degree, is detail for large migration streams obtained by classifying places of origin and destination in various ways, such as rural, urban, metropolitan.

Net Migration Estimates

Much of what I have said is applicable primarily to Census data, based on questions relating to place of residence at some date previous to dates of census-taking. Such data are obviously superior in many ways to estimates of net migration obtained by either the vital-statistics or the survival-rate methods. The advantages, disadvantages, and peculiar problems of these different methods and types of migration data have been discussed quite fully and adequately by Don Bogue, Dorothy Thomas, Everett Lee, and others./3

It should be emphasized, however, that net-migration estimates by one of the residual methods provide for a decade the only large set of migration statistics covering not only the total population of the nation but also that of counties, cities, states, economic areas, divisions, and regions, with age, sex, color, residence breaks. It is also possible to study net migration by educational level, age, sex, color, and residence down to areas as small as states.

An important and useful function of the net-migration approach is that it can, and does, show the net selective effect of population migration on the characteristics of population. Net migration rates, as is the case with gross migration rates, may also be correlated with any number of independent variables as a means of studying the causal and conditioning factors in the migration process.

Unfortunately, because of changes in the definition of the rural-farm population, it will not be possible to study net migration from farms during the 1950-1960 decade. Calvin Beale and Gladys Bowles, in a recent paper, have shown the effect of these changes on the 1960 farm population of the nation./4 The net effect is that the rural-farm population of 1960 is approximately 21 per cent less than it would have been by the 1950 definition. Thus, we have not only succeeded in sweeping four million farm problems under the statistical rug, but we have at the same time eliminated a vast amount of professional labor on the part of the agricultural economists and rural sociologists, who would like to use the net-migration estimates usually made by the Farm Population and Rural Life Branch of the United States Department of Agriculture!

Fortunately, it will still be possible to make net-migration estimates for the rural and urban segments of the counties and states of the nation. Here, however, there is the tedious problem of adjusting the 1960

populations for changes in urban boundaries. It is to be hoped that this important work will be done either by the Farm Population Branch or by some other agency equipped with the needed skills and tabulating machinery.

Problems of Analysis

Although I hold the philosophy that the most effective statistics are those that are simple, straightforward, and easy to understand, I realize, too, that such advanced and complex techniques as analysis of variance, co-variance, and multiple regression, properly used and interpreted, are useful tools in the hands of a capable operator.

It is recognized, also, that the nature and limitations of the data available lead the researcher to use one of these techniques, or possibly some combination of them. In the ideal situation where all possible combinations of independent variables, or sets of nominal characteristics, have been cross-tabulated with a migration rate as the dependent variable, a multi-way analysis-of-variance model may be used as a means of breaking the total variance down into its component parts, consisting of all the main and interaction effects, and of determining the proportion of the total variance of the dependent variable that can be explained by the association with the independent variables. In this model, it is also possible to include characteristics of areas of origin and destination; and the dependent variable may be expressed in terms of in-migration, out-migration, or net migration.

The analysis-of-variance model is suitable for more than explanatory purposes. The rates of the ultimate cells of the table may be used as prediction coefficients when applied to similar population subclasses.

Unfortunately, the Census Bureau cannot, for one reason or another, provide all the cross-tabulations needed for a complete analysis of all the information available. The researcher must either use what he can get or go over to a multiple-regression model based on areas as units of observation.

Limitations of Analysis of Variance and Multiple Regression. There are several logical and mathematical problems in the use of analysis of variance and multiple-regression models which have not, in my opinion, been satisfactorily resolved. The use of group and area means as units of measurement, in analysis of both variance and multiple regression, leaves unexplained the variance within subclasses and areas. In studies of mortality and levels of living, and in net migration, I have found that the usual procedures resulted in greatly exaggerated estimates of explained variance. In some cases, where analysis of variance and multiple regression indicated explained variance running as high as 75 to 90 percent, the true percentage of explained individual variance was found to be less than from 15 to 20 percent, and frequently much smaller. Although I have seen some apparently logical defenses of the use of group and area means as measures, I have not been satisfied with them.

Our objective, statistically speaking, is to account for all the variance in the dichotomous migration variable in the population about which we

wish to generalize. This variance is the product of the migration rate and its complement, i.e. PQ . For operational purposes, this objective may be expressed in terms of sums of squares which, in the case of dichotomous variables, is NPQ , N being the number of people subject to the "risk of migration," i.e. the number of people who could have migrated. Even though it may be impossible to explain or account for 100 percent of the sums of squares, or of the variance, such a goal serves as a measure of the success of our efforts.

Unexplained Variance. In cases where proportions or true rates of migration are used as dependent variables, it is possible to determine, not only the total individual variance to be explained, but also the unexplained variance within cells. With these two pieces of information it is possible to determine the real proportion of total variance which has been explained by analysis either of variance or multiple regression. As I said before, the results are usually quite deflationary. It may be concluded, therefore, that even if we had all the Census data on migration cross-tabulated with every other characteristic, or piece of information available, we would still be far, very far, from our scientific goal of explaining 100 percent of the variance in migration. To approach this goal more closely we would have to obtain reliable quantitative information on all of Bogue's fifty factors, to which I referred a few minutes ago!

If this makes you feel pessimistic about ever explaining migration behavior, just remember that the goal of social science in many other areas is also far from being achieved. Migration is a complex behavioral problem. If we are ever able to explain it fully, we shall at the same time have gone a long way toward understanding human behavior in general. Another consoling thought is that if the statisticians were able to explain human-migration behavior easily and successfully by means of mathematical models and computers, there would soon be little work left for sociologists and economists who are not high-powered mathematicians!

Use of Migration Research in Predicting Population Growth

Because of the great interest in future population growth of both large and small areas, attempts have been made to use migration research as a tool for improving the population predictions and projections. These efforts are based on the fact that migration and natural increase are the two major components of population change and on the belief that predictions of population change can be made more accurately if these two components are handled separately. There is some justification for this point of view, because of the relation of each of these components to the demographic composition of the population (age, sex, race, marital status, etc.), and because each component (migration, births, and deaths) is differently related not only to the demographic factors but also to external conditions and influences.

However, as we all well know, the prediction of future population changes is a hazardous undertaking. At least the amount of migration directly attributable to demographic factors is small, and the prediction equations are all based on the conditions and events of the recent past.

It can be shown that, with other factors constant, a surplus of natural increase in one area will migrate to another area that has a natural increase deficit, but that the age distribution of the natural deficits and surpluses must be taken into account. A large percentage of internal migration is closely related to the age factor. For example, a relative surplus of babies in an area might not "cause" outmigration, unless there was also a relative surplus of working parents of the babies. Possibly, trained demographers can utilize such information in improving predictions of future migration; but in the final analysis, the future population of any area, and hence, migration to and from the area, are functions of economic and social changes that are difficult to predict. If we are going to try to predict the amount of migration to or from any given area we must, therefore, turn to a study of the long-term economic and social influences, and adjustment processes, that may either stimulate or limit migration to an area.

For example, in the case of agriculture, it can be expected that the present cycle of technological change and improvement in labor efficiency will continue for a decade or so and that migration from rural areas will continue. Detailed studies of particular types of farming could possibly improve our estimates of the expected amount of out-migration from certain areas. Possibly, also, detailed studies of the changing need for labor in other industries and occupations could improve our estimates of migration to and from other areas. To supplement this type of analysis, detailed studies of the growth and decline of the economies of the various regions and metropolitan areas of the nation are needed.

But with all these different avenues of research completely exploited, there would still remain many unknowns in an estimating or prediction equation. Each researcher must decide for himself whether the difficult and tedious work involved in introducing migration estimates into his prediction equations is worthwhile.

Conclusion

The need, the demand, and the opportunities for research in internal migration were never greater than they are today. The increased need for such research grows out of the dynamic character of modern society. Rates of technological, social, and economic change have speeded up, and have created a greater need for social and geographical mobility of workers and members of their families. The rapidly rising levels of living and of real incomes have opened up new occupations and industries, and have expanded the need for labor in many old industries and occupations.

On the other hand, scientific discoveries, technological developments, changes in ways of living and in social organization have restricted opportunities for gainful employment in many occupations and industries. Agriculture is a prime example: but the trend may be seen also in many nonagricultural industries and occupations. Automation is even reducing the amount of labor needed in many service occupations.

Obviously, these changes in sources of jobs involve migration from one community to another. There is, therefore, an increased need for a better understanding of the extent and character of internal migration and the role that it plays in the process of economic and social adjustment.

The increased demand for the products of migration research is coming from administrators, social scientists, and policy-makers who, for one reason or another, are interested in economic development and in the relation of human migration to the functions and responsibilities of government and other social institutions. For example, in my own field of interest the demand for knowledge about rural-urban migration as it relates to agricultural adjustment and public policy has increased greatly. Questions such as the following are being asked:

“In view of the rapid development of new agricultural practices and of the increasing size of farms, should not more people leave farms and seek employment elsewhere?”

“Under what conditions and for what inducements will more rural people move to nonagricultural occupations and communities?”

“Assuming continued national economic growth, including an increase of the nation’s population (of something like 3,000,000 each year), and assuming a continuation of the increase in the productivity of agricultural labor, how many people will likely move out of agriculture during the next ten years?”

These questions and others like them, and their corollaries, have many ramifications. They are complex. The kind of research required to answer them is not simple. Much of the data needed are not readily available, and available methods of research and analysis are not altogether satisfactory.

The point I want to make here is that there is greatly increased demand for the products of migration research; and this provides an opportunity and a challenge, not only for those of us who have been working in these fields for a number of years, but also for new talent. Unless social scientists, trained in demography, take advantage of this increased demand for migration research, others, not so well equipped or experienced, will move into the area. If such should happen, we can expect to see, within the next decade, a lot of superficial migration research, naively conceived, poorly designed, and inefficiently executed. It goes without saying that the present generation of experienced demographers, doing migration research, should continue to sharpen their tools, improve their research designs, and invent and develop new techniques of analysis.

In closing I should express the hope that, in spite of the difficult methodological problems involved in migration research, more of the younger generation of demographers and social scientists will see the opportunities and specialize on internal migration research. The road is rough, but the rewards and satisfactions in research achievement are great.

FOOTNOTES

- 1/ Hauser, Philip M., and Duncan, Otis Dudley, Editors. The study of population: inventory and appraisal. Chicago, University of Chicago Press, 1959. Pp. 499-501.
- 2/ Pearson, Karl. The grammar of science. New York, Meridian Books, 1957. First published in England in 1892. See Chapter VI, Space and time.
- 3/ Hauser and Duncan. *Op. cit.*, pp. 507-509.
- 4/ Beale, Calvin L., and Bowles, Gladys K. The 1960 definition of farm residence and its marked effects on farm population data. Paper presented at the annual meeting of the Association of Southern Agricultural Workers, Jackson, Miss., Feb. 6-8, 1961. Washington, U. S. Agricultural Marketing Service, 1961. 10 pp., processed.

RESEARCH INTERNESHIPS AND FELLOWSHIPS The University of Michigan Population Studies Center has been organized to expand the existing programs of training and research in population studies centered in the Sociology Department. Professor Ronald Freedman is the first Director of the Center. Professors Amos H. Hawley and David Goldberg are Associate Directors.

Research internships and fellowships are offered by the Center for graduate work in sociology or other departments for students with a major interest in population problems. Stipends range from \$600 to \$2700. Travel grants are available also for a limited number of foreign students coming to the United States and for advanced American students planning field work abroad.

A program of research in population and public policy is jointly sponsored by the Center and the Institute of Public Administration. In addition the major research activities of the Center will deal with population distribution and organization, migration, social factors affecting fertility, and comparative population analysis. The Center will also develop in time a co-operative program with one or more universities or institutes abroad for research and training in population studies. The basic program of the Center is supported for seven years by a grant from the Ford Foundation.

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