Theory and Method in a Study of Argentine Fertility

AARON V. CICOUREL

A WILEY-INTERSCIENCE PUBLICATION

JOHN WILEY & SONS, New York • London • Sydney • Toronto

Library of Congress Cataloging in Publication Data:

Cicourel, Aaron Victor, 1928-
Theory and Method in a study of Argentine fertility.
"A Wiley-Interscience publication."
Bibliography: p.

1. Fertility. 2. Argentine Republic—Population.
3. Social surveys. I. Title.

HB903.F4C44 301.32'1'0982 73-14985
ISBN 0-471-15793-7

Printed in the United States of America

10 9 8 7 6 5 4 3 2 1

Preface

My research in Argentina was motivated by several interests. I wanted to pursue methodological and theoretical issues described in previous writings in a comparative context. I felt that these methodological and theoretical issues should be pursued in a different area—for example, fertility—to show the breadth of earlier ideas applied to education and deviance. I wanted to demonstrate how notions of everyday social interaction are presupposed in the study of such macro issues as differences in fertility rates. Finally, I wanted to initiate a long-standing (but still unrealized) interest in the study of Latin American social structure. I had no desire to become an "area" specialist, but I did want to immerse myself in a setting that would provide a useful comparative basis for pursuing methodological and theoretical issues explored heretofore only in an American context.

This book does not follow the usual sociological approaches to the study of fertility or Latin America. I try to do more than replicate important research on fertility in Jamaica (Blake 1961) and Puerto Rico (Stycos 1955) using modified survey methods. I try to address these issues by contrasting traditional theoretical and methodological approaches with the program described in my earier work, *Method and Measurement in Sociology*. Hence I create traditional tables primarily to allow the reader to compare their interpretation with a textual analysis of the original interview materials. The tables, therefore, are relegated to a rather minor position in the study.

I begin with some general problems in demography and the substantive study of fertility that cross-cut the social organization of different societies. I discuss attempts to narrow the gap betwen these macro studies and the concern with social psychological aspects of fertility. A recent book by Beshers (1967) provides a social psychological perspective that enables me to outline my own contrasting views. Background information on Argentina is then supplied to enable the reader to locate the study ethnographically and historically. The remaining chapters describe the

sample used for the study, presenting some findings, as well as a detailed discussion of how a textual analysis of the interview materials forces us to reexamine traditional research strategies and theoretical assumptions usually associated with surveys. An idea of the field conditions encountered is conveyed by a detailed picture of a few families. I have tried to stress the importance of understanding the social context within which the interviews were conducted and the larger problems in human information processing that these interviews create. These larger problems take us beyond the study of fertility and deal with important issues in all field research. The final chapter contains a brief outline of some new directions that are proposed for field research.

This is the first foreign study I have attempted. Fortunately, however, I had several friends, relatives, and acquaintances in Buenos Aires; this meant that I could obtain a native's view about my initial tourist-oriented observations and feel assured that the information I recorded would be checked for accuracy. Having these key informants in the local area, I could compare my more naïve initial impressions against those of natives, thereby avoiding mistakes a novice to foreign field work can make. But the contrasts also served to warn me how informants can filter out impressions that underline differences between groups within the country, and differences between Argentina and the United States.

My contacts enabled me to adjust more easily (but never completely) to changing views of my perceptions of a respondent's sincerity, as well as the interpretation of variations in local punctuality, "friendly" gestures, and the like. The verbal and nonverbal cues one is accustomed to in his native land and community are not the same in a foreign country, and they cannot be assumed to be the same if we are to recognize and describe comparative research problems.

Some of my difficulties began when I sought to recruit assistants. It took several weeks to discover that obtaining good interviewers meant crossing political and friendship lines and that it was important to guide training sessions carefully to prevent the opposing parties from using the time for their own arguments or disputes. On rare occasions, I felt pressured to take on and keep poor assistants (while assigning them less and less work) to maintain a viable research team.

Problems of comparative research became painfully obvious when I began, with the help of my friend Miguel Murmis, to translate the questionnaire. What I thought to be simple problems (how to say "flirting" without offending women respondents, yet maintaining colloquial usage when discussing with men their premarital contacts) turned out to be complicated. I was advised by assistants that it would be almost impossi-

ble to ask certain questions about sexual activities and the use of contraceptives, but I decided to push ahead nonetheless with a few pretests, and finally I convinced the group that the questions could be asked. My assistants felt that the questions were too personal for an Argentine sample and that a survey was not the best way to elicit information about sexual activities and the use of contraceptives.

My project was modestly financed and could not have been done if the Social Science Research Council (Joint Committee on Latin American Studies) had not given me its support. I received additional support from the University of California through half sabbatical pay and from the Institute of International Studies at Berkeley. The Institute of Sociology at the University of Buenos Aires provided funds for some secretarial help and helped pay part of my salary. The Population Council provided a small grant which enabled me to hire Ricardo Muratorio Posse at the International Population and Urban Research project at Berkeley. Mr. Muratorio did virtually all the coding operations, provided excellent suggestions about the quality of the materials, and constructed approximately two-thirds of the tables.

I am grateful to Judith Blake Davis for advice and the generous use of the questionnaire from her Jamaica study. I also received helpful advice from Kingsley Davis, Gino Germani, and William Petersen. In Buenos Aires, Miguel Murmis and Darío Canton were generous with their time and helpful advice. Nancy Lopez de Nisnovich proved to be a dedicated and outstanding administrative assistant, research assistant, and interviewer. She worked hard and was responsible for organizing the data. A number of persons helped me with the interviews and research assistance. I am grateful to Mable Arruñada, Elizabeth Balán, Anna Maria Caellas, Miriam Chorne, Santos Colabella, Estella Elbert, Marta Fernandez, Leticia Gurman, Adolfina Janson, Cristina Mendelarzu, Maria Luisa Salvarezza, Marta Slemanson, Juan Carlos Torre, Ponciano Torales, and Valentin. Margarita Sandoval proved to be an excellent typist when the questionnaire responses were placed on 5×8 cards. Mrs. Charlotte Williams typed most of the initial draft of the first five chapters of the manuscript from a messy handwritten copy. Pat Apodaca typed the remaining chapters of the first draft quickly and accurately. The last two drafts were typed with enthusiasm by Sue Miller, Beverly Strong, Jessica Diaz, and Becky Miller. I am grateful to Eric Valentine for insisting that I complete a careful reediting of the "final" draft of the manuscript, and to Martha Ramos for her very helpful editorial suggestions.

I also acknowledge the valuable assistance of the copy editor Brenda Griffing. I am responsible for the defects of the final version of the book.

Three interviewers were especially busy doing the majority of the interviews. The three—Estella Elbert, Leticia Gurman, and Maria Luisa Salvarezza—were very generous with their time and were exceptionally dedicated to my concern with field research problems. Santos Colabella did some intensive interviewing in a difficult suburban area where many had refused to participate in the study. In his attempt to discover the background of the resistance, he found a local "witch" and wrote me a very helpful report on the methodological problems he encountered.

I obtained a number of important interviews at the Children's Municipal Hospital because of the generosity of Dr. Florencio Escardó. Dr. Escardó made ward 17 available to me for intensive interviews with mothers coming from the shantytowns of Buenos Aires.

I found the study difficult, but it surely would have been impossible had it not been for those just mentioned and many, many others who remain anonymous to protect their identity. Finally, I want to acknowledge the careful research assistance of my wife Merryl, and her devoted patience and understanding throughout a difficult year.

AARON V. CICOUREL

University of California, San Diego
La Jolla, California
July 1973

Theory and Method in a Study of Argentine Fertility

Contents

CHAPTER 1

Theoretical and Methodological Issues in Studies of Population and Fertility

The study of fertility is often linked to the population problem—the relation between numbers of people and their social and economic resources. This study is restricted to the procreative activities of women, primarily those who are or were married. I am less interested in the population problem than in what demographers and population analysts have to say about the causes of differences in fertility, and how such causes are attributed to social and economic conditions.

Demographers and population analysts often begin their discussions of fertility behavior by referring to what are believed to be facts, noting, for example, that underdeveloped areas usually have a much higher fertility rate than urbanized–industrial societies. They pay particular attention to establishing the point at which birth rates started to rise or fall and how rapidly the rise or fall occurred. The variables affecting the possibility of intercourse during the reproductive period, leading to the possibility of sexual behavior and pregnancy or the use of contraceptive techniques, are usually discussed as obvious influences on fertility rates. Factors mentioned are age of entrance into a marital or informal sexual union and the point at which the union was broken by divorce, desertion, separation, or death.

In the preceding paragraphs I used the terms "facts" and "variables" without placing quotation marks around them. Throughout this book, however, I often use such terms with quotation marks because it is seldom clear in studying human fertility how investigators incorporate their own and their respondents' decisions and inferences made during day-to-day activities, into the aggregated "facts" and designated "vari-

ables" usually cited as causally relevant. The present study seeks to narrow the methodological and substantive gap between the everyday activities of reproduction, and the demographic and population studies describing abstract relationships that cross-cut the social organization of different societies. The Indianapolis studies (Whelpton and Kiser 1958) helped narrow the gap by linking attitudes obtained from social surveys to abstract structural characteristics such as the age, sex, and occupational distributions of societies, and social mobility patterns, kinship, and gross measures of economic growth. But surveys designed to elicit information about process through attitudes, or about structure by asking factual questions, do not recognize adequately the importance of the interview setting. The interview settings often reify past experiences while simultaneously creating forms of social interaction that can exert independent influence on the manner of asking or paraphrasing questions and on the truncation or transformation of answers during the interview.

Recent studies of fertility have dealt with the micro level of analysis more carefully; the reliance on attitudinal surveys tends to prevail, however, despite important attempts to provide careful interviewing procedures and a textual analysis of responses (Stycos 1955; Stycos and Back 1964; Blake 1961; Rainwater 1960, 1965).

I became interested in fertility and family organization because I wanted to demonstrate the importance of linguistic and cognitive factors in the study of everyday social interaction. This chapter is restricted to a few traditional notions about fertility in industrial societies; it closes with a brief outline of the orientation I propose as an alternative to traditional approaches.

Recent Sociological Thought on Fertility. Discussions of fertility by Davis (1959), Petersen (1961), and Wrong (1967) have stated the issues well. In this section I want to summarize some of the relevant literature, subsequently focusing on an important paper by Davis (1963) to set the stage for my own observations on fertility and family organization.

There has been a great deal of speculation about the reasons for the decline in fertility rates in the Western nations during World War I and until the middle 1930s, and their increase in the late 1930s or early 1940s and into the 1960s. Out of this curiosity have come a number of studies of psychological motivations and social determinants affecting reproductive behavior. The decline and rise of fertility has been linked

to trends in urbanization, changes in the mobility of social classes, and changes in family structure and function, attitudes, and motives.

Because of variations in reproduction within a given family over the woman's childbearing years, it became necessary to examine a number of factors—for example, the age at which persons marry, the decision to postpone having children, and how couples might change their attitudes and motives before and after having had one or more children. Researchers also examined differential fertility across different types of grouping (e.g., by designations of religion, rural or urban residence, race, occupation, income, or education, or by composite measures leading to statements about social class differences). Scholars have considered the reproductive intentions of couples before and after the birth of each child. The study of couples' intentions has led to the view that each birth must be examined in its particular social setting and the local circumstances deemed to be associated with reproduction (Westoff, Potter, Sagi, and Mishler 1961; Westoff, Potter, and Sagi 1963).

Many scholars have stressed the importance of demographic factors in assessing differential fertility rates, in contrast to the decisions leading to marriage and one or more children. The factors often cited include separating changes in the sex ratio (i.e., an increase in the number of women), the age at marriage and the number of women marrying, an apparent growth in the number of married women bearing a first child, and the number of births per mother (Petersen 1961: 239). Yet many researchers would agree with Wrong's statement that the decline in the fertility rates of Western countries is due to voluntary causes and is ". . . the result of deliberate restrictions on procreation consciously practiced by modern populations" (1967: 53–54). Hence the decline of fertility in Western populations is not simply a function of chemical and mechanical methods of contraception, and abolishing such methods would not lead to a rapid increase in the birth rate (Wyon 1963: 88; Wrong 1967: 57). The social scientist is interested in differential fertility because of the relation between types of families and the social structures within which they are created. This interrelationship has been described as the ". . . social usages governing (*a*) the conditions of marriage and (*b*) family behavior patterns that can affect the relative frequency of conception and childbearing" (Petersen 1961: 563).

Recent work on fertility has focused on nonindustrialized countries because of their declining mortality and high reproduction rates. Many of these studies have been concerned with the impact of fertility on

economic development and with the possibility of changing reproductive patterns.

In discussing fertility studies in nonindustrialized areas, Davis (1959: 322–323) underscores a number of problems. For example, how do we arrive at the social conditions behind fertility? How do we deal with the ambiguity of the verbal statements elicited, and what do they mean in terms of our understanding of past and future behavior and reproductive decision-making in the context of different birth order? Additional factors cited by Davis include the discrepancy between normative and actual behavior, inappropriate categories for elicting information because of culture-bound problems, and the tendency to restrict such studies to limited contacts with the respondents. We could add to the foregoing list, but the difficulties noted will indicate some of the issues involved in fertility studies.

Wrong (1967: 62) has questioned whether we can compare a decline in mortality and fertility in the West to possibly similar declines in non-Western or Western countries with low industrialization, despite the case of Japan. Wrong's observation serves to reemphasize the importance of a culture area wherein a special "institutional arrangement" exists that is conducive to high fertility.

Many of the issues in the study of differential fertility can be clarified if we now examine more general topics in modern demograhic history.

Kingsley Davis is a sociologist who has spent much of his career emphasizing both micro- and macro-level aspects of social structures. I do not agree with the specific approaches he has used, nor with the way he has kept the micro and macro levels of analysis separate. However, Davis has done work which in several ways forms an exception to criticisms of fertility studies I describe in the next section.

In the paper "The Theory of Change and Response in Modern Demographic History" (1963), Davis provides a sophisticated sociological statement for explaining the data assembled by demographers. He confronts the analysis of macro data (aggregated statistical information) and offers a theoretical explanation of how social and behavioral activities could have produced the structural effects identified. He proposes what appear to be invariant stimulus conditions and response consequences to account for modern demographic history. Some of the problems associated with the process of demagraphic change and response are described in the following passage, (in which Davis is critical of the tendency of demographers to treat attitudinal or interactional factors as tacit rather than as central features of demographic outcomes (1963: 345):

> One method of escape [from the "frightening complexity" of the process of demographic change and response] is to eschew any comprehensive theory, simply describing computations or working on a single hypothesis at a time. Another is to adopt some convenient over-simplification, such as the assumption that population is simply a matter of two capacities—a "reproductive urge" on the one side and "means of subsistence" on the other—or, at an opposite extreme, that demographic behavior is a function of a "traditional culture" or "value system."

A common practice of demographers is to use such terms as "society," "fertility," "abortion," "Japanese tolerance," and "West European beliefs," to reify or to anthropomorphize abstract collectivities or phenomena. Such truncated descriptive terms simplify and distort the complexities of everyday behavior. Calling the use of contraceptives or abortion a "response" to social and economic conditions leaves unexamined the paths by which the members of a group have come to perceive such conditions as "negative" in their everyday communication. Another practice is to avoid the study of the daily communicational activities of a group, yet to assume that such activities index or reflect demographic outcomes.

Davis notes that it is too simple to say that fertility eventually falls in response to a drop in mortality. (I am ignoring recent claims that fertility rates may have started to decline before a decline in mortality, for Davis's remarks are more general than these substantive claims.) The complexity of the claim is avoided when demographers ignore the kinds of information presupposed about how a decline would be noted by different members of a group and how respondents' social distribution of knowledge would be a central ingredient in making claims about the "perception" of a decline. The demographer fails to explain how the sustained natural increase resulting from a decline in mortality now would be seen as a "negative" sign or "stimulus," leading to collective responses that then lowered fertility.

The unexamined systems of communication and social settings embedded in different cultural arrangements—which presumably lead to responses such as postponed marriage, the use of contraceptives, sterilization, abortion, and migration—are inferred in an interactional and cultural vacuum. Even when normative practices or accounts about cultural areas are used, or when the researcher simply makes use of his nativeness in asserting claims about demographic patterns in his own country, the sources of the information are not questioned, nor are they seen as ad hoc descriptions serving to sidestep the difficult issues rooted in the study of social interaction.

Davis's statement that "every country in northwest Europe reacted" to the excess of births over deaths makes reference to activities such as postponing marriage, emigrating overseas, obtaining abortions, practicing celibacy, and (to some extent) using contraceptives. The assumption made is that a "message" (that a decline in mortality was leading to sustained natural increase) was evident or perceived in different countries among various groups and individuals. The perception of the stimulus (excess births over deaths or sustained natural increase) was apparently "clear," but the "response" varied.

Davis's claim that a simplistic notion of poverty is inadequate to explain the "response" of lower fertility, despite a decline in mortality, is persuasive. He notes (Davis 1963: 351) that northwest Europeans and the Japanese were not "pushed" by growing poverty:

> The answer to the central question about modern demographic history cannot be posed, then, in the framework of ordinary population theory, which assumes the sole "population factor" to be some relation between the population–resources ratio and the collective level of living. It is doubtful that any question about demographic behavior can be satisfactorily posed in such terms, because human beings are not motivated by the population–resources ratio even when they know about it (which is seldom).

The appeal of this analysis lies in the author's continual references to realistic possibilities in the everyday life of the family and his suggestion that these contingencies could have produced the gross outcomes that demographers tend to oversimplify by avoiding explicit theorizing at the level of everyday family life. Let me outline some of the points made by Davis.

People in northwest Europe during the late nineteenth and early twentieth centuries, as well as the Japanese after World War II, somehow became aware that their customary demographic behavior was handicapping their efforts to take advantage of a changing economy. The link between a "changing" economy that offered new opportunities and a desire on the part of members to benefit from the changes is presumed by demographers to result in modifications in demographic behavior; such substantive claims are difficult to study historically at the level of day-to-day living, however, and they remain difficult to study in contemporary research. Theories about everyday life and the general structure of social interaction are presupposed.

Davis's remarks lead us to reason that individual family members experienced the consequences of a decline in mortality by way of having to live with several siblings with whom they were obliged to share

whatever they could obtain from their parents. Several additional implications suggest that the individual family member might have become preoccupied with having to spend more time with his parents because of an economic obligation to help support the family. As an adult, because he had been raised among many siblings, he would have fewer resources with which to start his own family and to provide for educating his own children and endowing their marriages.

Davis addresses the assumption of similar orientations within families in northwest Europe and Japan during the historic periods under review, but he does not deal with the measurement of the orientations and the implied communication involved at the level of social interaction.

Substantive problems having to do with class differences in towns, cities, and agricultural areas (particularly with respect to presumed inheritance troubles described by demographers) are neatly questioned by Davis; who reminds us that individuals and family groups react to changing perceptions and definitions of their socially organized settings rather than responding to "traditional values."

In the case of a rural population's demographic response to declining mortality, Davis tries to pinpoint the critical decisions affecting changes in fertility behavior. Declining mortality leads to land scarcity and changes in traditional patterns of land distribution for couples wanting to get married. Land scarcity problems lead to postponement of marriage and to the migration of rural youth to find nonagricultural jobs created by a changing economy. Davis reasons that the decisions to postpone marriage and leave agriculture were probably made jointly. The actor's commonsense interpretations of his everyday life circumstances remains a tacit but central feature of Davis's description of possible substantive decisions within the family.

Davis implies that members decided to postpone marriage and to leave the farm setting because they saw that this declining mortality (and hence, under the present argument, overpopulation) were contributing to a scarcity of land. The "responses" of postponing marriage and migrating to nonagricultural occupational possibilities are linked to changes in the larger economy, whereby industrialization requires emigration from agricultural areas. The rural youth who moved to the city, which was the locus of industrialization, were "rewarded" for such migration. Farms were supplied with needed capital, farm youth were able to marry, and so on. These activities can be viewed as having provided an "alerted" or self-conscious population with the opportunities to participate in the economic revolution. For these youth, Davis notes, there was no "resist-

ance" to modernization in terms of adhering to a "traditional value system." Thus the rural population of industrialized countries "responded" to declining mortality by out-migration and a postponement of marriage, not simply to reduce fertility or to lessen the population problem, "but as a response to the complexity and insecurity of the new requirements for respectable adult status under changing circumstances" (Davis 1963: 355–356). Thus Davis seems to rely on an implicit model of the actor who makes everyday interpretations to perceived environmental changes. Knowledge of the structure of everyday interaction, therefore, is essential for a viable demographic theory.

Davis emphasizes that conditions external to daily family life, such as knowledge and technology, can influence declining mortality, industrialization, and economic development. His work suggests that the locus of change resides in the day-to-day living patterns and decisions within the nuclear family. Families presumably were faced with recognizing the social significance of declining mortality and the opportunities of a developing society. To maintain their social status and consumption (or perhaps to improve either or both), they had to alter their demographic behavior. The plausible substantive conclusions advanced by Davis, therefore, presuppose an implicit model of the actor and a theory of social organization that makes use of, but does address, the researcher's resources as a member of some group or society. The researcher makes implicit use of his ability as a member to imagine or construct typical courses of action for fellow members, to attribute typical motives, and to assume the existence of practical reasoning to satisfy the observer's thinking about practical action based on his conception of the settings.

Problems of Demographic Analysis. In this section I discuss some of the issues that are slighted or ignored by the demographer.

The demographer does not question the nature of the demographic stimulus. Is the stimulus intended to alert the reader to a feasible way of "talking about" conditions leading to changes in population size, density, reproduction, and so on? References made by the demographer to "stimulus" conditions occur after the fact; hence the researcher can always claim "responses" while obscuring the nature of the stimuli.

Davis's paper forces us to ask whether persons in everday settings are actually responding to the kinds of stimuli that demographers mention as relevant. For example (to simplify the issue), suppose that people do respond, by migrating and using contraceptives, to a decline in infant