



ANSLEY J.  
COALE

AN AUTOBIOGRAPHY

panel on Egypt was delayed a bit because data from a crucial source (the census of 1976) were not yet available, and the responsible statistical body in Egypt, the Central Agency for Public Mobilization and Statistics or CAPMAS, had a reputation for reluctance to release data, especially to non-Egyptians. Its director, General Gamal Askar, was allegedly a prickly person who might not be cooperative with the committee. I had the idea, in forming a panel on Egypt, to include as usual several Egyptian authorities, and to designate General Askar and myself as cochairmen of the panel. I wrote to Askar explaining how the committee worked, the importance we gave to Egypt, the necessity to have several Egyptians on the panel, and the value of having both the chairman of the NAS Committee and the head of CAPMAS involved in guiding the work. Askar agreed, and assumed responsibility for assembling and initially processing the data, while I assumed primary responsibility for supervising and carrying out the analysis. There were three meetings of the panel in Cairo. Jane Durch (of the committee staff) and I did much of the analysis and the composition of the report. A gratifying feature of the cooperation of CAPMAS was that all of the crucial tabulations of the 1976 census were made available to the panel, some before they were published in Egypt. We also enjoyed the full cooperation of the CAPMAS staff, in addition to the invaluable help of the six Egyptian members of the panel. The report includes estimates of corrected age and sex distributions every five years from 1927 until 1977 (and at the census dates in 1960 and 1976) plus estimates of annual birth rates, death rates, total fertility, infant mortality, and life expectancy.

The report on China, which I wrote, has an only an indirect connection with the regular work of the NAS committee. I had first become involved with the population of China in the mid 1970's when Irene Taeuber, shortly before her death in 1974, became intrigued with data Notestein had retained in unpublished form: tabulations from a large demographic survey of rural China conducted in 1929-1931 by the Nanking Agricultural College under the direction of Lossing Buck. The demographic information was collected in connection with a survey of land usage in China. When the three volumes on land usage were published in 1937, Notestein and a Chinese colleague contributed some 40 pages on population, and

Frank also published a paper on the survey data in 1938. The fertility and mortality rates found in the survey seemed low to many; in fact Notestein rejected the data from communities in which birth or death rates were below the 25th percentile, because some were so low as to be clearly incredible, a flaw not surprising given the circumstances of the survey. Thus most scholars were unwilling to accept the fertility and mortality rates derived from the survey as representative of rural China. Irene Taeuber, who had become engrossed with the population of China in the last years of her life, knew of the tabulations which Notestein had kept from the 1929-1931 survey, tabulations that would be suitable for analysis by procedures developed since Frank had worked on the Buck survey material. The procedures were those that had been used in studying the demography of Tropical Africa, and those later employed by the panels of the NAS committee. By combining different sorts of imperfect data, valid age schedules of fertility and mortality could be constructed for the Chinese rural population around 1930.

Several years after this historical analysis (in 1976), I became involved with Chinese population again. I was asked in 1980 to help strengthen technical demography in China by giving a series of lectures at People's University in Beijing, to potential university and government demographers. While on this mission (described later) I had an audience with Chen Mu Hua, a vice premier who had nominal responsibility for population policy in China. I said to her that demography in China could not progress very far when fundamental data such as the results of the censuses taken in 1953 and 1964 had not been published, meaning that Chinese demographers could hardly do serious empirical work. Another suggestion I made to Chen Mu Hua was that some of the very able young men finishing their studies at Chinese universities, those who had skills in foreign languages, and some training in a social science plus mathematics and statistics, should be sent abroad for graduate training in demography. As I was leaving the audience room, I caught up to Sue who said "How about the able young women finishing their studies?" I turned back to the Vice Premier and repeated through the translator what Sue had said. She smiled and made a thumbs-up gesture.

In 1982-83, while spending a year on leave from Princeton at the Center for Advanced Studies in the Behavioral Studies at Palo Alto, I became involved in assisting Professor Arthur Wolf of Stanford University to analyze the results of interviews he had conducted in 1980-1981 with elderly women in seven communities in China. This involvement in Chinese demography, plus the availability of the results of the three Chinese censuses and the large sample survey on fertility, led me to start an analysis of population trends in China since 1952. When I returned to Princeton I completed this work, finding excellent consistency between data from the censuses and the survey. I calculated schedules of fertility, mortality, and first marriages over a thirty year time span. Methods that had been used in the committee work on other countries were employed (and sometimes extended). The results were written up in a manuscript of about 90 pages. I wondered where to find a publisher. I called Bob Lapham who was approaching completion of his duties as study director of the NAS committee, and asked his advice about publication. He said "Why not issue it as a report of the Committee?" The committee had strongly desired a panel on China, but felt that the data were insufficient; instead of trying to form a panel at such a late date, it could just name the chairman as in effect a one man panel on China.

After the report on China was issued, the Academy asked me to come to Washington for a press conference. It turned out that most of the reporters were interested in a small point: the 1982 fertility survey had noted that the ratio of male births to female births reported for 1981 was higher at higher order than at lower order births. I stated that this rise in sex ratio with order of birth could not be true. It indicated that some of the higher order female births had been omitted from the birth histories. I mentioned that because the one-child policy had been introduced recently, higher order births had become taboo; respondents might fail to report a recent third birth, and omit such a birth more often if it were a female, because of the traditional pride in bearing a son. I also mentioned rumors of the reappearance of female infanticide, and said that it might be a factor. It was the possibility of infanticide that the press wanted to hear about. I repeated that the higher sex ratio at higher orders was evidence

that some higher order female births had been omitted, but whether omission occurred because the birth was opposed by the authorities or because of female infanticide could not be judged from available information. I was then asked if infanticide were the reason, how many girl babies would need to have died. I repeated that I did not believe it to be the reason, then reluctantly did a rough calculation in my head and came up with a large number, adding that it was highly implausible. The Washington Post and other papers represented at the press conference carried a low-key story on the China Report, but a tabloid quoted me as saying that many thousands of female babies had been killed.

A little later, The Economist, in an article on the population of China, quoted me as saying that 250,000 female babies had been killed after the one-child policy had been introduced (evidently having picked up the item from the account in the tabloid). I wrote a letter explaining that I had not made such a statement and remarking that I found their misquotation an instance of irresponsible journalism. I did not receive a response in the mail, and expressed my frustration to my older son's wife who is a gifted administrator. She suggested that I call the New York office of The Economist, and to get the attention of a person of authority, say that I wanted to speak with someone authorized to discuss litigation.

I followed her advice, and immediately was put through to a senior person who asked if I had not seen my letter. I explained that I did not subscribe to the journal. He said that the letter had been published, and added that because of a shortage of space letters were edited, not for content but for length. I found a copy of the issue that contained my letter, and found it complete except for the statement that I found The Economist guilty of irresponsible journalism!

Three years later an article on world population in The Economist again included a statement attributed to me that 250,000 girl babies had been killed in China. My letter this time was quite irate. I said that their journalism was inept as well as irresponsible since they themselves had published my earlier disavowal of this statement. Later at the demographic conference at Ditchley Park near Oxford mentioned earlier the chairman announced that a staff writer from The

Economist was keeping notes and would prepare a summary of our discussion. At a coffee break I asked the journalist if he knew about the difficulty I had had with his employer. "I'll say", he replied, "I wrote the piece." He then told me the reassuring news that The Economist had revised its procedures, and now routinely checked its correspondence files while preparing its text for publication.

#### INVOLVEMENT IN THE WORLD FERTILITY SURVEY

The World Fertility Survey, planned beginning in 1972, partially in connection with the anticipated World Population Year of 1974, was led by the International Statistical Institute (ISI), in collaboration with the IUSSP, and the Statistical Office and the Population Division of the United Nations. It began operation in 1974 with headquarters established in London under an outstanding statistician Maurice (later Sir Maurice) Kendall. It was funded primarily by UNFPA and USAID. Its primary activity was the organization of sample surveys of fertility in ldc's: the design of questionnaires and samples for each country, selection and training of interviewers, oversight of the conduct of the survey, and participation in its analysis. The surveys were conducted in 42 less developed countries; they provided the richest and best quality comparative data available by the early 1980s on the demography of the third world.

I became involved in the design and analysis of the World Fertility surveys, serving on several committees and making 11 trips to London to visit the headquarters on Grosvenor Gardens. These trips gave Sue and me a very pleasant exposure to London. We located a nice small comfortable hotel in Knightsbridge; it was a colorful walk for me to the WFS office; we ate well in small restaurants we found; in the morning or evening I could take a stroll around the lake in Hyde Park; we sampled the London theater; and we (especially Sue) enjoyed the marvelous London museums.

The most colorful experience we had with the WFS was a trip to Nepal (with a visit to Shri Lanka thrown in). The Survey design for the Nepal Survey began with a staff visit in 1975; the survey itself was conducted in 1976; the Country Report was published in 1977; a national meeting was scheduled for the