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Some Recent Developments in American Fertility Research

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IN 1952, with the analysis of the Indianapolis Study drawing to a close and with an apparent increase in American family size desires and behavior taking place, a Milbank Round Table was held to explore the possibilities for new studies of fertility.¹ Since that time, some of the proposed studies have been undertaken and are at various stages of completion. Three of the studies will be discussed in this paper.

Two of the three studies deal with rather restricted populations and focus on the social and psychological factors which presumably affect fertility behavior. One—the “Princeton Study”—is a sample of 1,165 two parity native-white women living in the seven largest metropolitan areas. Each of the women interviewed had given birth to her second child four to seven months prior to the time of interviewing. The second project—the “Detroit Study”—was undertaken in metropolitan Detroit as part of the general research program of the Detroit Area Study. This was a sample of 221 married fecund women under 33 years old.

The third study under consideration is called the “Growth of American Families.” Its purpose is different from the others in that it seeks to provide a unique set of information concerning the fecundity and planning status of the nation’s population and, also, data on fertility desires and expectations to be used in a set of national population projections. The GAF sample is made up of 2,713 white married women under 40 years of age.

The purpose of this paper is to report on the progress of these three surveys and to show how each of them fits into the findings, problems, and gaps created by the design and analysis of the Indianapolis materials.

The Indianapolis Study

The Indianapolis Study was specifically designed to look into future trends in American fertility through its emphasis on factors affecting

¹ A summary of the round table is reported by: Clyde Kiser, “Exploration of Possibilities for New Studies of Factors Affecting Size of Family,” *Milbank Memorial Fund Quarterly*, Vol. 31, 1953, pp. 436-480.

Note: See introduction to this volume for a fuller description of the studies mentioned in this chapter.

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planned fertility. Most of the 23 hypotheses dealt with planning status and size of planned family. An emphasis on planning status led to what was perhaps the most striking finding of the study. Fertility was inversely related to socio-economic status among the couples who were least successful in family planning while it was directly related to status among the couples who were most successful in planning the number and spacing of their children.² The study did not yield any strong relationships between psychological variables—such as feeling of personal inadequacy, ego-centered interest in children, or felt restriction of personal freedom—and fertility.³

In reviewing the major findings of the Indianapolis Study, it became apparent that there were a number of flaws in the design which made the analysis and interpretation of the data a difficult task.⁴ One problem was the *ex post facto* nature of the study. The independent variables were measured after the birth of children. Thus, in a number of the analyses it became difficult to ascertain the direction of the relationship between the presumed independent variable and family size. In addition to the problem of selectivity, there is the problem of refining the dependent variable in fertility research. Completed size of family represents the cumulation of a series of births, each of which may be affected by a different set of variables. The small magnitude of relationships found in the study has been attributed to the possibility of there being distinct sets of conditions coming into prominence with each birth order.

If the Indianapolis materials were to serve as a guide to further research, it was apparent that future studies should focus on the dynamics of the relationship between fertility and socio-economic status and make some attempt at refining the dependent variable both in order to eliminate the *ex post facto* problem and to gain more precise knowledge of the factors affecting the decision to have children at each birth order. The three studies under discussion have met these objectives with some success.

The Princeton Study

The Princeton Study grew out of the work of a committee whose major purpose was to develop new schemes for the study of differential

² Clyde Kiser and P. K. Whelpton, "Social and Psychological Factors Affecting Fertility. ix. Fertility Planning and Fertility Rates by Socio-Economic Status," *Milbank Memorial Fund Quarterly*, Vol. 27, 1949, pp. 188-244.

³ Charles Westoff and Clyde Kiser, "Social and Psychological Factors Affecting Fertility. xxi. An Empirical Re-Examination and Intercorrelation of Selected Hypothesis Factors," *Milbank Memorial Fund Quarterly*, Vol. 31, 1953, pp. 421-435.

⁴ Clyde Kiser, "General Objectives and Broad Areas of Interest in a Proposed New Study in Fertility," in *Current Research in Human Fertility*, Milbank Memorial Fund, 1955, pp. 115-120.

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fertility.⁵ Various members of the committee presented working papers, of which the ones that seem to be most relevant to the final product were those of Freedman, Mishler, and Westoff and Bensman.

The central idea in Freedman's proposal is that family size is related to the degree of concentration of activities within the family unit. Differences in fertility among social strata, he says, are a reflection of differences in the extent to which activities are family oriented. Thus changes in fertility, over time, are linked to the relative concentration of activities in role systems within and outside the family unit. The cost of having a child is small in cases where a large proportion of the adult functions are already performed in the home or family. Freedman argues that the socio-economic variable has affected fertility not directly but by its impact on the location of such functions inside or outside the family.⁶

In the Mishler proposal, fertility is viewed as a social event which is dependent upon a system of psychologically meaningful relationships. His major assumption is that fertility will be depressed if raising children interferes with the needs of the husband or wife. The central hypothesis states that the probability of adding a child to the family is inversely related to the discrepancy between the desired social structure and the structure required for child rearing. As the number of desired statuses increases, the number of rights, obligations, and demands also increases. The addition of a child makes the maintenance of existing statuses more difficult. Mishler points out that high income and education have been accompanied by low fertility and argues that the variables income and education produce the opportunity for many distinct statuses which will be in competition with the parental role.⁷

In the Westoff and Bensman proposal, it is suggested that family size is but one value in a whole scale of values held by the couple. The scale of values is labeled "life plan" and it is assumed that fertility values must be consistent with the other values in the life plan. A work-mobility-success orientation is hypothesized as being incompatible with a family orientation. Thus, the person committed to upward mobility will spend his leisure time in matters related to self-improvement rather than in

⁵ The Steering Committee on the Development of Plans for New Studies in Fertility is sponsored jointly by the Milbank Memorial Fund and the Population Council. The Committee consists of Frank Notestein, chairman; Ronald Freedman, Philip Hauser, Clyde Kiser, Frank Lorimer, Frederick Osborn, Lowell Reed, and P. K. Whelpton. Elliot Mishler, Robert Potter, Philip Sagi, and Charles Westoff are members of the staff.

⁶ Ronald Freedman, "The Family Function Approach to Fertility Studies," *Ann Arbor*, 1954, mimeographed.

⁷ Elliot Mishler, "Problems of Method and Theory in the Social Psychological Study of Fertility," Princeton, 1954, mimeographed.

consuming the benefits of his children. The authors suggest that differences in mobility orientations among the social classes have been at least partially responsible for the pattern of differential fertility observed in Western societies.⁸

Note that each of these proposals assumes that there is a systematic relationship between family structure and fertility. And each one attempts to show how differences in family structure, whether labeled "family activity ratio," "status field," or "life plan," have produced the pattern of differential fertility observed over the past hundred years or so. Each seemed to agree that family structure was the variable underlying socio-economic differences in fertility. This was in no way out of line with previous ideas about the subject. The most widely held but least researched hypothesis concerning the decline in family size had to do with the changing function of the family. Furthermore, while none of the original Indianapolis hypotheses were concerned with the relationship between extra-familial activity and family size, Pratt and Whelpton found that the amount of club and work activity was inversely associated with number of children.⁹ This relationship held for all socio-economic groups.

The final product of these working papers was the Princeton Study proposal, developed by Mishler and Westoff. It includes some 40 hypotheses in an attempt to integrate the various proposals. These hypotheses were spelled out in considerable detail in a recent paper.¹⁰ Let us mention only some of the themes and then get on to the preliminary findings.

Mishler and Westoff focus on two broad questions: What styles of life or family organization are compatible with child rearing? What factors of personality are relevant to family size decisions? The idea of using personality as a major independent variable had not appeared in the previous proposals, but had been discussed at some length at the 1952 Milbank Round Table. To me, one of the peculiar developments resulting from the Indianapolis Study was the fact that in spite of the weak and inconsistent relationship of psychological characteristics to

⁸ Charles Westoff and Joseph Bensman, "The Social Mobility Model Restated with Some Research Implications and Suggestions," Oxford, Ohio, 1954, mimeographed.

⁹ Lois Pratt and P. K. Whelpton, "Social and Psychological Factors Affecting Fertility. xxx. Extra-Familial Participation of Wives in Relation to Interest in and Liking for Children, Fertility Planning, and Actual and Desired Family Size," *Milbank Memorial Fund Quarterly*, Vol. 34, 1956, pp. 44-78.

¹⁰ Elliot Mishler and Charles Westoff, "A Proposal for Research on Social Psychological Factors Affecting Fertility: Concepts and Hypotheses," in *Current Research in Human Fertility*, Milbank Memorial Fund, 1955, pp. 121-150.

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family size, a number of the investigators felt that the next logical step in fertility research should emphasize personal motivations. Some of the personality variables included in the Princeton Study are ambiguity tolerance, manifest anxiety, impulse gratification, nurture needs, and compulsiveness. The hypothesis around which most of the personality variables are organized is that excessive and unsatisfied dependency needs are incompatible with desires for children.

Hypotheses at the family organization level are an extension of the working papers. They include not only hypotheses about the extent to which activities are located in the family or home, but hypotheses concerning aspects of the structure of the family such as marital adjustment, homogeneity in the characteristics of the marital partners, husband-wife dominance, and division of labor within the household. At both the individual level and the family group level the mobility theme of the Westoff-Bensman proposal is restated.

Another theme developed in the proposal, and incidentally one which may ultimately prove fruitful in cross-cultural studies, is a consideration of the relative benevolence of the environment in defraying the economic and social costs of having children. Here it is hypothesized that the greater the amount of help available from one's community or kinship system, the weaker the desire to prevent or postpone pregnancy.

In the study design it was felt that completed family size was too complex to deal with, given our present understanding of the problem. So the dependent variable is broken into its component parts. The design originally called for an examination of factors affecting the postponement of a first pregnancy among newly married couples and factors affecting the probability of occurrence or the timing of third pregnancies among two parity couples. However, the cost of carrying out both of the studies resulted in limiting the field work to women who had recently given birth to their second child. At present it is expected that the 1,165 women who were interviewed in the fall of 1956 will be reinterviewed in 1960.

Mishler and Westoff state that the hypotheses presented in their paper are not graded in order of priority for study. Some months ago, I received a copy of a code covering 81 variables in the study. While this does not exhaust all the variables from the study, it must reflect some of the major interests.

Of the 81 variables, 16 represent either the dependent or control variables. These include total fertility desires of husband and wife, expected third birth interval, contraceptive efficiency, age, and age at marriage. Of the remaining 65 independent variables, nearly half are

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devoted to mobility or personality measures. Mobility is measured in at least 16 ways, including intra- and inter-generation mobility plus scales of mobility drive or commitment. The standard socio-economic data are represented by ten variables. Six variables are given to a theme which concerns the degree of felt control over the social environment. For example, scales of achievement of life goals and feelings of economic security are included. Religious interest is measured by four scales. The remaining variables cover a wide range of topics and levels of abstraction—husband-wife dominance, I.Q., availability of help in child rearing, heterogeneity in the backgrounds of husband and wife, and so on.

Clearly, the central theme running through all the working papers has been slighted. The original papers as well as the final product by Mishler and Westoff stressed the relative participation in familial or extra-familial social roles as related to fertility desires. Granting that mobility aspirations are one part of this general theme, the schedule is barren with respect to other materials which could illuminate the division of labor between the family and other social institutions. There are virtually no data dealing with the locus of leisure activities, extra-familial involvement, or participation in the kinship network. We are left only with measures of the internal structure of the family unit such as marital adjustment, power relations, and homogamy.

The data collected on mobility in the Princeton Study represent the most thorough exploration of the topic until now. Demographers have frequently suggested that upward mobility and familism are incompatible. However, the few American studies showing consistent differences in fertility by mobility have been restricted to small segments of the population, usually elite groups.¹¹ In the present study each of the mobility measures is related, in the predicted direction, to the family size desires of the two parity women. That is, the greater the mobility experienced by the couple or the greater the drive to get ahead, the smaller the number of children desired. But the magnitudes of the relationships for the total sample are extremely small. The absolute values of the Pearsonian correlations between the sixteen measures of mobility and number of children desired by the wife range from 0.008 to 0.187. Only five of the 16 measures account for more than one per cent of the variance in the dependent variable. From the initial tabulations it would appear that many of our hunches concerning the impact of mobility on familism were grossly inaccurate, outdated, or, at best, inoperative at this stage of

¹¹ E. Digby Baltzell, "Social Mobility and Fertility Within an Elite Group," *Milbank Memorial Fund Quarterly*, Vol. 31, 1953, pp. 411-420.

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family development. Of course, these are only preliminary runs. I am certain the investigators will make some attempt to isolate those elements of the population in which the mobility variable is operative and those in which it is not.

From the collection of 14 personality measures, five account for more than one per cent of the variance, the largest correlation being 0.136. My objection to the use of personality as a major independent variable in a fertility study has nothing to do with the size of the correlations. It is simply out of the mainstream of our knowledge about trends and differences in fertility. Any hypotheses about fertility which make up part of a theoretical scheme should give us a more comprehensive knowledge of relationships observed in the past. They should help us understand changes in fertility over time and differences among societies. In the working papers, hypotheses dealing with the performance of functions in the family or other social units were precisely an effort to close the gap between levels of fertility and degree of urbanization-industrialization or levels of fertility and position in the social system. Unless the personality hypotheses are linked to social system variables the size of the correlations makes little difference. A correlation between ambiguity tolerance and fertility desires of 0.80 instead of 0.11, as it actually stands, will contribute little if anything until we can demonstrate that changes in social organization are accompanied by changes in personality structure.

The correlations between the socio-economic variables and fertility desires are low and inconsistent. Desired family size has no relationship with prestige of occupation, has a slight negative relationship with income or positive changes in income, and a slight positive relationship with education of husband and wife. The strongest association is that between income change and desired number of children, — 0.169. These findings are consistent with some recent census data, in the sense that they are an extension of the convergence pattern and they are almost identical with the GAF and Detroit Studies. The data may reflect changes in our economic system which has witnessed a minimization of income and occupational differences through the leveling of pay, the mushrooming credit structure, and the rationalization of white collar jobs.

Among the other independent variables, none of those dealing with the internal family structure, such as husband-wife dominance and marital adjustment, or perceived control over environment, account for more than one per cent of the variance in number of children desired. Peculiarly, one item, a question dealing with the relevance of finances in having another child yields the strongest correlation in the study, 0.373.

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Women who feel that finances have nothing to do with adding children to the family are the ones desiring the largest number of children.

One group of variables yields relatively high correlations. The association between the four religious interest variables and desired family size ranges from + 0.187 to + 0.286. Being closely related to one another, the multiple of the four items with fertility is 0.310. While religious groups differ in their ideas about the use of contraception, all of them place considerable emphasis on the values of family life. Religiosity may also be linked to certain aspects of family structure, particularly with respect to the role of women. It seems likely that participation in religious activities reinforces the traditional female role with its emphasis on home- or family-centered activities. I believe that the relatively high correlations between religious activity and fertility in the Princeton Study may be a rough index of the impact of family-oriented activities on desired number of children. Religiosity and traditionalism were among the better predictors of fertility in the Indianapolis Study as well. Using data from the Borgatta-Westoff articles,¹² I find that the two measures of traditionalism together with a measure of religious activity predict completed family size as well as or better than the combination of income, education, and occupation when fertility planning status is held constant. In the total Indianapolis sample we obtain the following results through the use of the multiple-partial correlation:

$$r_1(3, 4, 5) \cdot 2, 6 = 0.204$$

$$r_1(6, 7, 8) \cdot 2 = 0.140$$

And in the "number and spacing planned" group we obtain the following:

$$r_1(3, 4, 5) \cdot 6 = 0.242$$

$$R_{1.678} = 0.203$$

where: 1 = completed family size	5 = religious interest
2 = fertility planning status	6 = income
3 = traditionalism in female role	7 = education
4 = general traditionalism	8 = occupational prestige

¹² Edgar Borgatta and Charles Westoff, "Social and Psychological Factors Affecting Fertility. xxv. The Prediction of Total Fertility," *Milbank Memorial Fund Quarterly*, Vol. 32, 1954, pp. 383-419.

Charles Westoff and Edgar Borgatta, "Social and Psychological Factors Affecting Fertility. xxvi. The Prediction of Planned Fertility," *Milbank Memorial Fund Quarterly*, Vol. 33, 1955, pp. 50-62.

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Although some facets of family structure have been ignored in the Princeton Study, the schedule provides us with many opportunities to explore certain areas which up to now have been untapped. These include: (1) an analysis of differences in factors affecting fertility among those segments of the population within the large scale bureaucratic setting and those outside its boundaries, the purpose being to make use of cross-sectional data in order to reconstruct the effect of organizational changes on work-mobility ideology and family orientations;¹³ (2) an analysis of differences in the variables that influence the family size desires of husband and wife.

The Detroit Study

I can elaborate on the factors affecting family size desires of husband and wife by reference to the findings of the Detroit Study. In contrast to the Princeton Study this one included young married women (17-32) of all parities. The dependent variable was expected number of children. The similarity of the Detroit Study to some of the working papers was not coincidental.¹⁴ Emphasis was placed on the underlying differences in family activities which may have provided the basis for the differences in fertility associated with economic differences in the past.

Probably the most striking feature of the Detroit data is the demonstration that there are two distinctly different sets of conditions which influence men and women in their family size decisions.¹⁵ Among wife-dominant couples, fertility norms and behavior are conditioned by the extent of the wife's participation in the kinship network and home-centered roles. Kin contact (+ 0.165), organizational participation of the wife (- 0.183), the production of goods and services in the home (+ 0.136), and a home-centered leisure pattern (+ 0.318) are all related in the expected direction to the decision to have additional children. Partialing out the influence of number of children already born and age of the wife, the combined influence of the above variables as measured by the multiple partial is 0.375.

In the husband-dominant families, the activity variables have no impact on expected family size. Instead, status considerations seem to play the most prominent part in the decision to add children to the family, high status being associated with relatively high fertility. A

¹³ This theme is elaborated in the Westoff and Bensusan proposal, *op. cit.*

¹⁴ Ronald Freedman, author of one of the original working papers, was an active participant in the study.

¹⁵ David Goldberg, "Family Role Structure and Fertility." Paper presented at the meetings of the Population Association of America, 1957.

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combination of four socio-economic indicators yields a multiple correlation of 0.384 with expected number of children. Thus the number of children born to a family may represent a compromise between the desires of husband and wife since the pressures exerted by each have a tendency to cancel one another. As the socio-economic status of the family becomes higher, the wife is likely to be exposed to areas of consumption which shift the balance of roles away from home and family. Under these conditions the husband may want a relatively large family, as his status position will accommodate, while the wife may exert pressures in the opposite direction because a large family would put considerable strain on her role pattern. These divergent influence patterns help us to account for the relatively small social class differences in fertility found in the youngest cohorts.

Another set of data from the Detroit Study amplifies our understanding of the process of family building. In the Princeton Study, the decision to concentrate on the family size desires of a group of women all of the same parity was guided by the assumption that the reasons for adding children to the family vary with the number that are already in the family. Factors influencing the family size decisions of newly married couples are different from those involved in the decision to add a third child in a two child family. Since the Detroit sample includes couples at various stages in the family building process, we can explore the merits of the assumption.

For couples that have no children or one child, the variable that seems to have the most influence on number of children wanted or expected is the proportion of leisure activities of the wife that are home-centered. Also related to large family size desires or expectations are the variables that measure the amount of contact with the kin group. In general, the socio-economic standing of the family is inversely related to the dependent variable.

Among couples that already have three or more children, the decision to have additional children is influenced in a completely different manner. The syndrome of status characteristics is directly related to expectations, whereas home-centered leisure and kin contact either lose their importance or are negatively associated with expectations. Factors influencing families which now have two children lie somewhere between these polar types.

Although the correlation coefficients are rather small, the findings are fairly consistent. The data show an apparent shift in the kinds of variables that influence family size decisions during the process of family growth.

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The partial correlations between the independent variables and expected number of children, holding constant age and number of children already born, for the 0-1 parity and 3+ parity couples are summarized below:

home centered leisure of wife	from +0.351 to +0.036
frequency of family gatherings	from +0.144 to -0.123
per cent of all visits with relatives	from +0.130 to -0.048
participation in formal organizations	from -0.139 to +0.156
income	from -0.151 to +0.105
education	from +0.046 to +0.194

The multiple using leisure and kin contact to predict expected number of children for the zero and one parity couples is 0.382. The combination of income, education, and organizational participation on the expectations of the three-plus parity couples yields a multiple of 0.285. An overwhelming part of the variance remains unexplained. But I think we have enough information to tell us that a zero parity or *n* parity study will unfold only part of the story.

Why are young couples influenced by the leisure pursuits of the wife or kinship contact, whereas couples having two or three children add to the family on the basis of their socio-economic position? Or to put it another way, why are the female variables important early in marriage, while the male variables are important later in marriage? Much as I would like to believe that the balance of power shifts from wife to husband as the marriage matures, our data do not support the hypothesis. The change in the variables may result from the fact that a first child will grossly alter the activity pattern of a couple but that additional children necessitate only minor shifts in the already existing parental roles. Once the parent is committed to a home-centered role structure in order to provide the services for child care, adding children to the family becomes a type of luxury item rather than a revolution in daily activities.

The Detroit Study data fill some of the gaps in the more comprehensive Princeton Study. It raises some questions regarding types of variables affecting husbands' and wives' decisions about family size which may be examined more intensively in the larger sample of the Princeton Study. And it provides an all-parity sample base which may be used to evaluate some of the two parity findings. But like the Princeton Study, it fails to account for a large part of the variation in fertility.

Growth of American Families Study

In the same sense that the Princeton and Detroit Studies complement one another by their division of labor in the choice of independent

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variables, the Freedman-Whelpton-Campbell Growth of American Families Study complements the research efforts of the other two studies.¹⁶

One purpose of the GAF Study is to map the relationship between socio-economic or cultural variables and an extensive set of fertility data. The achievement of this objective provides us with national data which were previously available only from the Indianapolis sample. On a national basis our knowledge of differential fertility is extended both on the independent and dependent variable sides. In addition to the types of data available in census publications, GAF provides information about religion; occupational mobility, personal economic perspectives, rural-urban origin, and work history of the wife, and social class identification. The fertility data include the use, attitudes, and expectations about family limitation practices, actual, expected, and ideal family size, and fecundity information. A large part of these data are then used for a projection of births in the coming five-year periods.

The results of the investigation show that while fecundity impairments are widespread, their complete elimination would only lead to an increase in births by about 10 or 15 per cent. Most cases of subfecundity develop after the birth of children. Complete sterility exists in less than four per cent of the sample. A cross-section of married women under 40 indicates that 10 per cent are definitely sterile, most of them having had operations making additional live births impossible, and an additional 24 per cent have some type of fecundity impairment, ranging from cases in which conception may be possible but dangerous to the mother's health, to cases in which the only difficulty encountered was having children at a relatively slow rate when contraception was not used. Among the two out of three women for whom there is no evidence of fecundity impairment it seems likely that there is some hidden subfecundity which could not be tested because of the continual use of contraceptives. About half of the women aged 35-39 or married at least 15 years are subfecund.

The GAF data clearly demonstrate that the overwhelming majority of couples in all major socio-economic strata use family limitation practices. Differences in the proportion of users among the strata are fairly small. Nearly 95 per cent of the fecund couples are past or future users (including rhythm and douche "for cleanliness only" users). Alternative practices and effectiveness of use vary to a greater extent among the major strata than differences in the proportion of users.

Although nearly all couples in the sample use some form of contraception,

¹⁶ All data in this section of the paper are taken from the forthcoming Freedman, Whelpton, and Campbell monograph, *Family Planning, Sterility, and Population Growth*, McGraw-Hill, 1959.

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only a small proportion of couples have completely planned families. Nineteen per cent of the women conceived only at times when contraception was interrupted in order to have a child. Only one family in ten is completely planned after 15 years of marriage. The most common pattern is the partially planned family. Two-thirds of the couples fall into this heterogeneous category including some non-users who desired all their pregnancies. Excess fertility (most recent pregnancy was unwanted at that time or later by either marriage partner) was reported by 13 per cent of the women.

These data raise some questions about the meaning of the Indianapolis analysis of factors affecting fertility in the "number and spacing planned" group. An emphasis on size of planned family was important as a means of looking into future fertility patterns, but restriction of the analysis to number and spacing planned may have been drawing the line at the wrong point. Most American couples are able to plan their families in the sense of having the number of children they desire. The planned family, however, is not usually achieved by stopping the use of contraception for each pregnancy. The completed planned family is likely to be a minority pattern in American society for many years to come, particularly in an abundant economy.

Religion and education seem to be the most important variables in differentiating family limitation patterns. About 93 per cent of the fecund Protestant couples and 79 per cent of the fecund Catholic couples have used contraception. These differences become smaller with increasing age or duration of marriage. Many Catholic couples start late, not beginning the use of contraception until they have had at least one child. More than half of the Protestant users but less than one-third of the Catholic users began using contraception before the first pregnancy. Even Catholics who do not attend church are less likely to be users than Protestants, regardless of church attendance.

Among the socio-economic variables, education is the only one that consistently produces differences in planning practices. More than a third of the college women who are fecund and use contraception have completely planned families. The comparable proportion for grade school women is less than one in ten.

The authors assert that the widespread use of some form of family limitation could result in great variability in the birth rate for the coming years. If new members of the population are added in waves rather than in a continuous flow, the consequences for institutional growth and decline are considerable.

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The American public has apparently reached consensus on families including two to four children. Three-fourths of the women interviewed expect to have two to four children, 85 per cent said they would have two to four if they could live their lives over again, and 94 per cent consider two to four children ideal for the average American family. Most of the women expecting less than two children are subfecund while a substantial number of those expecting five or more children don't want them.

The 1871-1875 cohort of native-white married women had an average of four children. Smallest family size was attained in the 1906-1915 cohorts, an average of 2.4 children. GAF data cover the cohorts of 1916-1937. The women report an average expected number of children of 3.0, having already produced about two children.

The study data are suggestive of a continuation of the narrowing socio-economic differences in fertility. Differences in expected family size by income and occupation are very small. Once more, education produces the largest differences in fertility behavior. Grade school women expect an average of 3.6 children, while all others expect an average of just less than three.

What does the likely increase in family size mean in terms of the future growth of the American population? The authors answer this question by a method that is unique in population projections. Forecasts of fertility rates are made by combining past rates with the future rates implied by the expectation data. In addition, adjustments are made to include the women who were not interviewed, the divorced, widowed, separated, and single women who will marry.

Expectations of future births are checked against the record of cohorts of women who have already completed their families. For example, women 30-34 expect an additional 578 children per 1,000 women. The minimum and maximum number of births from age 30-34 to the end of the child-bearing period as recorded in the actual cohorts tables, is 362 and 673. Additional expectations of women aged 20-24 and 25-29 are also in line with the experience of previous cohorts.

It is the anticipated timing of births which the authors doubt. They feel that the women who were interviewed overstated the number of children they would have in the five years after the interview. Using the example of the women aged 30-34 again, the 578 additional children per thousand were distributed as follows by the women themselves: 530 children per 1,000 in the next five years, which would be a record high in the cohort tables, and 48 per 1,000 from ages 35-39 to the end of the

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childbearing period, a record low. Because the stated patterns seem unlikely, the timing of future births has been adjusted to a distribution that takes into account the experience of previous cohorts. In addition to the "adjusted" projections that were made, I would like to have seen the women's expectations projected without adjustment for "reasonable" timing patterns. Since the projections are age-based rather than duration-based, a shift to younger marriage age (as has occurred) *can* result in record highs and lows for given age intervals even if the timing pattern of births by duration of marriage remains unchanged.

The survival rates used in the projections are those prepared by Greville and the migration rates are the ones developed by the Census Bureau. Neither mortality nor immigration will have much influence on the future growth of American population. Greater changes can result from small changes in family size, length of generation, and proportion marrying. A 10 per cent deviation in completed family size together with comparable changes in proportion marrying and median age at child-birth would produce about a 20 per cent difference in the medium projections by the latter part of the twentieth century.

The medium projections imply an average crude birth rate of about 21-22 and a crude death rate of 8 or 9. Results of the cohort projections are similar to the component projections of the Census Bureau. The cohort projections do, however, give us a much broader base for evaluating the results by spelling out in detail a larger number of assumptions, such as how many women marry, when they marry, how the children are distributed over time, and the resultant family size.

I have not been able to do justice to the research efforts involved in the three fertility studies. Clearly, they emphasize the need for research which will pinpoint the accuracy of expected family size data both on an individual and group basis and call for additional research into the family building process, perhaps with a zero parity study.

COMMENT

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Since much of this paper concentrates on the "Princeton Study," with which I have been connected for four years, I would like to reply to a number of specific criticisms.

Goldberg finds it strange, considering the lack of encouraging results in the Indianapolis Study, that personal motivations are stressed so in the follow-up Princeton Study. If his generalization is true that the three

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different proposals preceding the final formulation of this study are in fact saying the same thing, then the emphasis on "personal motivations" is simply the level at which a number of "family structure" variables are being measured. Thus, "psychological commitment to work" can be regarded either as measuring an individual interest in work or as an index of an extra-familial orientation.

Although I think there is some confusion here between theory and measurement, the basic criticism that the interview schedule is "barren with respect to other materials which could illuminate the division of labor between the family and other social institutions" is essentially accurate. Particularly there is a lack of attention paid to the locus of leisure activities or participation in the kinship network. The version of the schedule pretested on a probability sample of 100 couples focused much more than did the final version on the internal social structure of the family. However, the underlying theme stresses the capacity of the internal distribution of role responsibilities to absorb the strains resulting from the addition of another child rather than the "division of labor between the family and other social institutions." The former type of variable was minimized simply because there was little pay-off in the pretest; as conceptualized and measured the variable simply did not relate to the various fertility indicators. In view of Goldberg's reported success in the Detroit Study with such factors as extent of home-centered leisure of the wife, frequency of family gatherings, visits with relatives, and participation in extra-familial organizations, it is tempting to consider including at least some of these measures in the second interview with the same couples. However, his own results indicate their decreasing importance with increasing parity. If this is a reliable generalization, such factors would be of little value in our sample. Nor would they have paid off particularly in our original sample of two parity women if I may "interpolate" a little in the correlational values for the 0-1 and 3+ parities.

Goldberg concludes that the exploration of the social mobility hypothesis has produced correlations of only negligible value. This is basically correct but, as his own data on different variables indicate, correlations with family size desires seem to depend upon what stage in the family-building process is considered. It may very well be that mobility is irrelevant at this stage but that it was quite relevant in deterring some couples from ever having two children, or that it might affect future fertility. I think it would be fairer to conclude that mobility considerations seem unimportant at this stage of the process rather than that "many of our hunches concerning the impact of mobility on familism were grossly

inaccurate." Although difficult problems of conceptualization and measurement remain, I would agree with Goldberg's alternative explanation that our hunches may have been outdated. The whole ideology of "getting ahead" has changed in the postwar period from one reflecting the competitive individualism of the small entrepreneur period of American history to one of association with the large organization's visible channels of mobility. And with the increased economic security and credit psychology of the postwar period, there seems to be little economic or career need for postponing a family. The fertility rates of graduate students at many university housing developments bear eloquent testimony to this new climate.

One of the most interesting but also perplexing sections of Goldberg's paper is introduced by the statement: "Probably the most striking feature of the Detroit data is the demonstration that there are two distinctly different sets of conditions which influence men and women in their family size decisions." In the Princeton Study, wives and husbands were asked independently for the total number of children they desired.¹ Each is treated as a dependent variable in the analysis so that each was correlated separately with 79 other variables on the total sample as well as within the religious and class subdivisions of the sample. A graphing of correlational values for these 79 "observations" reveals a very close correspondence; in fact, this correlation of correlations reaches 0.96. Even considering the questionable properties of such a statistic as, for example, the influence of low reliabilities, it seems safe to conclude, for this sample at least, that no new information is to be gained by utilizing both variables in the general correlational analysis.²

The confusing aspect of Goldberg's description lies in the implication that he is referring to analyses within two control groups of (1) wife-dominant families for whom familial interests correlate with the decision to have additional children, and (2) husband-dominant families among whom socio-economic considerations best predict expected fertility.

A little further elaboration by the author would clarify such questions as (a) does this dichotomy exhaust the sample? (b) if these are different couples, I do not understand how "the number of children born to a family may represent a compromise between the desires of husband and wife since the pressures exerted by each have a tendency to cancel one another." It would seem more likely that the fertility of different segments of the population is predictable from different variables much in the same

¹ The correlation between the two is 0.65 on a sample of 1,165 couples.

² The same high correspondence prevails in the subsamples.

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way that we in the Princeton Study seem to be finding different factors operating for Protestants, Catholics, and Jews.

Perhaps the most fascinating analysis reported by Goldberg is that different factors seem to affect fertility decisions at different stages in the family building process. Thus, he reports that home-centered leisure and kinship contact of the wife with no children, or only one child, correlates positively with decisions to add another child, whereas income correlates negatively, and education not at all. However, among couples with three or more children, attitudes toward further enlarging the family relate positively both to income and education while the "female" variables diminish in importance or become negatively associated. Although further elaboration, if not analysis, is necessary to relate this to the earlier generalizations about factors affecting the fertility decisions of males and females, this finding is very exciting in theory and tends to confirm the strategy of the Princeton Study which controlled parity by design. I would prefer Goldberg's conclusion that "we have enough information to tell us that a zero parity or n parity will unfold only part of the story," if the word "only" were left out.

One of the chronic complaints about research in this area generally is that the correlations tend to be quite low. In fact, if the value of a zero-order correlation reaches 0.20, we tend to be pleased; if it reaches 0.30 or 0.40 there is jubilation; on the rare occasions when it exceeds this, the usual reaction is distrust. Either there has been a clerical error or the variables are in fact the same phenomenon. I have heard it only half-jokingly proposed that all correlations over 0.30 should be checked again on the assumption that they must be in error.

It is clearly possible that our theories may be simply invalid or that our measures contain too much unreliability, but I am inclined to believe rather that our expectations are simply naive. Why should a variable so complex even as number of children *desired* yield to single unidimensional predictors? Fertility is a complex variable, a fact that researchers dealing with individuals rather than aggregates must take into account in their expectations. I believe that we will be doing very well to achieve a 50 per cent control of the variance with a battery composed of perhaps 20 sociological and psychological predictors.

In conclusion, I would like to applaud the over-all objective of Goldberg's paper, which is to evaluate the theoretical implications of a few of the chief findings of the major fertility studies currently in process. He has made it dramatically clear that more integration of research efforts is desirable.