PREVALENCE AND EFFECTIVENESS OF FAMILY PLANNING

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Forty years devoted to research is a long time in the life of a scholar such as Clyde Kiser. Forty years is also a long time in the life of a young discipline such as demography. In fact, it is practically the life span of the discipline; almost everything that is known about the prevalence and effectiveness of family planning in the United States has been learned over the past four decades. Today, in 1971, we may say with a sense of accomplishment that a great deal is known about the reproductive behavior of the American people, although important lacunae remain to be filled, one hopes, by a new generation of scholars. Limited time will permit no more than to sketch in broad outline some of the major efforts, achievements and failures of the pioneer generation of American demographers. Many of these pioneers, in addition to Clyde Kiser, are present in this room; some are no longer with us.

RAYMOND PEARL

At the threshold of this forty-year epoch stands Raymond Pearl, who tried to assess the "extent of contraceptive efforts in the American population" and their "effects upon natural fertility" on the basis of almost 31,000 reproductive histories obtained from women delivered in 139 metropolitan hospitals in the United States.[1] This project was started in 1931, with financial support from the Milbank Memorial Fund, and was completed in 1938.

Today, with the wisdom of hindsight, it is easy to recognize the shortcomings of Pearl's methodology, but in the 1930's he was breaking new ground and defying well-established taboos.

Pearl estimated the "proportion of married women in the general population practicing contraception" at no more than 55 to 60 per cent. He was well aware of the limitations of this estimate, which he described as "the best present judgment of one student of the problem."

Pearl's major contribution to contraceptive research was his famous formula relating the number of pregnancies occurring in a population to the period during which the women were exposed to the risk of conception; i.e., to the aggregate number of months of cohabitation, excluding those coinciding with pregnancies or the puerperal state.[2] He was interested in the combined result of the extent and intensity of the contraceptive effort, so limited himself to comparing the total reproductive performance of women who claimed they had never practiced birth control in any form at any time during their married lives with that of women who had done so during all or part of their marriages.

This application of Pearl's formula was substantially improved by others concerned with the evaluation of contraceptive services and methods. Stix and Notestein, two other pioneers in the field, separated the marital histories into two types of exposure: periods during which contraception was not practiced and periods when it was practiced.[3] Pregnancy rates could then be computed for both types of exposure and the effectiveness of contraception assessed in terms of the difference between the observed rates. Pregnancy rates could also be computed for periods of exposure with a particular contraceptive method.

In the course of time, Pearl's formula, with the improvements introduced by Stix and Notestein, was adopted by the medical profession and the pharmaceutical industry as the standard procedure for assessing the efficacy of contraceptive methods and products. The formula continues to be used for this purpose, although it has in recent years been severely criticized by demographers and statisticians.

THE INDIANAPOLIS STUDY

The first comprehensive attempt to ascertain the prevalence and success of efforts at family planning in a representative sample of an American population was the study of Social and Psychological Factors Affecting Fertility, known throughout the world as the Indianapolis Study.[4] Like Pearl's earlier work, this study was sponsored by the Milbank Memorial Fund. Clyde Kiser served on the steering committee and was one of the editors of a series of 33 reports published in the

Milbank Memorial Fund Quarterly, of which he wrote or co-authored 19, often leaving the honor of senior authorship to younger colleagues.[5–23] He also wrote critical essays about the entire project.[24–26]

The overall purpose of the Indianapolis Study was to appraise the social and psychological correlates of two associated phenomena: family planning and the size of planned families. In more specific terms, the objective was the testing of 23 hypotheses concerned with these relationships. The data required for this evaluation were obtained from a sample of 1,080 couples interviewed in Indianapolis in 1941 and early 1942. All husbands and wives were native white Protestants, married once, 12 to 15 years earlier, who had completed at least the eighth grade and had lived in a large city most of the time since marriage. The interviews were conducted in considerable depth, with more than 1,000 specific items of information elicited from each respondent.

While "the Indianapolis Study failed to yield much in the way of the relation of psychologic factors to fertility behavior," it proved a rich and thoroughly exploited body of information on the extent and success of contraceptive practice in one American city or, at least, among a large segment of the population of that city. Above all, the study demonstrated the feasibility of obtaining information of a private and sensitive nature using house-to-house interviewing procedures.

The analysis of the vast amount of data collected in the Indianapolis Study was substantially delayed by World War II and was not completed until 1954. By that time, two new investigations, quite different in objectives and methodology, were already in advanced stages of planning. One was the first nationwide retrospective study of reproductive behavior, and the other was the first major prospective study on the same subject.

NATIONWIDE RETROSPECTIVE SURVEYS

The retrospective study is known as the GAF Study, an acronym for "Growth of the American Family" or more precisely as GAF/1.[27] Its major aim was to secure information on women's expectations as to the ultimate size of their families, which would be used to make population projections more reliable. In the course of pursuing this objective, many related topics were explored, including the extent and success of contraceptive practice.

Data for GAF/1 were obtained in 1955 from a sample of 2,713 white married women, 18–39 years of age. In addition to being nationwide,

the sample included the foreign-born, Catholics and Jews, and women with less than eight years of formal education, all of whom had been excluded by the Indianapolis Study. Blacks and other racial minorities were the only groups not represented in the GAF/1 sample.

The conceptual framework of GAF/1 was so clear and concise—one is tempted to use the word "streamlined"—and its implementation so successful, that it established a ready pattern for a series of similar studies at regular intervals of five years. GAF/2 was the first of the successors, the interviewing being done in 1960.[28] The nationwide sample was extended to include married women up to age 45, previously married women and Black and other nonwhite women.

Five years later, in 1965, a new group of scholars continued the tradition established by GAF/1 and GAF/2, with financial support from the National Institute of Child Health and Human Development. To mark the change a new name was chosen: National Fertility Study (NFS). The sample design was similar to that used for GAF/2, but the sample was larger (4,810 married women under 45 years of age, compared with 3,256 in 1960) and nonwhites were double-sampled to provide adequate numbers for analysis. A number of important articles based on NFS/1 have been published,[29-37] but the comprehensive final report was still at the printers when the present paper was written. Meanwhile, the interviewing for NFS/2 was done in late 1970.

Although each of the three surveys, taken in 1955, 1960 and 1965, has made important additions to the scope and depth of analysis of reproductive behavior, all of the scholars involved have been very conscious of the requirement of comparability over time. Accordingly, American demographers are in the fortunate position of possessing a record of changes in family planning attitudes and behavior, on a national scale, during two successive quinquennia, the second of which included the contraceptive revolution associated with the introduction of oral contraceptives. As data from NFS/2 become available, five more years will be added to the historical series.

The *ad hoc* organization every five years of nationwide surveys of reproductive behavior, with repeated efforts to secure the necessary funding from whatever sources can be tapped, is not an efficient procedure. Most demographers would probably agree that the time has come to organize these efforts on a continuing basis.

In 1970 the Subcommittee on Population Dynamics of the U.S. National Committee on Vital and Health Statistics, under the chairmanship of Clyde Kiser, recommended a "Federally supported series of

national interview surveys" as "the main vehicle for collecting information about the national population's fecundity, reproductive norms, and family planning practices, and the relation of these factors to natality."[38] The National Center for Health Statistics is now actively working on plans to conduct a "National Survey of Family Growth" as a recurrent activity. Present plans call for substantially larger samples than those used in earlier, privately financed surveys, with oversampling of Blacks. The field staff of the Bureau of the Census would do the interviewing. Hopefully, the National Survey of Family Growth will be scheduled for the first time in 1972, and then repeated at intervals of two years, rather than five.

THE PROSPECTIVE APPROACH

Let us now return to the year 1954 when the final report of the Indianapolis Study was published. A group of demographers assessing the results had felt strongly that two major reasons for the difficulties experienced in the identification of psychological factors affecting fertility were (1) the retrospective design of the Indianapolis Study and (2) the long period of married life—12 to 15 years—covered by the interviews. Accordingly, a new investigation was initiated that was to be (1) prospective and (2) focused on the transition from parity II to parity III. This project, officially known as the Family Growth in Metropolitan America (FGMA) Study, was again sponsored by the Milbank Memorial Fund and, once more, Clyde Kiser served on the steering committee. The study was conducted at the Office of Population Research at Princeton University; it should be no surprise to anyone that the name "Princeton Study" has become more popular than the four-letter acronym FGMA.

Data were obtained from 1,165 white couples, residing in seven of the country's eight largest Standard Metropolitan Areas, as defined by the Bureau of the Census and including a generous periphery of suburbs, who were interviewed in 1957, a few months after the birth of their second child.[39] More than 900 of the original 1,165 couples in this study were reinterviewed about three years later,[40] and 814 were questioned for the third time toward the end of the wife's childbearing period.[41] The time between the first and last interview averaged eight years, indicating one of the longest prospective studies ever undertaken.

Because of its prospective approach, the Princeton Study has produced many important insights into reproductive behavior, especially

the effectiveness of contraceptive practice, supplementing the broader picture obtained by the retrospective national surveys (GAF and NFS). It is worth noting, however, that efforts to associate reproductive behavior with psychologic, as distinct from social, factors were as unsuccessful in the prospective context as they had been in the retrospective Indianapolis Study, very likely reflecting the difficulties of measuring the dimensions of personality.

LIFE TABLE METHODS

By about 1960, it had become recognized that the computation of pregnancy rates by Pearl's formula was in need of reevaluation.[42] Because it equated 200 women observed for six months or 50 women observed for 24 months with 100 women observed for 12 months, the formula was based on the implicit assumption that the monthly risk of conception was independent of the duration of exposure to risk. This assumption is incorrect. Generally speaking, the monthly risk declines with duration of exposure; very rapidly, if no efforts are made to prevent conception; more slowly, if contraception is practiced. The progressive diminution of the monthly risk of conception arises from the fact that couples differ in fecundity as well as in contraceptive skill and perseverance. The more highly fecund couples and/or the less skillful or poorly motivated contraceptors tend to experience pregnancy early, whereas the less fecund couples and/or the more skillful or better-motivated contraceptors represent an increasing proportion of those not yet pregnant. This process of selection is more rapid in the absence than in the presence of contraceptive efforts,[43, 44]

Recognition of these facts led to the adaptation of life table techniques to the evaluation of the effectiveness and continuation of contraception, an area of research in which Robert G. Potter and the writer have been most active.[45–48] The new approach was powerfully stimulated by the resuscitation of an old but virtually abandoned method of contraception: the intrauterine device (IUD).

Because of the obvious potentialities of the IUDs for national family planning programs in developing countries, the Population Council initiated in 1963 a Cooperative Statistical Program (CSP) for the evaluation of IUDs.[49] The CSP represents the first effort to have a new, or in this case a revived, method of contraception assessed by an independent organization rather than by investigators supported by competing pharmaceutical manufacturers.

However, the life table technique can be used and has been used for the study of contraceptive methods other than the IUD and of contraceptive practice in general.[50] It can be limited to studying the effectiveness of a method while it is actually in use, although not necessarily consistently practiced. It can also be extended to include pregnancies that occur after the first method used has been replaced by another method or contraception has been entirely abandoned.

INDUCED ABORTION

The major areas of ignorance in the field of reproductive behavior of the American people are the extent to which induced abortion is used as a means of fertility regulation and the demographic and social characteristics of the women having abortions. Not only were most abortions in the United States, until very recently, performed illegally, but the social taboos that rendered the subject of abortion almost "unspeakable" were so strong as to make successful quantitative assessment impossible. Induced abortion was not included in the questionnaires in some of the major studies of the past forty years. Other investigators did ask about abortions, but the number of respondents admitting to having had one or more abortions was quite small, much less than could be expected on the basis of medical experience or such statistically suspect data as those collected, for a somewhat earlier period, by the Institute for Sex Research.[51] The implications of this inadequate response for the assessment of contraceptive effectiveness are obvious.

In recent years, public and private attitudes vis-à-vis induced abortion appear to have entered a period of change, more rapid than comparable changes in regard to contraception some years ago. As restrictive laws have been replaced by more liberal legislation in some states and struck down as unconstitutional by the courts in others, the number of abortions performed openly in hospitals and clinics in the United States has increased from 500 per month in 1966 and about 1,500 in 1968 to a rough estimate of 25,000 per month during the second half of 1970.

The greater availability of legal abortion has not yet been reflected by a higher rate of survey response. In a national survey conducted on behalf of the Population Council in November 1970, which focused specifically on abortion, only 51 out of a total of 2,400 women of all marital statuses, 20–49 years of age, admitted to a total of 86 induced

abortions. Ten additional women refused to answer questions relating to their own abortion experience; in effect they "took the Fifth Amendment." Clearly, either the formulation of the questions or the selection and training of the interviewers was inadequate.

A natural concomitant of the failure to obtain realistic quantitative data on abortions in the United States is the paucity of information on the characteristics of women having abortions and their reasons for having them.

Another area of virtually complete ignorance is the extent and effectiveness of contraceptive efforts among unmarried, but sexually active, adults and especially juveniles; including the role of deliberate or accidental conception in forcing marriage on lovers or parents. These and many other lacunae with regard to knowledge of the reproductive behavior of the American people remain to be filled. It is clear that challenging tasks are in store for the upcoming cohort of American demographers.

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DISCUSSION

Karl A. Smith: Let me first join in paying tribute to Dr. Kiser whom we have all gathered to honour today. As I look around I realize that there are perhaps only two or three of us who have been invited to this Round Table from outside of the United States; I have no doubt that Dr. Kiser had a hand in arranging this. I feel pretty sure that in my case this represents his quiet support for my continuing professional development. I join in wishing Dr. Kiser many years of happy semi-retirement. I say semi-retirement, because I know he would not be happy if he did not continue being active.

In discussing his paper, I should also like to pay tribute to Dr. Tietze who is in some ways a successor to Pearl, in that he has tried to improve on standardized methods for measuring contraceptive effectiveness.

Earlier we heard Dr. Ryder discuss formal measures of fertility and of course the prevalence and effectiveness of family planning are to a considerable extent linked to these measures of fertility. One must be used in assessing some aspects of the other.

Now for specific comments related directly to the paper. My first

comment would be that no clear differentiation is made between clinical and demographic evaluation. Secondly the emphasis in the paper is placed quite rightly on the methodology of research, but I think Dr. Tietze may have indicated for us the extent to which such methodology is used; and of a special interest to me would be the extent of such use outside of the United States. Next, since I have recently had to grapple intellectually with this problem, I should like to have heard more about the method of evaluation of effectiveness used in the cooperative statistical program and especially as compared with that recommended by Potter (*Demography,* 1966), though both are based on life table procedures. It seems to me that Potter's method is not insuperably difficult, and furthermore completely overcomes the objections to the use of Pearl's formula, while the cooperative statistical program procedure perhaps does not. One recognizes, of course, that there is much in common between the two methods and one also recognizes that there are difficulties related to the use of either of these methods in that there must be "cut off periods" over which reporting must take place. One wonders whether this difficulty can be reconciled. An added advantage of a life table method, such as Potter's, is that it can help us to see, at a glance, the extent of discontinuation in a program. This is often a factor that is overlooked by planners and administrators who may delude themselves into thinking that their program is going well, when in fact this may not be the case. I would like to know whether Dr. Tietze thinks that Potter's method as distinct from Tietze's is feasible for use on a national scale. One may observe that countries that have the greatest need for this sort of continuing evaluation usually do not have the expertise or the facilities for engaging in such evaluation. International bodies and foundations can perhaps offer expert help in such endeavours. As you all know the sort of problem about which I have just spoken led to the attempt by Wishik and others to develop evaluative procedures based on what they call the Couple Years of Protection Index, for such places as Pakistan where registration data are deficient; other investigators like Bean and Seltzer have outlined some of the shortcomings of such procedures.

One would like to know what is the present status of the Wishik procedure; has it yet been shown to be feasible? I personally, and I suspect others working in developing countries, would have an interest in this index if it were shown to be useful in such countries.

A word on demographic indices. Demographic is here used in the sense of "what is happening in the community"—the term quoted by

Dr. Tietze a decade or so ago. These indices would include such simple ones as age specific birth rates and fertility rates (standardized if necessary) and of course one would be looking at trends in these rates as indices of what is happening in the community. One acknowledges the problem that exists with respect to registration data, to be discussed in a later section of this Round Table. Again, in this regard Wishik has suggested the use of sample surveys at intervals to assess reproductive performance in the community and thus to contribute to the evaluation of a program.

What of the future? I refer once more to Dr. Ryder's contribution as well as to Dr. Hauser's presentation. I thought I was about to hear Dr. Hauser suggest what I am now going to suggest, but he did not. This is related to *data* rather than to *methodology;* and what I would suggest is that perhaps there is now a place for record linkage as a method which could help in the quick evaluation of both the prevalance and the effectiveness of family planning, using mainly service data. Particularly in developing countries this tool could to some extent replace *practice* surveys as well as supply some of the data which would otherwise come from vital registration. Of course I know that this suggestion tends to evoke a great deal of emotional response, but it is something we should now seriously consider.