

DEMOGRAPHIC DESTINIES

Interviews with Presidents of the Population Association of America

Interview with Clyde V. Kiser PAA President in 1952-53



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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CLYDE V. KISER

PAA President in 1952-53 (No. 16). Interview with Anders Lunde at the PAA annual meeting, New Orleans, April 26, 1973. See also the following interview of December 1976 in which Harry Rosenberg sits down in Chapel Hill with Kiser, Horace Hamilton, and Joseph Spengler.

CAREER HIGHLIGHTS: Clyde Kiser was born in 1904 in Bessemer City, North Carolina, where he again lived from 1974 until his death in 2000. He received the A.B. in liberal arts in 1925 and the A.M. in sociology in 1927, both from the University of North Carolina in Chapel Hill, and the Ph.D. in sociology from Columbia University in 1932. As a member of the technical staff of the Milbank Memorial Fund in New York City from 1931 until his retirement at the end of 1970 (at which time he was Vice-President for Technical Affairs), he was deeply involved in the Milbank Fund's pioneering promotion of fertility and population studies in the U.S. and also in Latin America. (Kiser has described these activities in "The Work of the Milbank Memorial Fund in Population Since 1928," in Forty Years of Research in Human Fertility, proceedings of a May 1971 conference honoring him on his retirement from the Fund, edited by himself, 1971; and "The Role of the Milbank Memorial Fund in the Early History of the PAA," Population Index, October 1981). Among these activities, he, along with P.K. Whelpton, played a leading role in the conceptualization, planning, organization, and analysis of the 1941 Indianapolis Survey, progenitor of a subsequent long line of major U.S. fertility surveys. (He was coeditor with Whelpton of the five volumes summarizing results of that survey, Social and Psychological Factors Affecting Fertility, 1946-58). From 1942 to 1975, he was also a senior research demographer with the Office of Population Research at Princeton University, where his wife Louise Kiser was co-editor of Population Index from 1945 until shortly before her death in 1954. He was Adjunct Professor of Sociology at New York University in 1945-46 and served on many U.S. government committees dealing with population and fertility. He is the author or co-author of several books and many articles on U.S. population and fertility in particular, including The Fertility of American Women (with Wilson Grabill and P.K. Whelpton), 1958, a 1950 census monograph, and Trends and Variations in Fertility in the United States (with Grabill and Arthur Campbell), 1968, a 1960 census monograph.

LUNDE: Clyde, would you tell us some of the things that you recall about our early organization?

KISER: It happens that I started work at the Milbank Memorial Fund in October 1931. That was the very year in which the Population Association of America was organized. There was a so-called organizational meeting in 1931 [May 7] which I did not attend; in fact, I knew nothing about it. But the following spring of 1932 [April 22-23] was the time of the first full-fledged meeting of the Association and at this meeting there was not only a scientific session but also a ratification of the constitution of the PAA.

Now, you ask about the circumstances surrounding the organization of the Association. The demographic background is somewhat as follows. As you know, this was a period of low fertility. The immigration question had been solved in that the immigration laws were enacted in 1924. So, the people who were interested in population, studying in the different colleges, began turning their attention more and more to problems of this country, national problems. In the meantime, the birth registration area was greatly improved. It was in 1933 that the last state of the union, Texas, was admitted to the birth and death registration area, thereby taking the whole country into the registration area.

In 1927 Margaret Sanger, the birth control person in this country, managed to get money and stir up interest in holding an international population conference in Geneva. The following year, 1928, the International Union for the Scientific Investigation of Population Problems was organized and

Raymond Pearl of Johns Hopkins University was elected first president. The International Union was organized on the basis of national committees rather than individual membership, so that it was necessary for a country to have a so-called national committee. The leading students of population so organized themselves on a rather informal basis into an American National Committee and this little group then stimulated the organization of an association. For a time, there were two groups in this country, the group of the American National Committee and then the group of directors of the Population Association of America.

After the Association was organized in 1931, provision was made whereby the directors of the Population Association of America would also serve as members of the American National Committee. The International Union was disrupted by the Second World War and in 1946 it was reorganized on an individual membership basis rather than a national committee basis. The national committees continued for a while. As I recall, though, the U.S. members of the International Union made up the American National Committee until the whole idea of the American National Committee was abandoned, about two years ago.

Now, you asked about the character of the Population Association of America. As I recall, in 1932 when we had our first annual meeting, the total membership was probably around 50 [67 persons registered for the 1932 meeting]. The first meetings were rather small; they were at the Town Hall Club [in New York City]. They had one fairly large dinner meeting. I recall that Frank Notestein gave a paper on differential fertility at that meeting.

The first really large meeting was the one in Washington in 1935 [May 2-4]. Roosevelt had come in as President, he'd formed these New Deal committees, and there were a lot of new government agencies working in fields in which population was a factor. The place of the meeting was the Willard Hotel. At an afternoon session, Carter Goodrich gave a paper on migration that stimulated a lot of interest. Mrs. Roosevelt attended that session. Fred Osborn was an old friend of the Roosevelt family, so he went over to the White House and escorted her to the meeting. It so happened that he and she sat right behind me. I was rather embarrassed once. I wanted to get a good look at Mrs. Roosevelt, so I turned around and stared at her. She glanced up at me as if she disapproved of what I was doing, so I turned back.

At the time of that meeting, Mrs. Roosevelt had a tea party for the members; she invited the whole membership. The actual numbers at that time were, I should think, probably not more than 75. But there were a good many government workers there too. I don't believe that all the government workers were invited to the party. It was a fairly good-sized party.

At the dinner meeting of the conference, Frank Notestein gave a paper of which I was co-author. We were told that we should wear black tie, so we wore black tie. Henry Pratt Fairchild presided. Fairchild was first [PAA] president and he was president from 1931 to 1935. I don't know what the constitutional provisions were about presidents in the beginning, but Fairchild was re-elected just as a matter of course for the first four years. After that Louis Dublin was elected for one year and it was pretty much on an annual basis until World War II, when [Lowell] Reed served for about three years [1942-45].

LUNDE: Was there any activity during the war period?

KISER: We missed three meetings, as I recall, 1943, 1944, 1945. There was not much activity; a lot of people had gone to war.

LUNDE: What about the period when you were president, in 1952-53. What were the issues in those years?

KISER: Those meetings were in Cincinnati [May 2-3, 1953]. We went out Thursday night; got there

Friday morning. In those days, they were much concerned about population problems in the underdeveloped areas, the demographic consequences of modernization. Notestein and his group at Princeton had discovered a rather consistent tendency for modernization to bring decline in death rates before birth rates are affected, thus modernization tends to be followed by a period of rapid population growth. And partly because of the activities of the WHO, death rates were tumbling all over the world.

So, we met in Cincinnati and one of the things I was worried about was the possibility of a parliamentary snag at the business meeting at which I was to preside. An important matter to be decided was whether there should be an amendment to the constitution of PAA regarding the election of officers. Before that time, the officers were elected by the Board of Directors. There was natural discontent among the people. Those who were against the change pointed out that this was the way business organizations did it; you elected the directors and they elected the officers. People were represented through the directors. But the people were naturally not satisfied with that, so they did manage to get that change in the constitution. Thereafter, the officers were elected directly by the membership and it was specifically provided that there should be at least two names proposed for all offices except for secretary.

The meeting in Cincinnati carried through Friday and then on Saturday morning, Warren Thompson had arranged for buses to come to take us from Cincinnati to Oxford, Ohio [location of the Scripps Foundation for the Study of Population Problems]. So, we had our Sunday meeting in Oxford, Ohio. And Thompson and Whelpton did a very good job as hosts; they treated the whole Association to a steak lunch.

LUNDE: Interesting that you should mention that. When I first went to NCHS, Harry Rosenberg and I took a trip out to see Whelpton, to see what he was doing for us on cohort fertility for a publication which later Arthur Campbell prepared for us. We were walking across the campus [of Miami University] and we bumped into Thompson; couldn't believe it. He was still spry and active and very anxious to talk to us. We had a wonderful luncheon with him.

What was the topic of your presidential address, Clyde? Do you remember?

KISER: I certainly do and it's somewhat relevant to this. It so happened that in 1953, if you take 1932 as the year of formal organization, the Population Association was 21 years old. So, the title of my address was "The Population Association Comes of Age." I gave a history of the first 21 years. It was published in Eugenical News [December 1953].

Incidentally, there had been one previous presidential address of that kind. In 1942 in Atlantic City when Whelpton was president, Fairchild himself gave a ten-year history of the Association. He told the story much better than I have about Margaret Sanger's role in the formation of the Association.

LUNDE: What was the early Association like in terms of its meetings and atmosphere and all that? For years the members met at Princeton. The first meeting I went to, when Kingsley Davis was at Columbia, was at Princeton. There was a certain flavor about meeting at the Inn there, which was certainly a lot different from what we have today, with increased membership and the big-town atmosphere of our meetings.

KISER: Yes. Well, as I said, the meeting in 1935 was our first large meeting, in Washington. The very next year, in 1936, we met at Princeton at the invitation of Frank Notestein, who had just gone down to Princeton the preceding September. This was a meeting in the fall, in October, as I recall. Notestein, as you know, went from Milbank to Princeton in 1936 to head the new Office of Population Research. Also took over Population Index, which previously had been Population Literature.

It was a small group. Of course, in those days there was only one session; everybody went to the same session. They had probably 100 people at that first meeting at Princeton. President

Dodds of the university opened the meeting and welcomed the guests. The Office of Population Research put on a little reception at the Princeton Inn; a cocktail party in the afternoon. For a while, every other year they'd meet some other place and then they'd meet at Princeton. The people seemed to like Princeton. It was fairly convenient. It was pointed out that Princeton was more or less in the center of the membership of the Population Association. Later, as we began getting more Western members, the center of gravity moved westward.

I believe the last meeting at Princeton was in 1954 or 55 [1955]. Margaret Jarman Hagood was the President and she had Carl Taylor, the agricultural economist, give a speech [in place of her presidential address]. After that the membership was pretty large, so they began meeting in other places. Now, of course, though we still have some way to go before we're the size of the [American] Sociological Association, we do have to pick the big cities to get a place to accommodate us.

LUNDE: Were the meetings pretty intimate?

KISER: Yes, they certainly were. In fact, I have mixed feelings on the Association getting too large. Everyone knew everyone else; everything was on an informal basis. I suppose one of the prices you pay for growing is what we witness--we're getting large.

LUNDE: To go back a moment, you were telling me earlier about your visit to the White House during the 1935 meeting and you said something about Rupert Vance being taken up in FDR's elevator. Tell us that story.

KISER: We were invited for tea, so immediately after the meetings in the Willard Hotel, we went over to the White House. Most of us started up the stairway there and a flunky came out and told Vance, who of course was on crutches, that they had an elevator for him. So they led him to a little elevator which Vance thought had been installed for President Roosevelt. This was the tea in 1935.

Well, about eight years later, in January 1943, there were about a dozen demographers that were invited to the White House, invited by Mrs. Roosevelt, and the invitation read something like this: that the recipient was invited to the White House for dinner on a given night in January 1943 to honor the work of Henry Pratt Fairchild. I happened to be one of those. I can't remember all of the names, but I do remember several. Mrs. Roosevelt and Vice President Henry Wallace were there. Orson Welles, Fairchild, Leon Truesdell, Frank Notestein, Professor Ed Hutchinson of Pennsylvania, and Warren Thompson; I distinctly remember some of the comments he made. I don't recall whether Whelpton was there or not.

We were first met in the hallway--we had our credentials with us and were able to get by the gate. We had a little reception, drinks, then went into State Room for dinner. I remember I sat next to Orson Welles. After dinner, Mrs. Roosevelt took us up the elevator to her private apartment, where she showed us a few rooms--up there on the next floor. I recall seeing the large photograph of Abraham Lincoln. After we got into her apartment, Mrs. Roosevelt settled down in a comfortable chair, took out her knitting, and told us that she didn't plan to say anything, that she wanted us to start talking while she listened. Fairchild acted as presiding officer. He outlined some of the outstanding population problems of the day as he saw them and tried to get the conversation rolling. I recall we got into questions of race, immigration, and urban-rural differentials in fertility. I remember that Thompson quoted one of his bits of wisdom in commenting on the fact that birth rates seemed to be low in cities. He said, "Well, with apartment-house dwelling it's a little like animals; you can't breed animals in captivity."

LUNDE: When you look back over the years, what do you think were some great moments of the PAA--some of the important things it either did or that it stood for?

KISER: I think the first great moment was this meeting in 1935. There were various government agencies that were concerned with population. You had the migration from farms to the cities; there was the Depression problem. So, I think this meeting in 1935 not only put the Association on its feet, it also demonstrated to some of the leaders the relevance of population to national government problems. [PAA's 1935 meeting of May 2-4 in Washington was called "Conference on Population Studies in Relation to Social Planning." PAA's third annual meeting was held on May 3.]

I think the next big peak probably came with the Second World War. Notestein's work at Princeton was something of which the Association can be proud. He involved the Association in the project he tried to carry out. Before the U.S. got into the war, between 1939 and 1942, Notestein had an invitation from the League of Nations to carry out studies of population trends in Europe and the probable trends after the war. The man who pushed Notestein was A. Loveday, director of the League of Nations Economic, Financial and Transit Section, which moved from Geneva to Princeton during the war. He became acquainted with Notestein's work and he commissioned him to carry this out. At several of our PAA meetings, Notestein and his coworkers would present their findings; they did that each year for two or three years. I think he always valued the advice he'd get from the members of PAA. Then after the war, Notestein had a similar invitation from the Office of the Geographer of the U.S. State Department to make similar studies of other parts of the world, the underdeveloped areas in particular [in Asia]. I think that era was one of which the Association can be proud. It was in that time that Notestein formulated the concept of the demographic transition.

From my own point of view, one of the most important things was the meeting of the IUSSP in New York City in 1961--important to me personally, because I served as chairman of the local arrangements committee. Dudley Kirk called me from the Population Council and asked if I would do this and said, "If you will do it, I for one will see that you get plenty of help." So, I did it under those terms. I thought I would have a lot of subcommittees that would help. So, for instance, we had a subcommittee on recreation for the group; we chartered a boat to go up the Hudson River. One duty I undertook was to receive the manuscripts that people submitted; any member of the Union can submit a paper. We accepted finally a total of about 130. The average length was about 20 pages. Some had to be typed over, they were all messed up; some of them had to be translated into English. We undertook to offset these papers after they were in shape and make 750 copies and send advance copies to the membership who'd expressed an intention of attending the conference. 130 papers, 20 pages each, 750 copies each amounts to a pretty large stack. We had very little room in the office [at the Milbank Fund], so I had to stack them up in my own little office. By the time the thing was finished, my office looked like an eskimo hut of papers, just a little path from the door to my desk. So I, naturally, derived a good deal of satisfaction out of that meeting.

Since that time, it would be difficult for me to say what are the important highlights of the Association. But the Association does try to discuss timely topics at its annual meetings.

LUNDE: What do you see as the future trend of the Association?

KISER: I don't know, but if you go by the recent past, I think we can look forward to a continuation of rather rapid growth, a continued increase in the dues, and to the necessity of meeting in large hotels in large cities. I think at this meeting here at most of the periods we have three sessions going simultaneously.

LUNDE: What were the papers at early meetings like as compared to those produced today? I have an impression that some of the early papers were perhaps philosophic and very broad in scope. Many of our papers today seem to be quite esoteric and pointed to a very fine area of investigation.

KISER: I think there's something to that. Back in the days of E.A. Ross, they were concerned with large topics of immigration, size of population, and things of that sort. I think E.A. Ross had great respect for the inductive method but he never did much in the way of scientific studies himself; he had the overall approach. So it was in demography. Take the very first PAA president, Henry Pratt Fairchild. He was a very good speaker; he needed no amplifier and he never needed any notes. Some people felt he traded a little too much on that ability. And Louis Dublin, Warren S. Thompson--they were all good men who could make a good speech without writing a paper. As you get on down the line, you find people who were interested in--interested in broad subjects, of course--but they tended to do research on small, narrow topics.

LUNDE:: What was your impression of Henry Pratt Fairchild as a person?

KISER: He was a very interesting man. I succeeded him at NYU, not as head of the department, but I began teaching the courses he'd taught there when he retired in 1945. I think the first time I saw Henry Pratt Fairchild was about 1929 or 1930. Warren S. Thompson was coming through New York at the time to go to Europe and old Professor Tenney told us that Fairchild and Thompson were having a meeting at some hotel at lunchtime and that his class in sociology was invited to go; so we did. Thompson was then a fairly young man. There was also a speaker there, a commissioner of immigration, I believe. He didn't know much about demography but he was under the impression that birth rates in Italy were increasing because he knew that the total number of births were increasing each year. Fairchild and Thompson assured him that the birth rates were actually decreasing.

Fairchild, as I said, had an important role in planning the Association. Incidentally, Fairchild was simultaneously the first president of the Population Association of America and of the Eastern Sociological Society, which was also formed in 1931.

LUNDE: What about the American Eugenics Society, when was that organized? You were active in that.

KISER: I was. [He was president in 1963-69.] The Eugenics Society goes back a long way. Some of its early history is not too savory today to some of the societies. If you go back to about the late 1920s, you'll find some highly respected people who were members of the American Eugenics Society. R.M. MacIver, I don't know whether or not he was an actual member but I recall that he wrote a testimonial praising the society for what it had done in connection with some abortion laws. The American Eugenics Society really had a pretty good start in getting across the immigration quota legislation of the 1920s [National Origins Act]. They would profess that they were not concerned with racial inferiority and superiority, but they put it in terms of the desirability of having immigrants of the same cultural background. But Madison Grant was one of the members of the early Eugenics Society. Not long ago I saw an old clipping of a New York Times review of Madison Grant's book and they praised it [The Passing of the Great Race, 1916?].

LUNDE: What about Louis I. Dublin? He was president in 1935-36.

KISER: He was the second president of PAA. Louis I. Dublin began writing articles for the New York Times. He'd have articles in the Sunday issue, on the aging of the population, so he was becoming pretty well known in the middle 1920s. Dublin was rather a curious fellow. He became second vice president of the Metropolitan Life Insurance Company. I suppose he was a very good administrator and he certainly put together--and sold to the Metropolitan Life--the work in population. And we should always give him credit for bringing Alfred Lotka to the Metropolitan. But they were a rather incongruous team. Dublin was the man who could sway audiences; he was not a technician.

Lotka on the other hand was a technician and he was a little timid about talking in public. But they got along well together, and Lotka himself would always defend Dublin. If you asked Lotka why Dublin's name came first on an article--one of their famous articles was "On the True Rate of Natural Increase" which came out in 1925 in the Journal of the American Statistical Association--if you asked Lotka why Dublin's name came first when you knew very well that Lotka had done most of the calculations, he would defend Dublin and say "We're co-authors."

LUNDE: How about Leon E. Truesdell [PAA president 1939-40]?

KISER: I first knew Truesdell in 1928 at the Bureau of the Census. I went to the Bureau of the Census as a dollar-a-year man in connection with a study I was carrying on. It finally terminated as my Ph.D. thesis at Columbia [Sea Island to City: A Study of St. Helena Islanders in Harlem and Other Urban Centers, 1932]. This was a study of Negro migration from St. Helena Island to Harlem and other urban centers. T.J. Woofter, Jr., was the man who headed up that project and he cooked up the idea that if I'd go to Washington and compare the names of the people enumerated on the island in a special census in 1928 with those on the census record for 1920 and if I'd take into account the death deficit, I could have a reasonable basis for knowing who had migrated from the island. So I did that. Truesdell was then a rather young fellow. He had black curly hair. He's still living; he's now, I guess, 93 or 94.

LUNDE: I had a letter from him just the other day, indicating his continued interest in the Association and in the development of the historical end of it and saying a few words about his recollections. It was fascinating to hear from him; I couldn't believe it.

KISER: Truesdell was a good administrator, I think. Paul Glick used to say that when he went to Truesdell's office [in the Bureau of the Census], he'd put his jacket on; he wouldn't go in there in his shirt sleeves. Truesdell was a good Republican; he came from New England. I believe he said that if Hoover had been re-elected he would have become Director of the Census. Hoover was not re-elected but Truesdell did stay on as chief statistician. As you know, he was [PAA] president in 1939-40. He gave his presidential address at the 1940 meeting down in Chapel Hill and he told about plans for the 1940 census.

LUNDE: The Association met two or three times in Chapel Hill [1940 and 1951]. What brought them to Chapel Hill, of all places?

KISER: Rupert Vance was in Chapel Hill [University of North Carolina]. Howard Odum was in Chapel Hill; he was not a member but he had Vance on his staff. And T.J. Woofter [PAA president 1940-41] had close connections with Chapel Hill.

There are some interesting things about meeting in Chapel Hill. We met down there in 1940, when Truesdell was President. The next time we met there was when Philip Hauser was president; that was in 1950-51. There was one well-known Negro who was a member of the Association who attended the meeting [of 1951]. And Odum, who was head of sociology and certainly no one could accuse him of being anti-Negro--he devoted his life to a study of the Negroes--but he was also a person who didn't want to defy convention too much, particularly if he thought it would hurt his cause. Now, he said, "We have one well-known man there; I won't mention his name unless you want me to," and they canceled the dinner. Daniel Price had arranged the dinner, but Odum told Price he better cancel the dinner. He said, "We'd lose more than we gain." Some people got pretty sore about that. Dorothy Thomas got sore; she would never go to the South again for a meeting until or unless they made suitable arrangements for the nonwhite members.

The next meeting in the South was in 1954 at the University of Virginia [Charlottesville] and Irene Taeuber was President that year. As you recall, we had a little trouble. There was one man from the West Indies there and the local arrangements committee had guaranteed us that they would have the racial question well in hand, that they would make suitable arrangements for the nonwhite; there'd be no discrimination. Well, actually when the chips were down, they had the same thoughts that Odum did; they might lose more than they gained. They got him a good room in a nonwhite neighborhood and, of course, the members wouldn't accept that and they threatened to boycott the meeting unless they put him in the same place that we were in. Well, I think this man told Irene Taeuber that he liked his room.

LUNDE: Do you remember who the chairman [of the local arrangements committee] was? We've had an inquiry about him; they want to give him an honorary degree and wanted to know about his Association activities. I was at a loss, because I couldn't find anything in the records.

KISER: It was Lorin A. Thompson. [Conrad Taeuber, who was more involved in this incident, says the PAA local arrangements chairman was Lambert Molyneaux. Conrad Taeuber letter to Daniel Price, July 14, 1982.]

LUNDE: His university wants to give him an honorary degree. I wrote to Hugh Carter who was [PAA] secretary at that time [1954] and Hugh told me that he remembered an incident at the meeting and he thought that Lorin Thompson had handled it quite well under the circumstances.

KISER: He did. He finally got this man put in the hotel where the rest of us were. [See further description of this incident in the 1974 Hugh Carter interview below.]

LUNDE: It's interesting that you should remember that, because other members do too. When did this whole business get cleaned up?

KISER: They had had an episode earlier, when we met in Atlantic City in 1942. There was a man who was finally affiliated with Howard University but he died several years ago; he was a promising demographer. [Lionel Florant. See Shryock interview below.] I think he was a student of Frank Lorimer's at American University. They gave this man a room in a private home, I believe, and Frank Lorimer went to the hotel man and said, "Now listen, you run this hotel, but I can tell you this, this man has good connections and he'll raise a stink." So the compromise--I think it was a compromise--was that he could have a room on the second floor of the hotel if he did not use the main stairs. Now, I don't know whether they accepted that, but that was the compromise handed out. They probably accepted it.

LUNDE: How times have changed. That's no longer a problem. But as you recall, a year ago in Toronto our ladies raised an issue over access to the men's bar. I don't know if we've got anything happening here [see Harriet Presser on "happenings" in New Orleans, 1973].

KISER: I notice we have a women's caucus this year. But I was thinking the other day that something had changed this year. For the last two or three years, we've had the group of young beatniks wanting more riots, but they're not here this year.

LUNDE: No, they're not. For a time there, it looked like they'd take over one of those meetings.

KISER: Up at Princeton University there's an old fellow who teaches history; he retired a year or two

ago. He was talking about changes in the students' attitudes in the last two or three years. He was bemoaning the fact that we no longer have the students who threw all the demonstrations and locked the professors out. He said he'd rather have that than the indifference he encounters now.

LUNDE: When I asked some of the ladies today at noon what they'd like to get involved with, they all wanted to go see the flood crest of the Mississippi; it's supposed to be the highest in 200 years here today. I'm just waiting to see what will happen at the business meeting tomorrow.

You knew Herbert A. Bloch, didn't you? He was my first instructor in statistics as an undergraduate, in 1932 at St. Lawrence University.

KISER: We had a seminar together at Columbia University. Frank Ross, professor of statistics, used to get the graduate students in sociology to come up to Hanover, New Hampshire, in the summer. It didn't take much urging. He told us, "There's no summer school at Dartmouth. I know I can get you a study in the library, it's cool up there." He did that for two or three summers. One summer there were several of us there. The Blochs were there; I was there; F. Galda, a fellow student from Romania who later became a priest; Bob Kutak and his wife; and John Innes. Ross lived over in Thetford, Vermont, 12 miles away. We'd go over to his place sometimes and have a steak fry. This was in 1929 and 1930. I think the Blochs were there in 1930.

LUNDE: Did you and Herb receive your Ph.D. together?

KISER: I think he received his in 1934. He was about two years behind me, in that it took him that much longer to write his thesis. And I'll tell you this, the first time I met you was at the Shoreham Hotel at a meeting of the Sociological Society. You were with the two Blochs.

LUNDE: I remember that very well.

KISER: And also I had the privilege of attending a service that you conducted for Bloch. That was a very touching service.

LUNDE: I've often thought that if Herb were alive today how interested he would have been in what has happened to the Population Association.

KISER: I liked his wife very much too; her name was Belle. The four of us went down to White River Junction, about three miles from Hanover, to see an old country stage show out in the open, "Uncle Tom's Cabin." One old man played two or three roles. Herb got quite a kick out of that. His thesis was about conflicting loyalties, wasn't it?

LUNDE: Yes, it was. Later on, he got very involved with social deviation--delinquency and social disorganization. I guess his books are still quite used.

Let me just ask you about T.J. Woofter, Jr.

KISER: He died only about a year ago. T.J. Woofter came from Georgia. His father was in the field of education. Woofter came to North Carolina in the summer of 1927 to teach sociology and cultural anthropology. Unfortunately, Woofter was addicted a little too much to the bottle; you've probably heard of some of the things he got into. He was a very astute man. People who knew him would say that he was the man who could put his finger on and state the nub of a problem, and if you can do that, you can begin to find ways to solve it. He worked with CIA a long time. He was once with the Southern Interracial Commission, which was the forerunner of what is now the Southern Regional

Council, devoted to Negroes. He was a good friend of Frank Ross of Columbia and they got together and hatched up the idea of studying the Negro culture in St. Helena Island. It was those two who rather shaped my career. I was at North Carolina in 1927. I planned to try for the Ph.D. degree there, but they got this study together and I got roped in on it. Then after I got the St. Helena, Ross and Woofter put their heads together and said, "Why doesn't Kiser come up to Columbia for a year and follow up some of these Negroes in Harlem and then come back down and get the degree?" After I got to Columbia and got my classes started, I just stayed up there.

LUNDE: When did you go to the Milbank Memorial Fund?

KISER: In October 1931. This was before that. The study of Negro culture in St. Helena Island started in the fall of 1927. I went down to St. Helena--that's right off the coast of Beaufort, South Carolina. Beaufort is between Savannah and Charleston. The Rosses used to go down there. That's the reason I went to Columbia.

LUNDE: What are you doing right now? You're supposed to be retired.

KISER: I retired January 1, 1971. But I've been trying to write a little history of the Milbank Memorial Fund.

LUNDE: When will this be published?

KISER: I've turned it in; I know there are revisions I must do. It will be published within a year or two [The Milbank Memorial Fund: Its Leaders and Its Work, 1905-1974, 1975].

C. HORACE HAMILTON, CLYDE V. KISER and JOSEPH J. SPENGLER

All interviewed together by Harry Rosenberg at the Carolina Population Center, Chapel Hill, North Carolina, December 15, 1976.

CAREER HIGHLIGHTS (see above for Kiser)

C. Horace Hamilton was PAA President in 1960-61 (No. 24). He was born in 1901 and died in 1977. 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.

Joseph J. Spengler, PAA President in 1956-57 (No. 20), was born in 1902 in Piqua, Ohio, and died in 1990 in Durham, North Carolina. He received all of his degrees in economics and all from Ohio State University: the A.B. in 1926, M.A. in 1929, and Ph.D. in 1930. He was Assistant and then Associate Professor of Economics at the University of Arizona in 1930-32 and 1933-34, interspersed by a year (1932-33) at Duke University, Durham, North Carolina. He returned to Duke in 1934 and remained there until his retirement, as Professor of Economics and eventually Director of Graduate Studies. His many publications in population economics dealt with such themes as the history of population and economic theories, migration policy, changing rates of fertility, and the concept of optimum

population.

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 PdoubleA 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. Woofert is another man I would meet at all the meetings and enjoyed contacts with him. In fact, Woofert 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 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. [as of 2020, see: <https://academic.oup.com/bioscience/article-abstract/23/10/595/237033?redirectedFrom=fulltext>]

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 Frank Notestein's interview.]

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 Taeuber 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 [June] Sklar and [Beth] 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. [Editor's note: Sklar and Berkov were correct, as it turned out.]

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.



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Appendix: Remarks at Dinner Honoring Clyde V. Kiser

Author(s): Francis H. Musselman, Frank W. Notestein, L. E. Burney and Clyde V. Kiser

Source: *The Milbank Memorial Fund Quarterly*, Oct., 1971, Vol. 49, No. 4, Part 2: Forty Years of Research in Human Fertility: Retrospect and Prospect. Proceedings of a Conference Honoring Clyde V. Kiser. Held at the Carnegie Endowment International Center New York City, May 5-6, 1971 (Oct., 1971), pp. 239-247

Published by: Wiley on behalf of Milbank Memorial Fund

Stable URL: <http://www.jstor.com/stable/3349474>

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REMARKS

Francis H. Musselman: It is a distinct honor and privilege, and a high point in my nearly 20-year association with the Milbank Memorial Fund, to have a part in tonight's program. Indeed it would be great to be attending this dinner in honor of Clyde Kiser but sitting on the sidelines instead of substituting, as I am, for the Fund's Chairman, Sam Milbank, who could find no way to be here tonight. Sam Milbank's unhappiness at not being here tonight to do honor to Clyde is compounded by Sam's knowledge of the perils of using for the execution of an important play, a second string quarterback.

It would be presumptuous of me to recite to you a synopsis of Clyde's professional accomplishments. You might say with some justification that I am not competent to comment thereon in front of such a distinguished group of professionals. Thus, I will leave this pleasant assignment to Frank Notestein and to Lee Burney. On behalf of the Board of Directors and the entire family of the Milbank Memorial Fund, I can attest to Clyde's contribution to the Fund, an organization dedicated to the betterment of mankind. While much remains to be done, we are all better off—indeed the world is better—for the many years that Clyde has labored in his chosen field and the Milbank Memorial Fund has been privileged to have played a part in Clyde's successes.

It is entirely fitting that on this occasion, in recognition to Clyde, that you permit me to take a few minutes of your program for the presentation to Clyde of an illuminated minute of the Resolution of the Board of Directors of the Milbank Memorial Fund adopted at its meeting on December 10.

RESOLUTION

Upon the occasion of the retirement of

CLYDE VERNON KISER

as Vice President for Technical Affairs

The Board of Directors records its appreciation for his distinguished service to the Milbank Memorial Fund over the past four decades.

Through his loyal dedication and his unwavering standards of scholarship, Doctor Kiser led the Fund in its pioneering association with methodological innovation in demographic science. His contributions to the study of Knowledge, Attitudes and Practices affecting fertility are universally acknowledged, and have brought great distinction to the Fund and advancement to his profession.

Doctor Kiser has shared generously of his knowledge and the fruits of his industry. In his authorship of several honored volumes, and six score articles, in his contributions as encyclopedist, editor and critic, he has both informed and inspired students and colleagues around the world. The qualities of warmth and wisdom, and the ever-present gentleness of wit, have suffused his pursuit of excellence in service to the Fund through five successive executive directors. For ourselves and our predecessors, we salute with gratitude and respect this gentleman from the South.

Adopted 10 December, 1970

Samuel R. Milbank, Chairman
Francis H. Musselman, Vice Chairman
L. E. Burney, M.D., President
George Baehr, M.D.
Cornelius N. Bliss, Jr.

Alexander D. Forger
C. Glen King, Ph.D.
R. Bruce McBratney
Thomas I. Parkinson, Jr.
Charles E. Saltzman
John S. Baugh, Secretary and Treasurer

Frank W. Notestein: My word to Clyde tonight is, "Come on in, the water's fine." One hears a lot of worry about what to do when retired. There is simply no trouble about that. Like most professional people, I am sure Clyde has mainly done what he wanted to do all his life. From now on, Clyde, you do it in spates—just what you damned please. If you want to work, who's to stop you—no committee meetings or conferences or protocol interviews. You won't even have to play the perennial foundation game of finding new and softer ways of saying "no" to grant applicants. If you don't want to work, who's to make you? All your life you've said *only* the things you've wanted to say. But there is a difference; now you can say *all* of the things you want to say. You have been lucky and virtuous enough to live with few imposed constraints. Now you have none, if you are wise enough to stay away from those sometimes nasty medical characters. My interpretation of retirement is that the pressure is off. Now you do as you please and when you please. Now you can even tell yourself to "go to Hell."

I have only seen Clyde, the most modest of men, show pride twice, and each time greater justification would be hard to find. The first was in 1932, when he read a review by a distinguished scholar of his first book, *SEA ISLAND TO CITY: A STUDY OF ST. HELENA ISLANDERS IN HARLEM AND OTHER URBAN CENTERS*. In it our young Southerner read that the Negro reviewer had continually tried to decide as he read whether the author was white or Negro, but had not been able to make up his mind.

The other time was when he invited my wife and me to be the witnesses when he married Louise Venable Kennedy, whom only a few months earlier he had casually described as "that Miss Kennedy I met at Columbia," and whom Daphne and I later also came to love and admire as close friend and colleague at Princeton.

There is simply no point in dwelling to this audience on Clyde Kiser's virtues. All of you know them as well as I do, and Clyde won't believe it when he hears them. As a man not prone to reckless conclusions, or snap judgments, and vastly suspicious of partisan opinion, he would on all counts conclude that the evidence is neither adequate nor persuasive, and that all conclusions about his virtues should be postponed for referral to a much higher court—if any.

And, since we can't talk about virtue, it's a shame that our roles are not reversed this evening so that Clyde had to talk about me instead of vice versa. He can remember at least a hundred follies of mine for every one I can remember of his. I'd like to hope that this ratio relates

less to the actual past than to the selective sampling at which my memory becomes progressively more efficient.

I am afraid in those early 1930's both Clyde and I gave Dorothy Wiehl, who was in charge of us, disciplinary as well as instructional problems. We had moved from 49 to 40 Wall Street. At 49 Wall our room was on an ill-ventilated court with pigeons for scenery. We worked with small temptation to look out. But then when the Manhattan Bank Building opened we moved to the 49th floor with its magnificent view. We worked a little but, until Miss Wiehl put a stop to it, our most ingenious efforts were devoted to trying to sail paper darts to Brooklyn. To put the best face on the matter, it was there that we first learned to answer the physicist who was bragging on the superiority of his science over ours in its predictive capabilities. We asked him to tell us from his science whether when we opened the window and threw out a piece of paper it would go up, down, sideways or all three. Of course the physicist ultimately won by producing air conditioning that seals the windows.

Jumping to conclusions with firmly stated opinions is not one of Clyde's vices. If his opinion is rather more definite than on the surface seems to be justified, somewhere under the surface sleeps the proposition that two plus two equals four. I remember we were discussing racial differences in the early 1930's. I don't remember the context, only the locale—it was the washroom. Probably as an impudent damn-yankee, I was suspiciously testing this young Southerner for racial prejudice. In any case I wondered whether there were significant inborn differences between Negroes and Caucasians. "Probably," said Clyde. "What," said I, ready to pounce for the kill. "Well," said Clyde, "There's skin pigmentation."

I must say that Clyde's incorrigible insistence on limiting his views to conclusions buttressed by carefully analyzed data is a major factor in the present state of our knowledge about differential fertility. It's hard to remember that when Clyde started, Pearl was still enamored with his biological law of population growth, and Gini was proposing differential fecundity as the engine causing civilizations to rise and fall. I recall that one meeting of the Sociological Society did not even crack a smile when a distinguished academic advanced the opinion that the concrete cliff dwelling of city life was so shielding us from ultraviolet rays as to induce sterility. Of course when Sociologists talk physiology they are even more likely to be wrong than the physiologists.

Slowly, carefully, with small fanfare and absolute integrity, Clyde,

whether working alone or with his colleagues, Whelpton, Grabill and Campbell, has been revealing the truth. Because new truth asks new questions, much remains to be learned, but meanwhile much nonsense has been discarded. The real story has progressively emerged thanks in large part to Clyde's intelligent, persistent and careful research lucidly and unpretentiously described. What the world knows today about differential fertility is in no small part the result of Clyde V. Kiser's work.

But now, Clyde, that you have been promoted, when anyone asks you about differential fertility, you should feel perfectly free to say, if you care to, that you are in favor of it. Age involves few verbal constraints. And so I close giving you the well-earned and affectionate admiration of the demographic fraternity, and a warm welcome to the new freedom.

L. E. Burney: I am happy to join with you, as friends and colleagues of Clyde Kiser, to honor him with this dinner. It is most fitting and proper that we do this. Clyde has honored us throughout these many years by his warm personal friendship and his distinguished scholarly contributions to population research and knowledge. He has brought honor and integrity, humor and urbanity to every task assigned to him.

Rich in experience and honors, he has served with distinction and success for forty years as a member of the staff of the Milbank Memorial Fund. It is one of the contributions that a Fund such as ours can make in providing an intellectual environment with freedom for the growth and development of individuals with new ideas, innovative approaches, flexibility of action and time for scholarly thought. He has utilized these freedoms faithfully and well and all of us are privileged to share in the full and rich life he has achieved.

Clyde has agreed to serve the Fund as a Consultant in Demography for a period after his retirement. His primary task will be to prepare the history of the Fund's involvement in population research and studies, and, second, to update the history of the Fund for the past twenty-five years. You will agree he is admirably well qualified to do this through his intimate and personal involvement with the programs and personalities.

Clyde will miss the daily commuting between Princeton and New York—but not very much. He will have a little more leisure time for thinking and writing. At the same time, we will continue to have the benefit of his wise counsel and assistance as well as his exceptional kindness, humor and equanimity.

It is said that a man's secretary is an able, if not the best, judge of his personal qualities and character. I should like to read the comments of Miss Betty Vorwald, Clyde's secretary for nine years:

I have worked for Dr. Kiser for about nine years and the essential quality that stands out about him is his kindness to everyone, combined with great humility. Whenever I made a mistake he usually accepted the blame by saying that perhaps he didn't explain himself too clearly. There was nothing about him of superiority or indifference to others, but always a ready sympathy, an understanding and a way of making people feel at ease. With all his friends and visitors he was always courteous and ever ready to be merry with a little joke, yet he had great authority in his work. He knew what he wanted and in a very calm, gentle way accomplished so much without stress to others or to himself.

Truly, he was a joy to work with.

Again, we join with you in wishing for him a continuation of his rich and full life and thank him for his friendship, which has so enriched our lives. Would that more of us could have been so effectively "involved in mankind."

Clyde V. Kiser: Mr. Chairman, Dr. Burney, Dr. Notestein, Mr. Baugh, Fellow Demographers, Fellow Former Staff Members and Staff Members of the Milbank Memorial Fund.

Mr. Musselman, first of all I would like to thank you and the other Members of the Board and the Officers of the Milbank Memorial Fund for what they have done for me on the occasion of my retirement.

I knew in advance about some details of the dinner tonight. I knew that a program containing my photograph and the names of the guests was being prepared, but I did not see it until tonight. A typewritten copy of the citation was given to me in December but this is the first time I have seen the hand lettered illuminated copy done by Katherine C. Gensamer of the Fund's staff. It is a beautiful thing, and I shall always treasure it.

When Dr. Burney suggested a few months ago that we plan a small Conference and a dinner to commemorate my retirement from the Fund I was well pleased. Nevertheless, I was also reminded somehow of Julie Andrew's song in the recent movie, *Mary Poppins*—"A spoonful of sugar makes the medicine go down."

However, my retirement became effective January 1, 1971, several months before the Conference, and I can report that the medicine, even without the sugar, has not been bad. I can also vouchsafe now,

however, that perhaps partly because the medicine itself has not been bitter the sugar has been exceedingly sweet.

If it is a symptom of age to forget things that happened recently and to remember things sharply that happened a long time ago, I am beginning to qualify for admission to the category of the aged. I am sure that Miss Betty Vorwald can attest to my forever forgetting where I put things that cross over my desk. And yet I can remember well the details of my coming to the Offices of the Milbank Memorial Fund first for an interview and later work as a Milbank Memorial Fund Fellow in 1931.

I was spending the summer of 1931 in Hanover, New Hampshire working on that perennial problem of the graduate student, the thesis. Frank Ross, Assistant Professor of Statistics at Columbia University, was the faculty sponsor of my thesis, a study of Negro migration from St. Helena Island, South Carolina to Harlem and other urban centers. Professor Ross had a summer home near Thetford, Vermont and without much difficulty he induced several graduate students in sociology at Columbia to spend a month or two in Hanover, twelve miles from his home. The climate, of course, was usually delightful in the summer. There was very little in the way of summer school at Dartmouth at that time. Consequently, we could not only find good living quarters easily but Professor Ross, through his contacts with several faculty members at Dartmouth, managed to secure for us not only stack privileges at the library but also the assignment of faculty study rooms.

It was while I was in Hanover that I received a letter from Professor Robert E. Chaddock, Professor of Statistics at Columbia University, under whom I had taken courses. He enclosed a letter that he had recently received from Frank Notestein telling him of the possible availability of a Milbank Memorial Fund Fellowship for a year. The chief duty of the Fellow would be that of analyzing a sample of 1900 Census data on children ever born in relation to occupational class of the husband. Professor Chaddock suggested that I write directly to Dr. Notestein for an interview if I was interested.

After talking the matter over with Frank Ross, I did write and about two weeks later took an overnight train from White River Junction, Vermont to New York for the interview. I had an early afternoon appointment with Frank Notestein and Mr. Edgar Sydenstricker, the Director of the Division of Research. I suspect they were scraping the bottom of the barrel for shortly after I returned to Hanover, I received

a kind letter from Frank Notestein stating that I had been awarded the Fellowship.

I began work at the Fund in October, 1931. Besides Frank Notestein, three people in this room were members of the staff when I started. One was Miss Dorothy G. Wiehl, who was Assistant Director of the Division of Research. The second was Mrs. McGuire who was then Helen Slane, a private secretary, who later succeeded Miss Catharine Doran as Secretary of the Fund. The other was Miss Katharine Berry, who had been the Milbank Memorial Fund Fellow the previous year and at the time was a statistical assistant at the Fund.

The year 1931 doesn't seem very long ago to me and probably not to the several others here of roughly my age. However, as a matter of arithmetic it was 40 years ago. That is a long time—two score years.

In October, 1931 Herbert Hoover was President and he was having trouble with the Great Depression. Apple sellers were a common sight even in the financial area. Jimmy Walker was Mayor of New York but he was having his trouble with the Judge Samuel Seabury investigations. The Empire State Building had recently been completed and there had been an unsuccessful attempt to hitch a Blimp to the spire of the building. This intended function of the spire had been publicized but, according to report, the unforeseen wind currents of the area caused an abandonment of this possibility.

In that period we also witnessed the brief start and abandonment of large dirigibles as means of long-distance passenger transportation. During my first year at the Fund some members of the staff and other office workers at 49 Wall Street went to the roof of that building one day to see the Shenandoah pass over on a maiden voyage to South America. Later, on May 6, 1937, several of us at the Fund saw the dirigible Hindenberg passing over Manhattan on the way to Lakehurst, New Jersey where it burst into flames and burned during the attempt to moor it to the mast.

I mentioned that the Fund's office in 1931 was at 49 Wall Street. This was the Old Atlantic Building at the southwest corner of Wall and William Streets. The building had hydraulic elevators that even then were considered antique. They were slow and noisy but like the Model-T Fords, which were still fairly common in 1931, they were probably more reliable than the modern automatics. However, the elevator cars were not completely closed. Pigeons frequently got in the elevator shafts and into the elevator cars themselves. Now it happens that Frank Notestein has never liked pigeons or anything with feathers

that flutter. If he happened to be in an elevator car with a pigeon, he would break out with hives. He would also break out with hives when the dates of our Annual Conferences or the time for a speech drew near. When Frank arrived at the office with hives we knew that either a speech was coming up or that he had had contact with a pigeon in the elevator.

This morning I tried to give a little history of the Fund's work in population. To keep this in perspective, I would just like to mention the broad aspects of public health with which the Fund has been concerned. These are the pioneering health demonstrations, medical care, nutrition, housing, mental health, medical education, and it has been rumored that we may go full cycle back to medical care. I am the only person in captivity, I suppose, who has served under the five directors of the Fund: John A. Kingsbury, Edgar Sydenstricker, Frank G. Boudreau, Alexander Robertson and L. E. Burney. It has been an exciting life. One playwright, I believe, had Julius Caesar say that if he were not Caesar he would like to be a University Man. Probably some of the university people here would agree. However, if I could repeat my professional life I believe I would again cast my lot with a foundation.

It has been my privilege to work for forty years in a field that I liked. It has been my good fortune to be associated with an organization that I liked during this period. Probably most important it has been my privilege to work with people whom I liked both in the Milbank Memorial Fund and in the field of Demography. I want to thank the officers and the staff of the Milbank Memorial Fund for all they have done for me over the years. I want to thank my colleagues in demography present here for their varied types of professional and personal help over the years. I want to thank each and everyone present for making this dinner an occasion that I will always remember.

Although we do not have a full version of Clyde Kiser's presidential address at the PAA meetings in 1953 in Cincinnati, Ohio, this summary of those meetings includes a brief overview of his talk--see below on page 182.

The 1953 Meeting of the Population Association

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Source: *Population Index*, Vol. 19, No. 3 (Jul., 1953), pp. 175-183

Published by: Office of Population Research

Stable URL: <https://www.jstor.org/stable/2730919>

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CURRENT ITEMS

THE 1953 MEETING OF THE POPULATION ASSOCIATION

The annual meeting of the Population Association of America was held Saturday, May 2, 1953, at the Sheraton-Gibson Hotel, Cincinnati, Ohio, and Sunday, May 3, 1953, at Miami University, Oxford, Ohio. There were four sessions of papers and discussions, a business meeting, and a dinner meeting. At luncheon on Sunday those attending the meetings were guests of the Scripps Foundation for Research in Population Problems at Miami University.

First Session: Metropolitan Areas

Donald J. Bogue, Chairman

This session was concerned with different aspects of the distribution and movement of population in metropolitan and urban areas.

Leslie Kish of the University of Michigan discussed the differentiation of community units within metropolitan areas in his paper, "Differentiation in Metropolitan Areas." The central hypothesis was that communities in metropolitan areas are differentiated from each other in inverse relation to their distance from the central city. Distance zones are taken to represent gradients of influence of the central city and integration with it. The hypothesis that communities in the inner metropolitan zone show more differentiation than those in the outer metropolitan zones tends to be confirmed in twelve metropolitan areas studied, but not in the New York area. The amount and gradient of differentiation vary with the population characteristic used. The curve of differentiation (plotted against distance) is not linear and it flattens out past a fairly sharp "boundary of influence." The maximum differentiation is at about 15 or 20 miles for the largest cities and within the 5 mile zone for the smaller cities. The results of the investigation are not conclusive tests of the hypothesis but are generally consistent with it. Intra-class correlation was used as the measure of differentiation. It may have wide utility in ecological studies.

"Urban Daytime Population: A Field for Demographic Analysis" by Donald L. Foley of Columbia University was concerned with problems arising out of the marked discrepancies in the distribution of daytime and nighttime populations. Spatial segregation of residential and other activities in specialized areas requires a massive daily circulation of population between such areas. These movements can be studied usefully with respect to purpose of movement and of the general functional areas between which trips are made. In terms of purpose, the most numerous trips are the "journey to work." Social or recreational trips and trips for shopping are, respectively, second and third in importance. In the "journey to work" the length of trip is directly related to size of city, trips are predominantly to the Central Business District (CBD), work trips to the CBD are longest, and, apparently, workers strive to minimize the length of the work trip. In terms of functional areas, the predominant weekday movement pattern is toward central areas in the morning, with a reverse flow beginning in the late afternoon. This centralizing movement is less for large cities and those with strongly dis-

persed industrial areas. Recent dispersal of population has apparently only diminished the massive movements to central areas. With the increasing theoretical and practical interest in these movements there is need for both the theoretical formulations and more systematic collection of data, especially through official statistical agencies. The most important data needed are cross-tabulations of the origin and destination of daily trips for a variety of areas and time periods. This area of research may become a standard part of the work of demographers.

The last paper in the session was "Research on the Rural-Urban Fringe: A Case Study" by Samuel W. Blizzard of Pennsylvania State College. It dealt with the delineation of the fringe area of Williamsport, Pennsylvania, and the characteristics of the fringe population, according to place of origin. The "fringe" was defined as the area of mixed urban and rural land uses between the point where full city services cease to be available and the point where agricultural land use predominates. The fringe is linked to the city by the automobile as distinguished from suburban areas, for which the railroad is the transportation link. The outer boundary of the Williamsport fringe was based on a personal inspection determining the points where agricultural land uses began to predominate. In a sample interview survey the fringe population was found to be socially differentiated according to differing place of origin: local-urban, local-rural, and non-local origin. In only two of nineteen social characteristics were these groups closely similar. The local-rural origin groups had poorer housing, education, income, and jobs. The non-local origin group had a relatively higher status position based on these same social characteristics, and there is evidence that they are a mobile group, beginning careers.

In discussing the paper by Kish, Calvin F. Schmid of the University of Washington indicated that Kish had ingeniously applied a valuable statistical tool widely used in experimental design to the problem of ecological differentiation. Schmid then called attention to a number of problems, including discrepancies of definitions with those in standard use, criteria for selection of significant variables for the analysis, selection of metropolitan areas without a probability basis, omission of data for non-incorporated areas, possible failure of the data to meet assumptions of normality, weighting of data for communities of different size, definition of the central point for metropolitan areas, and the overlapping influence of differing metropolitan areas. In the absence of time to discuss these problems, reference was made to material bearing on them in a larger study from which the paper by Kish was prepared.

Leo Schnore of the University of Michigan noted that all three of the papers have a setting in a larger urban problem complex. Also, they have in common the dilemma of a choice between extensive official statistics which are not flexible and flexible but less widely comparable data gathered for a particular case. Thus, the daytime population data cited by Foley tend to be drawn from a sample of cities biased toward large size, industrial development, and severe traffic problems. Schnore strongly supported Foley's advocacy of official systematic collection of data on "place of work." As a case study of the rural-urban fringe, Blizzard's study has unique flexibility but is not comparable with other potential studies. The urbanized areas used in the 1950 census were suggested as a basis for uniform fringe studies. Data from all of these support the view that the organization of the metropolitan community is characterized by multilateral relations between the center and outlying units as a complex organized whole.

Second Session: International Population Movements

Warren S. Thompson, Chairman

The first paper, by Joseph J. Spengler of Duke University, considered the question "Is There a Tendency to Overpopulation in Human Societies?" A series of graphs was used to show possible relationships among the variables involved. Spengler first discussed the problem of relating the growth of output to the growth of the labor force and the stock of capital goods, and next, the question of the welfare implications of various output-population size conditions, when values and tastes are taken into account. The conclusion emerged that in many cases, despite supposed increases in per capita income that have accompanied population growth, per capita welfare may have decreased. Two factors tend to obscure this adverse effect, the fact that increases in population may destroy the possibility of knowing of superior welfare situations that might have existed had the population not grown, and the fact that increases in per capita national income that are really attributable to capital accumulation and the operation of increasing returns of non-demographic origin may be attributed erroneously to increases in population density. This tendency of dynamic changes to mask the effects of population growth is intensified by the difficulty of discovering what welfare index is to be maximized and its relation to variations in population size. Although the available data do not at present permit precise empirical resolution of the problem treated by his paper, Spengler suggested that the observed continual tendency of populations to grow in actual size and the unlikelihood that the optimum size of a population increases continually in corresponding fashion may allow the inference that under many empirical conditions a tendency toward overpopulation does exist in human societies.

"Can Overseas Immigration Provide Practical Relief to European Problems of Population Pressure?" was the subject of a paper written by Dudley Kirk of the Department of State, and read by Harold F. Dorn. Distinguishing two senses in which population pressure exists, the absolute ratio of men to land and the relative sense of differential population pressure between countries and between continents, Kirk addressed himself to the latter, as being the more significant for problems of international relations. After a résumé of the extent of population pressure in the countries of Eastern, Southern, and Northwestern Europe, Kirk turned to the question of emigration needed in the future. There is no very satisfactory method of defining the need for emigration. Using as a primary standard relief from any additional demographic pressure on the labor market and on the land leads to an estimate of about 705,000 per year as the current need for overseas migration from Europe. This need is declining at the rate of about 50,000 per year as a result of past declines in the birth rate. In 1951 net emigration from the countries in which the need exists was about 282,000. However, the total net emigration from Europe since the war has averaged 443,000 per year, of which roughly 200,000 were displaced persons. Thus, if emigration from countries of population pressure can be substituted for emigration of displaced persons (now largely liquidated as a source of emigrants), this and existing migration would meet over half of the current need, and if maintained might well meet all the need five years hence. The conclusion results that although difficulties will vary considerably from country to country, the relief of population pressure through overseas migration should be a realizable goal.

The third paper, by Harry N. Rosenfield, formerly Executive

Director of the President's Commission on Immigration and Naturalization and Commissioner on the United States Displaced Persons Commission, discussed "Basic Issues in American Immigration Policy." Beginning with the premise that there is only one basic issue in American immigration policy, namely, what is good for the United States, Rosenfield discussed the present United States immigration law from several points of view. In the area of international issues he found that the present law imperils our security because it has been formulated without consideration of its major and unfavorable impact upon our foreign relations. The principal domestic issue, as seen by Rosenfield, is that immigration is necessary for a strong domestic economy. Other issues arising from the present law are those of fairness and justice, and the administrative issues associated with the fact that the law is administered by two separate departments of the Government, State and Justice. Noting that most of the American people, as well as many members of Congress, have very little idea of what is in the present immigration law, Rosenfield concluded with a plea for a debate of the facts instead of name calling, and stated his opinion that the best interests of the United States require a complete rewriting of the law.

The discussant for Spengler's paper was Philip M. Hauser of the University of Chicago. Referring to the recent revival of discussion along Malthusian lines, of which Spengler's paper was an example, Hauser suggested two reasons for this trend in demographic thinking: the dramatic problem of population growth with which we are now confronted, and the deficiency of demography with respect to theory. Spengler's contribution goes beyond the recent literature by making clear the difference between an index of net per capita income and a welfare index taking account of values and tastes. Both he and Malthus provide a rationalistic approach without much recourse to the facts, however. If analytical thinking of the type exemplified by Spengler can be combined with empirical studies, there is hope for progress.

The second discussant of the session was Max Lacroix, who commented upon the papers by Rosenfield and Kirk. Lacroix observed that Rosenfield took a long-run view of the best interests of the United States and congratulated him for not evading the emotional issues involved. The chief weakness of Rosenfield's presentation seemed to be that it left the positive side of the matter to the listeners. If the underlying issue is the well-being of the United States, this still leaves unanswered the question "What is the best size of population for the United States?" The paper by Kirk represented an outstanding contribution to one of the most baffling areas that have faced demographers since the end of the Second World War, and put in proper perspective the emigration problems facing the several countries of Europe. Lacroix wished to emphasize somewhat more than Kirk had that however reasonable it is to use the criterion of additions to the economically active population to estimate needed emigration, this is nevertheless taking a partial view of the situation. Further, one should look into the difficult problem of the measure in which it is desirable to move resources to people rather than people to resources.

Third Session: Demographic Changes and Economic Development

Dorothy S. Thomas, Chairman

"Some Interrelations of Population and Atomic Power" were discussed by Vincent H. Whitney of Brown University. Although the tech-

nical problems of atomic power are outside the competence of most social scientists, the basic economic problem, production of power at a cost competitive with other sources, and the basic social problem, overcoming cultural resistances to its use, are already familiar to demographers in other contexts. Factors that have impeded the development of conventional power will be important also for atomic power. The principal significance of population for the development of atomic power is as a limiting or permissive element. In underdeveloped areas the potentialities for population growth at a rate in excess of any achievable rate of technological growth and the many social, psychological, and other factors connected with this situation constitute major obstacles to the use of atomic power. In general, active utilization of atomic power is probable only in "Westernized" nations, which have or can get capital, entrepreneurship, trained labor, and adequate markets for plants of optimum size, and in which the demographic pattern is not a hindrance for development. Turning to the implications of any utilization of atomic power for alterations in population patterns, Whitney observed that it is doubtful that atomic power spells any marked population changes for the great majority of areas. In nations that have undergone some substantial degree of urban industrial growth, it may be that the use of atomic power will heighten the concentration of population in leading industrial areas and further the dispersion of population within such local concentrations.

The discussion of Whitney's paper prepared by George J. Stolnitz of Princeton University was not presented because of a shortage of time but will be summarized here. Stolnitz first thanks Whitney for putting the question of atomic power in such a way that social scientists can understand and hope to profit by it, and for bringing to bear on a problem so filled with uncertainties a wide range of social, economic, and demographic considerations. With respect to Whitney's conclusion that the significance of atomic energy for economic development in underdeveloped areas is likely to be fairly limited, Stolnitz suggests the additional point, not pressed by Whitney in his paper, that the Western technology with which atomic energy would have to be used probably cannot be transferred wholesale to the underdeveloped world. Moreover, the uncertainty of the burden of financial insurance against the risks of radioactivity, recently a prominent problem in the United States, may be an even more serious impediment to development in areas with very little insurance experience in general. A more balanced view of the whole atomic energy situation might be obtained by considering not only obstacles to the use of atomic power but such other factors as the benefits to underdeveloped areas of atomic power in industrialized regions; the benefits to an economy if atomic power proves to be adaptable to only a segment thereof, such as transportation; and the fact that analyses comparing the expected costs of atomic power in underdeveloped areas with the present cost of conventional power in areas where such power is well developed may not yield the conclusions that would result if the attempt were made to compute the cost of conventional power in the underdeveloped areas themselves. Whitney's emphasis that industrialization induced by atomic power may be abortive because of the implications of the demographic structure of an area is the by now classical line of analysis. Stolnitz addresses to demographers the suggestion that the traditional demography of transition, though highly relevant to many situations, should be supplemented by a demography of adaptation—of "breathing spaces"—that would deal with adjustments to unfavorable demographic factors. We may be in danger of diminishing returns in stressing demographic impediments to the extent we do.

The paper by Harvey Leibenstein of the University of California,

"Optimum Population Theory Once Again," began with the observation that optimum theory, originally developed to counter the seemingly extreme Malthusian viewpoint, had appeared rather dead a few years ago but has of late shown signs of revival. Whereas the attempt to determine the optimum population size as such may be hopeless, useful results can be obtained by focusing attention on whether the existing population trend is toward or away from a local optimum. The major dynamic changes likely to affect the optimum size of population are capital accumulation, innovation, and changes in the terms of trade. A discussion of these factors yielded the conclusion that in many situations the optimum population generally moves toward larger population size. In the light of the presumption that a useful theory must be operationally significant and lead in at least some cases to quantifiable results, Leibenstein proposed a possible scheme for attempting to determine whether the population is approaching the optimum or not, directing his attention, as a first step, toward the size of the labor force rather than the size of the total population.

"Economic Problems Arising from America's Postwar Population Upsurge" was the subject of a paper by Joseph S. Davis of Stanford University. Davis began by summarizing material he has presented in earlier articles to indicate that the population outlook of the United States at this time is quite different from that which was anticipated on the basis of estimates and forecasts made during the 1930's and 1940's. In the light of up-to-date data Davis believes that no forecast of our population for 1975 or 2000 can be trusted, but that the prospect for the next few decades is one of continued but variable growth. The economic problems that we face will be those of a highly dynamic expanding economy in which changes in age and regional distribution will be no less important than changes in total size of the population. A consideration of such problems as resources and costs, agriculture and food supplies, over-all productivity, educational requirements, adjustments to prospective waves of population increase, the changing labor force, and the raising of levels of living led Davis to a generally optimistic assessment of the future. A specific suggestion offered was that the Bureau of the Census publish projections by single-year age groups of persons already born, for a considerable number of years ahead. These would provide valuable information and would be reasonably reliable in comparison with projections for the total population.

The discussion period that followed was concerned principally with the success of past population projections and the possibilities for future work in this field. With respect to the past, the points were made that the population projections which have thus far proved to be in error were the results of careful work and have gone wrong for reasons that no one could have predicted at the time; that the results would show up less badly than in Davis' presentation if the full range of high and low projections rather than just the medium projections were emphasized; and that in any case the projections of demographers have been no worse than those of the economists. It is perhaps too early to say just how bad the projections already made for the future will turn out to be. Although a considerable degree of pessimism pervaded many of the comments with respect to the possibility of making predictions in the field of behavior in general and demography in particular, there was a reluctance to be altogether discouraged. The suggestion was made that demographers should devote less of their energy to attempting to satisfy the public demands for projections and more to a study of the cultural and economic factors related to population changes.

Fourth Session: Contributed Papers

Howard W. Beers, Chairman

The opening paper of the final session by T. Lynn Smith of the University of Florida was entitled "A Study of the Reproduction Rate in Latin America." Smith pointed out that the historical paucity of data for Latin America is ending. With the taking of the Census of the Americas in 1950, Latin America is now one of the large areas of the world for which demographic data are becoming adequate. This is especially important because it is an area of unusually high reproduction rates. Most Latin American census data are presented as collected and tabulated. However, in some cases "adjustments" of the data complicate their use. The value of recent Latin American censuses for differential fertility studies has been increased by cross-tabulations of age and sex, by classifications of rural-urban, race, color, and ethnic background. Vital statistics data on births are available for most Latin American countries, but they are generally less accurate than recent census data. For most of these countries coverage is designated as "incomplete" in the United Nations Demographic Yearbook. Fertility ratios are probably a better measure of fertility than the birth rate for most Latin American countries. The recent data permit some preliminary statements on Latin American fertility. Crude birth rates are probably 45 for most countries and in some cases reach 50. Rural-urban fertility differentials are markedly in the direction expected previously, but are adequately documented for the first time. Preliminary data also confirm the view that whites have higher reproduction rates than Negroes.

"Patterns of Family Limitation in a Rural Negro Community" by Christopher Tietze of the Department of State and Sarah Lewit of the National Office of Vital Statistics was the next paper. It dealt with a study of the reproductive histories up to 1950 of 357 Negro women in rural Maryland. Nineteen per cent of the couples were currently using contraceptives or had used them just prior to a current pregnancy. This is considerably less than the proportions reported in studies for different populations in the United States and Great Britain. Analysis by year of birth of the wives shows a trend toward an increasing use of contraception at younger ages in the last three decades. Among methods of contraception there is a high incidence of douching and a low incidence of withdrawal—a pattern which seems to be peculiar to the American Negro. The survey group had not practiced contraception with perseverance or success. The "high" incidence of surgical sterilization is probably linked to lack of success with conventional contraception.

The next paper, "Factors in the Size of the Population 65 Years and Older," was by Richard Hornseth of the Bureau of the Census. It contained an analysis of factors accounting for the consistently high percentage rate of increase of the aged each decade since 1900. (See chart on front cover.) Since net immigration at age 65 or older is negligible, the increase is attributable to the difference between the number becoming 65 years old during the decade and the number dying at ages 65 years and over. Major elements in the sharp increase in the aged population during the past fifty years have been, first, the rapid increase in births in the last half of the nineteenth century; second, the large immigration in the first quarter of this century; and last, the reduction in mortality. The latter two factors apparently have attained their maximum effect, and the major source of increase of the aged during the last half of the century will be births in the first half of this century. Thus the per cent

increase in the aged population will probably be less in the last half than in the first half of the century.

A paper by Calvin L. Beale, Bureau of the Census, dealt with "The Enumeration of Mixed Racial Groups in the United States Census of 1950." There are a number of communities of mixed-blood people. They are, in fact or by tradition, of tri-racial ancestry: white, Indian, and Negro. Their race is variously defined by their neighbors, by census enumerators, and by themselves. They have been difficult to classify by race in the census. Their race status is not stable from census to census. Not only individuals but groups numbering thousands in the aggregate have passed from a colored status into the white population. In 1950 in 103 counties of concentration, 67,598 mixed bloods were identified. Special procedures for classifying the mixed bloods in the 1950 census were not entirely successful. By its nature, this is not a problem for which any simple enumerators' rule will suffice.

Thomas C. Wilkinson, of Columbia University, in his paper on "The Pattern of Korean Urban Growth" indicated that Korean urbanization was exceptionally rapid between 1936 and 1949. Korean cities developed primarily as administrative and distributive centers for Japanese exploitation of Korean natural resources and for distribution of produce from rural areas. The over-all focus of the economy remained agricultural, although there were limited industrial developments in the larger cities. City residence as an escape from poor rural areas has aided city growth. The cities of South Korea have approached a saturation point from growth based upon such factors. Since World War II the impetus from Japanese economic development has been removed. The appearance of factors necessary for continued urbanization—an agricultural produce surplus and the rise of specialized industrial activity—is highly unlikely in South Korea following an armistice. A decline in urbanization is likely.

A brief period of discussion at the last session centered on the explanation of the higher reproduction rates for whites than for Negroes cited by Smith. Several questions dealing with the possibility that the differences were artifacts of the statistical methods employed were answered in the negative by Smith. He pointed out that the differences are not due to mixed bloods, who are separately tabulated. Sharp differences in fertility by social class do not exist.

Dinner Meeting

President Clyde V. Kiser, Presiding

After several foreign guests had been introduced, President Kiser read a paper, "The Population Association of America Comes of Age," taking his title from the fact that the Association had passed its twenty-first birthday during the preceding year. Two interrelated but rather distinct situations were important in the formation of the Population Association. The first of these was the general upsurge of interest in population during the 'twenties, several reasons for which were outlined. The second was the stimulus given by the organization in 1928 of the International Union for the Scientific Investigation of Population Problems. The annual meetings of the Association began in 1932, and beginning about 1935 the Association expanded rapidly both in number of members and in the extent of its activities. These activities have included sponsor-

ship of meetings for the discussion of population problems, encouragement of participation of American delegates in international meetings, publication of Population Index, and indirectly, the stimulation of work in both government departments and private research agencies. Membership in the Association has increased from 38 to approximately 500. One of the problems for future officers will be to promote the further healthy growth of the Association while retaining the benefits of small discussion groups.

Warren S. Thompson, who retired this year as Director of the Scripps Foundation for Research in Population Problems, Miami University, recounted some of his experiences in "Forty Years of Study in the Field of Population." After considerable study of classics, psychology, and philosophy, Thompson became interested in sociology and in studying facts rather than absolute idealism. This led to work at Chicago and at Columbia, where he heard the word "demography" for the first time in 1912 and where he prepared a Ph. D. dissertation on "Population: A Study in Malthusianism." Thompson noted that it was possible to infer from the fact that there were but thirty-eight founding members of the Population Association in 1932 how many active workers there were in the field when he began in 1912, or even in 1920, when the professional staff of the Bureau of the Census consisted of two people. The dissertation, together with some articles in the international field, aroused the interest of E. W. Scripps, who, in the fall of 1922, established the Scripps Foundation at Miami University with Thompson as its Director. There have followed some thirty years of research that have been made especially satisfying by the fact that it was possible to pursue whatever line of study seemed of interest.

Following Thompson's remarks, President Kiser presented to him an illuminated scroll signed by members and friends of the Association present at the meeting.

Ronald Freedman
George F. Mair

REPORT OF THE COMMITTEE ON THE POPULATION INDEX

A special Committee on Population Index was appointed by the President of the Population Association in accordance with action taken at the annual business meeting of the Association in Chapel Hill in 1951. This Committee was reappointed in the following year. The Committee reports as follows on the results of the questionnaire distributed to members of the Population Association.

Of the 438 questionnaires sent out by the Secretary in the latter part of October, 1952, 150 were completed and returned to the Committee. The Committee recognized that, in view of the low proportion of returns—only about one-third—it had information on only a minority and probably a selected minority of the membership of the Association, but nevertheless felt obliged to content itself with what information it had. It is possible that the returned questionnaires are in fact more indicative of opinion within the Association than the percentage of returns suggests, for they may on the whole represent those members with more active interest, but it is not known who answered and who did not.