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A MODEL OF FERTILITY BY PLANNING STATUS

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Abstract—Data from the 1965 and 1970 National Fertility Studies are used to produce classifications of births and exposure to births by planning status for real and synthetic marriage cohorts, covering the experience of the 1940s, 1950s, and 1960s. Indices of reproductive input (three ends, three means, and three conditions) and of conventional reproductive output (such as the mean number of unintended births) are derived from these observations. The outputs are expressed as deterministic equations in terms of the inputs; this permits the study of the structure of fertility determination. The model provides a parsimonious description of the sources of change in fertility and an instrument of possible use in policy.

Fertility surveys permit the development of a system of variables intermediate between the underlying independent variables, such as religion and socioeconomic status, and the reproductive outcomes. One purpose of the present paper is to summarize that system of variables in a time series, showing the change in each variable during the postwar era in the United States. Specifically, the data concern the planning statuses of births, and of person-years of exposure to risk, across the reproductive life span and measures derivative therefrom. The idea behind this work was first described in a recent article (Ryder, 1976b).

The data used in this article come from the 1965 and 1970 National Fertility Studies. A model is implicit in the particular sections of those questionnaires to be used here. Couples are conceptualized as having both quantitative and temporal intentions with respect to childbearing. They intend a particular number of births and at a particular tempo, e.g., as soon as possible or only after some delay. Those who intend to delay the next birth or to terminate their childbearing with the last birth are more or less likely to use contraception to achieve those ends and, if they do so, are more or less likely to be successful in that pursuit. From the perspective of this model, the fertility that occurs is seen as the consequence of that set of actions, guided by intentions, within a context of reproductive conditions, such as the overall length of exposure to risk and the level of fecundability.

The object of a fertility survey in this regard is to "explain" a particular reproductive outcome—whether it be the reproductive histories collected in our surveys or the outputs of the official systems of registration and enumeration—as a consequence of particular values of those ends, means, and conditions. This sets the stage for the investigation, not of the correlates of fertility itself, but of the correlates of the ends, means, and conditions which determine fertility. Before we had access to the kind of intermediate information about reproductive behavior that can be provided by surveys, we had ample evidence of the relationships between fertility measures and explanatory variables but were obliged to infer the sources of those relationships in variations of reproductive ends, means, and conditions.

The growth of fertility surveys has expanded greatly the detail of information concerning reproductive behavior, in particular the analysis of individual pregnancy intervals in which contraception may be used (the study of contraceptive efficacy) and intervals in which contraception is not used (the study of fecundability). The measurement procedures appropriate to these detailed inquiries are substantially different from those employed in summarizing the typical products of the official registration and enumeration systems. An underlying purpose of the present paper is to provide a methodological bridge between these two kinds of analysis (Ryder, 1971).

In what follows, we report observations of the planning statuses of births and of the person-years of exposure to risk of births. These observations are used to develop two kinds of index: (a) a system of inputs, identified as reproductive ends, means, and conditions; and (b) various conventional outputs, such as the mean number of births per woman over the course of a reproductive lifetime. Because of the approach adopted, we can develop algebraic relationships between each output and the various inputs responsible for the value of that output. The resultant equations display the structure of fertility, i.e., the nature of the linkage between each input and each output. They may be used to determine the extent of modification of each kind of behavior which would be required to achieve a particular fertility response. The indices of input and output provide a parsimonious summary of postwar changes in American fertility, and a precise measurement of the contribution of changes in each input, over time, to changes in fertility, within the limits of the reliability and comparability of the basic data.

OBSERVATIONS

The ideal record for the reproductive lifetime of a cohort of women, in terms of present purposes, would be one that permitted us to classify both occurrences and person-years of exposure to risk with respect to reproductive intention and the use of contraceptive methods, as well as with respect to the outcome. Each birth would be classified as wanted now or later or never, each episode of exposure would be characterized as involving use or nonuse of a method, and the success or failure of actions with respect to each episode would be recorded. Analyses of such kinds are familiar to readers of our books on the National Fertility Studies (Ryder and Westoff, 1971; Westoff and Ryder, 1977). The most difficult problems in practice arise with respect to exposure to risk. It would be desirable to partition the entire reproductive span into segments of exposure and nonexposure. There are three types of nonexposure: periods of noncopulation, unexposed periods associated with pregnancy and the subsequent anovulatory interval, and periods of nonexposure because the couple is sterile. For the remaining segments of net exposure, it would be desirable to classify each month by use or nonuse of contraception, and by intention—meaning operationally how one would classify a conception if it were to occur in that month.

The data we have collected are less than ideal for such an assignment. Some information we did not collect, either because we feared to give offense to the respondent or because we anticipated an unreliable result (because it would put too much strain on recall or would be liable to be misreported). We regret to note that there was also some information we neglected to obtain because we had insufficient foresight, and some information which we did collect but are reluctant to use because of evidence that it is unreliable.

In the present model, the occurrences, although for simplicity referred to as births, are, in fact, fertile pregnancies—i.e., twins or triplets are counted as one. In previous work, we have also treated a current pregnancy as a fertile pregnancy. That practice is not followed here, partly because some current pregnancies have an infertile outcome, and partly because we have evidence that current pregnancies are substantially underreported (Ryder, 1976b). For many respondents, the records for years 1965 and 1970 in the two surveys, respectively, were incomplete because they were interviewed before the end of the year. We resolved this problem and that of the current pregnancy by terminating our accounting with the beginning of 1965 and 1970. This is not a happy solution, because it sacrifices some recent data. To avoid that, we are adopting a different procedure in a new version of the model.

In the questionnaire, we also solicited information about each infertile pregnancy and collected the full panoply of information about its planning status as well. We estimate, however, that only one-half of the infertile pregnancies with a spontaneous outcome, and perhaps one-tenth of those with an induced outcome, were reported. This has led us to ignore the data for infertile pregnancies. In terms of the ambition to provide a kind of bridge between inquiries into contraceptive efficacy and fecundability (keyed to all conceptions) and those derivative from official registration and enumeration data (keyed to births), the decision leans in the direction of the latter rather than the former. It has an unfortunate consequence with respect to the record of exposure. Some of the fertile pregnancies come at the end of an interbirth interval which encompasses an infertile pregnancy (identified by its planning status) and a period of pregnancy and puerperium subsequent to that conception, as well as the fertile pregnancy (identified by its planning status). Our decision to suppress the evidence for the earlier event is tantamount to assigning the planning status of the later event to the entire interval. Otherwise said, had we anticipated the inadequacy of the record of infertile pregnancies, we would have tailored the questionnaire to successive birth intervals rather than pregnancy intervals. Note also the implication that one form of fertility regulation, the resort to induced abortion, is absent from our accounting; this is the single most grievous flaw in our analysis.

Before we turn to the difficult questions of classification of exposure to risk, one final reservation must be expressed concerning our classification of births by planning status. Only three options are provided a respondent for classifying a birth by intention—it was intended then, or later, or never. This leaves no opportunity for an important alternative—the deferred decision. This would arise in the common situation in which a couple were using contraception in order for the woman not to become pregnant at that time and had not yet reached a decision as to whether or not they would eventually try to have a baby. If the wife of such a couple became pregnant and had a baby, the couple would have no answer for us concerning whether that baby was wanted later or never.

The evidence concerning the classification of exposure to risk is inherently more intractable than that for births. The principles are clear: one has a length of time constituting gross potential exposure, beginning with some initiation date and ending when the record for that marriage cohort is closed. (By confining ourselves here to the case of a marriage which remains intact, we defer for the time being the question of the consequences of dissolution of marriage.) From that gross exposure, we subtract intervals of nonexposure because of noncopulation or because of nonovulation—the latter associated either with pregnancy or with the couple's eventual sterility. Then the remaining net exposure time is classified by the planning status one would assign to a conception if it were to occur in the month concerned.

The first question concerns the date of initiation of exposure. It is evident from the presence of premarital conceptions and births in our records that exposure frequently, perhaps commonly, begins at some time prior to first marriage, but we did not have the courage to ask our respondents when that was. Because this information was missing, we chose, in the study of pregnancy intervals from the 1965 survey (Ryder and Westoff, 1971), to exclude those intervals which ended in a premarital conception or birth—an unsatisfactory choice because the remaining intervals are select. In the comparable exercise from the 1970 survey (Westoff and Ryder, 1977), our decision was to ignore all first intervals, but that is also an unsatisfactory choice because such a large proportion of our information concerns first intervals. In the present work, we have taken a third tack—despite the hazards of the venture—of estimating for each cohort the length of premarital exposure to risk (for classes of women rather than for individuals). The estimates are plausible (and, in any event, of little numerical significance for the main purposes of the model); the procedure may be of interest.

The estimate proceeds from the following premises. From the date that respondents start to copulate, they are exposed to the risk of two competing events—conception and marriage. Suppose these risks are both homogeneous and fixed over time, at q' for the probability of conception and at q'' for the probability of marriage. Then the proportion of conceptions occurring before marriage (a piece of information we have) is $K = q'q''/(q'p'' + p'q'')$ (where $p = 1 - q$). The waiting time until the first event (either conception or marriage) occurs is $M = 1/(1 - p'p'')$. Now we assume that the waiting time to the first conception is the same as an observed waiting time to the second conception, say E. The value E has been calculated with an allowance of three months for puerperium, confined to nonlactators, and subdivided into users and nonusers of contraception in the second interval. Then $q' = 1/E$. That provides us with $M = E[1 + K(E - 2)]/(E - K)$. For those with an open first interval, the problem is to determine the waiting time until marriage, i.e., $1/q''$. Since $M = 1/(1 - p'p'')$ and $E = 1/q'$, we have $1/q'' = M(E - 1)/(E - M)$.

The assumptions required for this estimate are questionable in various ways. Consider first the assumption of homogeneity for each of the risks. Some proportion of respondents are chaste at marriage; in other words, they have a probability $q'' = 1.0$ of getting married simultaneously with starting to copulate. A plausible allowance for the proportion chaste would have the consequence of reducing the estimate of mean length of the premarital exposure by a small proportion. This is counterbalanced by the consideration that the nonchaste respondents are heterogeneous in fecundability; a plausible allowance for that heterogeneity would increase the estimate by a small proportion. Furthermore, it is apparent from the detailed records of waiting time to postmarital conception from the marriage date that the dates are frequently adjusted to conceal premarital conception; our estimate of K is accordingly too small. It is also probable that there is less induced abortion within the second than within the first interval; our estimate of E is accordingly too small. The consequence of these two underestimates is an underestimate of premarital exposure to risk.

It is important to note what we are trying to accomplish with this estimate. We are not attempting to achieve a date of defloration but rather an aggregate length of exposure of comparable density to exposure within the second interval. Thus, we avoid the measurement problem of a record of premarital exposure to risk which might contain sporadic episodes with long stretches of abstinence. The principal flaw in the estimate relates to intervals of use of contraception prior to the first conception; being unmarried is an important motive for continuing use, not relevant in the second interval. It is fortunate that only a minority of our respondents report use prior to the first conception.

The basic assumption concerning the copulation aspect of exposure is that marriage is sufficient (but not necessary). This assumption is belied by any period of separation of spouses, absent adultery. Now, our record for each pregnancy interval did determine months of separation from spouse, but the reports were implausibly rare. Therefore, we chose to ignore the information; the decision is of trivial empirical import.

Exposure calculations also require subtraction of the time subsequent to the onset of sterility. Some respondents reported the date at which an operation took place. If they reported that the operation was contraceptive in intent, then the respondent was classified as a user of that particular method. If they reported that it was noncontraceptive in intent, we subtracted from gross exposure the time subsequent to the date of that operation. But others reported sterility, not consequent upon an operation, and gave the date of onset. Preliminary analysis of reinterview results obtained in 1975 for a large subset of the respondents interviewed in 1970 suggests considerable unreliability in these reports; accordingly, we have not made use of this information. In any event, there is a progressive attrition of fecundability in the final stages of the life cycle, which escapes our measurement net altogether.

The only other subtraction from exposure to be incorporated in this model is the arbitrary assignment of one year of nonexposure corresponding to each fertile pregnancy, nine months preceding and three months following the date of birth. Although it is tempting to consider the possibility of a more generous allowance to accommodate nonexposure attributable to infertile pregnancies, we did not do so, largely because we were sensitive to the probably substantial problem with respect to induced abortions about which we have so little information for the era under examination.

In classifying net exposure by use, our practice is to assign the entire (open or closed) interval to that category if any use is reported in the interval in question, excepting only the case in which contraception is reported stopped (at a specified date) in order to conceive. This implies that our reports of the fertility of users of contraception do not distinguish between failures arising from accidents during use or from negligence in not always using.

More problematic than the distinction between use and nonuse is the classification of net exposure by intention. Our practice was to classify intention within each closed interval by the intention reported for the birth which ended the interval and within each open interval by the intention determined at interview. This approach suffers from the problem that intention may change over time. Whereas closed intervals may be sufficiently short to make the practice relatively innocuous, the same cannot be said for open intervals. Thus, after their final child (which they do not at the time know to be the final child), a couple may begin using contraception in order to delay the next birth but subsequently decide that they are too old for another baby. Another scenario would be a couple who stop use in the open interval and then find out they are unable to have any more children. Both of these intervals would, by our procedure, be classified in their entirety as termination intervals, the former because the couple reports at interview the intention to have no more, and the latter because we code all sterile couples as "intending" no more, rather than ask them the question.

The cases just mentioned are of some gravity for the interpretation of our results, not only because they are probably common but also because we rely, within each cross-sectional survey, on a comparison of successive cohorts to provide a sense of time series, and successive cohorts are, by definition, at different life-cycle stages at interview. Couples interviewed a short time after the birth of their last child are less likely to be classified as intending to terminate than they would be had they been interviewed a long time after the birth of their last child. In light of this consideration, we have incorporated in our 1975 study questions concerning intention at the beginning as well as at the end of each interval.

Another problem for our classification scheme became evident from the output of a preliminary version of the present model. Although it was our intention to produce fertility rates for post-termination exposure distinguished between use and nonuse (as with pre-termination exposure), we were dissuaded from that by the following finding: the proportion of exposure classified as nonuse declined from about 40 percent to about 10 percent over the time span of our evidence; the level of nonuse fertility rose from about 0.04 to about 0.12 per person-year over the same time. Now a level of nonuse of the order of 40 percent, subsequent to the last intended birth, is quite implausible, and so is a level of nonuse fertility as low as 0.04. Moreover, one would scarcely anticipate a rise in fecundability of anything like the magnitude suggested. Our inference is that there was a high level of covert fertility regulation at the beginning of our series and a low level at the end of our series: the rise in nonuse fertility is attributable to a decline in the proportion of reported nonusers who were actually users. In light of this finding, we decided to abandon the distinction between users and nonusers with respect to termination exposure (and unintended births). As explained subsequently, we have restricted the coverage of our data to experience within the first 15 to 20 years of marriage; one reason for so doing is to reduce the lengths of open interval included in the measurements.

To this point, we have been discussing the problems with information collected for a set of marriages which remain intact from date of first marriage to interview. The women interviewed in 1965 were currently married; those interviewed in 1970 were ever married. We considered restricting the records to intact first marriages but decided that it would be unfortunate to lose all of the records of those with a dissolved first marriage. We also considered including all available records and treating intermarital and postmarital episodes as nonexposure. Several considerations militated against this choice. In the first place, intermarital and postmarital episodes, like premarital time, have a substantial probability of being exposure rather than nonexposure. Secondly, when we collected information about intention and use within each pregnancy interval, we failed to discriminate—in cases in which a dissolution occurred within the time span of the pregnancy interval—between information appropriate to the first marriage segment and that appropriate to the second. Moreover, we decided that it would be indiscreet to ask a respondent unmarried at interview what had been the intentions of the couple prior to the dissolution of the marriage. Finally, we did not know how to resolve the quandary that arises with respect to classifying exposure by duration of marriage when married life is a series of episodes. Suppose a respondent is married for four years, unmarried for six years, and then remarried. Should the first year of her new marriage be treated as duration one, or duration five, or duration 11?

Our eventual decision was to include all experience within first marriage, whether or not the marriage dissolved. For reasons indicated in the preceding paragraph, this still left us with the problem of no classification of planning status for the open interval within a first marriage which dissolved. We were forced to exclude that experience from the exposure base; in consequence, as is well-known for estimates confined to closed intervals, our fertility measures are biased upward for those whose first marriage dissolved. Moreover, our model is not even-handed with respect to use of the 1965 and 1970 data: women with marital histories were excluded from the former study if they were not married at the time of the interview.

The decision to incorporate the first marriage experience of those whose first marriages dissolve means that each first marriage cohort experiences attrition over time. This is analogous to the attrition of birth cohorts over time, because of mortality, and the resolution of the difficulty, as in the age-specific fertility context, is to produce a series of duration-specific fertility measures and add them to give a picture of what fertility would have been in a marriage cohort not experiencing dissolution. This carries the implicit assumption that reproductive behavior prior to dissolution is not distinctive between those who do and those who do not subsequently experience dissolution (just as one assumes that there is no relationship between mortality and fertility in constructing a birth cohort's total fertitity rate). In the present application, the assumption is somewhat dubious because of the greater magnitude of dissolution than of mortality, and because the link of fertility with dissolution is probably closer than the link of fertility with mortality. Moreover, exposure is not distributed uniformly by duration within each segment but, rather, weighted toward the lower durations. Since fertility varies inversely with duration, the resultant estimate of fertility is a little too high.

DERIVATION OF INPUTS

In summary, we have calculated nine observations for each subset of respondents, as follows. We have a count of births (*B*) in each of five planning status classes. The first four of these are intended births:

BC = birth following nonuse because baby wanted as soon as possible,
BD = birth following nonuse for other reasons,
BE = birth following use which was stopped in order to conceive,
BF = birth despite use.

The fifth class of births is *BU* = unintended births.

We have also produced the number of years of gross exposure, divided into four classes.

1. *YE* = years of pre-termination nonuse. This has three components, that is, nonuse preceding *BC*, *BD*, and *BE* (and similarly for the open interval).

2. *YF* = years of pre-termination use. This has two components, that is, use preceding *BE* and *BF* (and similarly for the open interval). Note that, in the case of an (open or closed) interval in which use is stopped in order to conceive, there is a segment of successful use, followed by a segment of nonuse, prior to becoming pregnant or reaching the end of the record.

3. *YP* = years of premarital exposure, calculated as explained above.

4. *YS* = years of sterility—more precisely, the mean number of years of nonexposure subsequent to a sterilization for noncontraceptive reasons.

In this version of the model, the data refer to the experience of intact marriages in a marriage cohort observed up to duration 15 to 20 years (as explained in the following section). The gross exposure is, accordingly, $(YP + 17.5)$ years. Net exposure is obtained by subtracting from that exposure the time lost to sterilization (*YS*) and the one year of nonexposure associated with each birth. Since the total number of fertile pregnancies is $BT = BC + BD + BE + BF + BU$, we can derive, from the above observations, the number of years of exposure to risk of an unintended birth, $YU = (YP + 17.5) - YS - BT - (YE + YF)$.

These observations are used to calculate what we call input variables, viewed as the determinants of the distribution of exposure by categories and of the births, and what we call output variables, the typical focus of concern for policy purposes. We visualize the inputs as falling into three classes: ends, means, and conditions, following the model outlined above. The three categories of reproductive ends (*E*) are

1. number of births intended, $E1 = BC + BD + BE + BF,$

2. proportion of intended births not wanted as soon as possible,

$$E2 = (BD + BE + BF)/E1, \text{ and}$$

3. mean length of intended use prior to an intended birth,

$$E3 = \{c\ln[BE/(BE + BF)]\}/(BF/YF).$$

The derivation of the last formula requires discussion. We have tried to identify intentions with respect to the quantum of fertility (*E*1) and also the tempo of fertility. One aspect of the latter consists of that proportion of the cohort who intend a particular birth as soon as possible $(1-E2)$; in a sense, they have an intended use of length zero. The problem of estimation arises with those who use contraception to delay the next birth. We can observe the length of use for those who are successful in achieving their intended delay, but we neglected to ask those who failed how long they would have continued to use before stopping, had they not failed. With some simplifying assumptions, we can remedy that omission. The proportion who are successful in delay is $BE/(BE + BF)$; the fertility rate for that category of exposure is BF/YF. If the (unknown) average intended delay is $E3$, and one assumes homogeneity and constancy over time for the fertility rate, then the proportion successful $BE/(BE + BF) = \exp[-E3^*(BF/YF)]$. The indicated formula for $E3$ is implicit in this equation. The estimate is flawed not only by departures from the assumption of homogeneity of risk, but also by the likelihood that those with a shorter intended delay are more likely to be successful than those with a longer intended delay (whereas the equation assumes the same intention for all).

In the subset of inputs labeled means (M), we have three measures of fertility regulation.

1. Proportion of intended births, not wanted as soon as possible, which are preceded by use, $M1 = (BE + BF)/(BD + BE + BF)$. This measures the extent to which the intention to delay is followed by appropriate action.

2. Pre-termination use fertility, $M2 = BF/YF$. This is a failure rate, an inverse measure of efficacy of contraception for the purpose of delay.

3. Post-termination fertility, $M3 = BU/YU$. Our original intention was to subdivide post-termination exposure into use and nonuse, with fertility rates for each, but this was abandoned for reasons indicated above. In its present form, $M3$ reflects not only the propensity to use contraception to terminate reproduction, and the efficacy of that use, but also the fecundability of nonusers; the last would more properly be regarded as a reproductive condition. On the other hand, our interest is primarily in the question of changes over time, and it is our judgment that variations in $M3$ are predominantly dependent on changes in regulatory behavior rather than on changes in fecundability.

The final subset of reproductive inputs is labeled conditions (C). These are thought of as the context within which the means/end schema develops.

1. Mean years of premarital exposure, $C1 = YP$.

2. Pre-termination nonuse fertility, $C2 = (BC + BD + BE)/YR$. Three different kinds of exposure are combined in this measure: the segment of nonuse following successful use to delay, and the intervals of nonuse for those who did, or did not, want a baby as soon as possible. In a preliminary version of the model, we produced fertility rates for all three types of pre-termination nonuse exposure and determined that the rate for nonuse exposure following successful use was several times as high as that for the other two categories. We dismissed the speculation that this might be a consequence of those of high fecundability selecting themselves for use and those of low fecundability selecting themselves for nonuse, because there is little physiological information available to the respondents in the pre-termination period, and the lengths of preceding intervals are a poor guide to fecundability because of the stochastic nature of conception (Westoff and Ryder, 1977, pp. 224–225). In our judgment, this result suggests that some respondents used contraception and had an accident, then misreported that they had stopped using in order to conceive, and became pregnant right away.

Those reporting no use were divided into those whose only reason for nonuse was that they wanted a baby as soon as possible, and the rest. Although there would seem to be no connection between intention and outcome, it was determined that the latter had about one-third higher fertility than the former. We surmise that, in some cases, the statement of intention is posterior rather than prior to the outcome, i.e., those with a longer waiting time are more likely than those with a shorter waiting time to say in retrospect that they had wanted a baby as soon as possible. Moreover, the 1965 questionnaire does not permit us to distinguish between these two kinds of nonuser for the open interval. For these reasons, we have chosen to collapse all of the pre-termination nonuse exposure into a single category.

The measure of pre-termination nonuse fertility, $C2$, is the third fertility rate in our set of inputs; the others are $M2$ and $M3$. All of these have the form of central rates, like the Pearl index, and are accordingly sensitive to the distribution of exposure by interval length. For an individual, the probability of conception may change as the interval lengthens; for an aggregate it certainly does, provided there is heterogeneity at the outset. For the measures of pre-termination fertility (M and $C2$), this is probably of small consequence, but it may be a source of substantial bias in post-termination fertility, $M3$, because open intervals may be quite long.

The final reproductive condition is mean years of sterility per woman, $C3 = YS$. Although this would seem to be a straightforward classification, preliminary results of reinterview analysis have indicated that there is inconsistency of reporting the intent (contraceptive or not) of a sterilization. It is fortunate, then, from the standpoint of measurement, that this is a very small component of the system of reproductive variables to be examined.

In addition to the above set of nine input variables, we can also use our observations to produce various kinds of output. For present purposes, we focus solely on two of these, the mean number of births per woman over the course of a reproductive lifetime, BT, and its unintended component, BU.

TIME SERIES CONSIDERATIONS

If one is interested in examining a time series of parameters which represent experience over the reproductive life span, one has two options for the mode of temporal aggregation: real cohorts or synthetic cohorts (periods). The causal model which would lead to a choice of the former would place emphasis on the continuity of reproductive experience, the influence the past has on the future for an individual, and the integrity of the total history. The causal model which would lead to choice of the latter would put emphasis on the responsiveness of fertility and its determinants to changes which affect all cohorts more or less the same way in the same period of time. The real cohort choice has the disadvantage that a complete summary is unavailable until the cohort has reached an advanced age; the synthetic cohort choice has the disadvantage that movements of the indices of the quantum of fertility are ambiguously associated with changes in the quantum and tempo of real cohort fertility. With these considerations in mind, we have chosen to produce both kinds of series.

An important decision concerns the kind of cohort to be used. All of the cross-sectional American fertility surveys—the Growth of American Families Studies of 1955 and 1960, the National Fertility Studies of 1965 and 1970, and the National Studies of Family Growth of 1973 and 1976—have been based on a sample frame of birth cohorts (of currently married or ever-married women). Our longitudinal 1975 study, on the contrary, is oriented to marriage cohorts. The initial choice of a birth cohort orientation was probably made because American birth statistics are specified in terms of the age of the mother rather than her duration of marriage; certainly P. K. Whelpton saw the 1955 and 1960 studies explicitly as a vehicle for extrapolating the (birth) cohort fertility tables.

We favor the marriage cohort orientation primarily because so much of the experience collected in our cross-sectional surveys comes from women who have been married a relatively short time; for them, fertility is much more closely related to their duration of marriage than to their age. That consideration is of particular importance if one wants to produce a meaningful measure of fertility over the reproductive life span for a synthetic cohort in a recent period. If one were to attempt to merge in a single series the experience of ever-married women in each successive age, beginning with the youngest, one would be cumulating an excessive amount of low-duration (and thus high) fertility, since each successively higher age would contain new entrants who had just become eligible for the sample by getting married. No such difficulty arises with a synthetic marriage cohort construction, since each duration is represented by the marriages of an appropriate number of years prior to interview. Since we want to produce both a real and a synthetic cohort time series, this consideration leads to the choice of marriage cohorts.

We have discussed elsewhere the ways in which the decision to frame the universe definition in terms of birth cohorts (or age groups) of married women creates a censoring problem with respect to the available distribution of each cohort by age at (first) marriage (Ryder, 1975). If the earliest time of birth included in a survey is t, then those married d years before interview must have been married at less than age $(d-t)$. This implies a variable limit on age at marriage from one marriage cohort to another. Since age at marriage is known to be a potent influence on reproductive behavior, comparability requires some procedure to ensure that the same feasible range of marriage age be used for each cohort. In effect, one is forced to compromise among three desiderata: length of time series, coverage of the reproductive span, and upper limit on the range of age at marriage. Achievement of the first two goals, by encompassing respondents married a long time before interview, prejudices the third requirement, since the longer the span of duration of marriage covered, the more reduced is the upper age at marriage limit.

We decided to produce indices covering experience in the first 15 to 20 years following the date of (first) marriage. (The duration limit has a diagonal form because we are working with quinquennial marriage cohorts, observed up to a fixed date.) In the 1965 study, the cohorts which had completed at least 15 to 20 years of marriage by the beginning of 1965 were those married in 1925–1929, 1930–1934, 1935–1939, 1940–1944, 1945–1949. Since the earliest date of birth included was 1910.5, the implied limits on marriage age for these five cohorts were 14.5 to 19.5, 19.5 to 24.5, 24.5 to 29.5, 29.5 to 34.5, and 34.5 to 39.5, respectively. We felt obliged to eliminate the first two cohorts, not only because of limited coverage of the range of marriage age, but also because we only had a half-sample of women born between July 1910 and June 1920, inclusive. For the remaining three cohorts, one achieves comparability of the limit on marriage age by excluding from the 1940–1944 cohort those born before 1915.5, and from the 1945–1949 cohort those born before 1920.5.

Since the earliest time of birth included in the 1970 study was 1925.5, the marriage cohorts of at least 15 to 20 years duration by the beginning of 1970 were 1940–1944, 1945–1949, and 1950–1954, with implied age at marriage limits of 14.5 to 19.5, 19.5 to 24.5, and 24.5 to 29.5. The 1940–1944 cohort is excluded because of inadequate coverage of the age at marriage range. Rather than exclude the 1945–1949 cohort on the same grounds, we decided to devise a procedure for estimating what its observations would have been had we interviewed women born between 1920.5 and 1925.5. This decision was based on the desirability of making feasible a comparison of the change from the 1945–1949 to the 1950–1954 cohort with data from the 1970 study for both, rather than using 1965 data for the earlier and 1970 data for the later cohort (and thus begging the question of interstudy comparability). The assumption underlying these estimates was that the proportional change in each observation, for the 1945–1949 cohort, consequent upon raising the limit on age at marriage, would be the same as the comparable change for the 1950–1954 cohort (for which both options are available in the data set).

Precisely analogous considerations were involved in the production of a series for synthetic marriage cohorts. From the 1965 study, we have produced observations up to duration 15 to 20 years, with age at marriage limit 24.5 to 29.5, for periods 1950–1954, 1955–1959, and 1960–1964 and, from the 1970 study, for periods 1960–1964 and 1965–1969, using an estimation procedure to raise the age at marriage limit for the period 1960–1964 from the 1970 study. We tested the legitimacy of the estimating procedure by considering the stability of such proportional changes over the eight (real and synthetic) cohorts for which both the lower and higher limit on age at marriage were feasible. The legitimacy of the assumption underlying our estimation procedure depends not on the changes consequent upon an extension of the age at marriage range (which were appreciable) but on whether the changes are similar from cohort to cohort. For these eight cohorts, the procedure would have resulted in an average error over all parameters of 3.3 percent. This gives us confidence in our estimating procedure.

The above steps yield 1965 data for the real marriage cohorts of 1935–1939, 1940–1944, and 1945–1949, and 1970 data for the real marriage cohorts of 1945–1949 and 1950–1954; likewise, they yield 1965 data for the synthetic marriage cohorts of 1950–1954, 1955–1959, and 1960–1964, and 1970 data for the synthetic marriage cohorts of 1960–1964 and 1965–1969. The final question in terms of data presentation is the optimal way of merging the results of the two surveys: that is essentially a question of comparing the 1965 and 1970 results for the real marriage cohort of 1945–1949 and for the synthetic marriage cohort of 1960–1964. We anticipated differences arising from two sources—noncomparability of samples and noncomparability of questionnaires and coding procedures. With respect to the first of these, the surveys were conducted by two different organizations, National Analysts, in the first case, and the Institute for Survey Research, Temple University, in the second. The 1970 survey had a substantially lower yield than the 1965 survey and, when compared in composition with other presumably more reliable sources of the same information, appeared to be more subject to bias; for example, the 1970 survey had an inadequate representation of respondents from central cities.

In our judgment, this is a much less important source of noncomparability than differences in questionnaires and coding procedures. In order to investigate the latter, we focused on the sequence of questions for each interpregnancy interval, comparing the responses at each routing point, for the same cohort as interviewed in 1965 and in 1970. The first step in the sequence was to dichotomize respondents between users and nonusers. For the real marriage cohort of 1945–1949, the 1965 survey showed 62 percent using, whereas the 1970 survey showed 50 percent using; for the synthetic marriage cohort of 1960–1964, the 1965 survey showed 67 percent using, whereas the 1970 survey showed 53 percent using. This is a substantial and important discrepancy which we examine below. The second step in the sequence is to ask the users what proportion of them stopped using in order to conceive; here the proportions from the two surveys are 44 percent and 42 percent for the real cohort, and 35 percent and 40 percent for the synthetic cohort, an undisturbing outcome. The third step in the squence is to ask the nonusers what proportion of them were not using because they wanted a baby as soon as possible; the proportions from the two surveys are 58 percent and 56 percent for the real cohort, and 58 percent and 51 percent for the synthetic cohort. We return to this small discrepancy below. The final step in the sequence is to determine what proportion of the "failures" (those who answered the second or third questions in the negative) did not want to have the baby at any time in the future. The proportions from the two surveys are 34 percent and 29 percent for the real cohort, and 35 percent and 33 percent for the synthetic cohort. The salience of this item justifies consideration of its possible bias as well.

The large relative decline from 1965 to 1970 in the proportion reporting use in closed intervals is probably attributable to defects in the 1970 questionnaire. The discrepancy provides eloquent testimony to the sensitivity of observations to nuances of the interviewing procedure. In the 1965 study, we presented the respondent with a comprehensive list of methods on a card and then asked "Which method or methods, if any, did you or your husband use?" This procedure may be faulted because it asks two questions in one, and because it implies that the typical response is that the respondent is using, so that the essential question is what method. That was a deliberate strategy on our part, based on the assumption that some such pressure would help to counterbalance an anticipated under-reporting of use. In the 1970 study, on the other hand, we followed the more straightforward procedure of asking the respondent, "Was any method of family planning used?" Only if that response was affirmative was the respondent shown the methods card and asked which method. The problem with that approach is that the respondent is required to provide her own definition of what is meant by use of a method of family planning in answering the first question, since the card is not shown her until she reports use. Now there had been some questions about methods of family planning, prior to this section of the questionnaire, but they were focused on methods obtained through a doctor or family planning clinic. This raises the suspicion that methods such as rhythm, withdrawal, and abstinence may be considered by the respondent to be excluded from our definition. Moreover, we have some relevant evidence from another quesion asked in 1965 (Ryder and Westoff, 1971, p. 9). When asked their attitude toward "doing something to limit the number of pregnancies they will have," 92 percent of Protestants but only 70 percent of Catholics were in favor, but when asked their attitude toward "using a natural method, rhythm or safe period," an additional 4 percent of Protestants and 23 percent of Catholics were in favor. When we investigated the religion-specific data on use, as reported in 1965 and 1970, we found that Catholics were a bigger source of discrepancy in use than non-Catholics. Moreover, in response to the question addressed to nonusers whether their reason for nonuse was that they wanted a baby as soon as possible, the proportion of Catholic nonusers was more than 50 percent higher in the negative category in 1970 than in 1965 (although essentially the same for non-Catholic nonusers). That is precisely the result one would expect if those using rhythm were denying use of a method of family planning (but obviously using rhythm so that they would not have a baby as soon as possible). This finding also helps to explain the decline from 1965 to 1970 in the proportions wanting as soon as possible, among nonusers. In our judgment, the source of noncomparability in reported use between 1965 and 1970 is an underreporting of use in 1970, because the respondent used a narrower definition of use than we intended. Preliminary inspection of reinterview data in 1975 supports this conclusion; there is substantially more use reported in 1975, by the same respondent talking about the same pregnancy, than in her first interview in 1970; the 1975 questionnaire provided a careful comprehensive definition of what we meant by use of a method before the question was asked.

Although the differences between the proportion of failures reported as unintended births in 1965 and 1970 were much smaller relatively than the differences in the proportion using, they deserve attention because of the importance of the information. One source of decline from 1965 to 1970 is the circumstance that interviewing practice in 1970 produced a substantial proportion of respondents (one in three) whose answers were coded as noncommittal, in contrast to the 1965 practice (in which the proportion was one in 20). In order to code the 1970 noncommittal responses as either intended or unintended (that is, delay-failure or termination-failure), we were obliged to resort to a comparison of the order of that birth with the answer to a retrospective question: "Given the circumstances of your life, how many children *in all* would you really consider the most desirable for you and your husband?" This was employed, faute de mieux, despite our expectation that it would bias downward the report of unintended fertility. Our preliminary examination of reinterview data verifies that expectation: noncommittal responses in 1970 were much more likely to be classified as unintended, in the 1975 questionnaire, than by the procedure adopted for such births in 1970.

Since our investigation led to the conclusion, in two significant respects, that the discrepancy between results in 1965 and results in 1970 for the same cohorts was attributable to problems with the questionnaire and coding procedure in 1970, we have chosen, in producing a single series across the four real and four synthetic cohorts, to align the 1970 results with the 1965 results, rather than the other way around. Thus, the level of the indices to be reported below accords with the 1965 findings. Nevertheless, the change from the penultimate to the last cohort, real and synthetic, depends on a comparison internal to the 1970 study and is, thus, independent of whatever residual noncomparability there may be between the 1965 and the 1970 studies.

INDICES OF REPRODUCTIVE OUTPUT AND INPUT

The results of the application of the procedure just described are presented in Tables 1–11. Before discussing these results, an explanation is required for the particular temporal alignments of real marriage cohort and synthetic marriage cohort indices. The experience of the former is distributed over the reproductive life cycle and cannot, therefore, be precisely dated like the latter. The practice adopted in these tables is to locate the values for the real marriage cohort at the approximate average duration of marriage of the experience summarized. For pre-termination indices (such as intended births), the mean duration is between five and ten years, and similarly for all births; for post-termination indices (such as unintended births), the mean duration is between ten and 15 years, and similarly for mean years of sterility. Mean years of premarital exposure are dated at duration 0. However, in each time series, the percent changes are shown separately for real and synthetic marriage cohorts, because of the distinctive characteristics of these two modes of temporal aggregation.

In Table 1, we show total births per woman, for real and synthetic marriage cohorts (up to duration 15 to 20 years). In

Table 1.—Total Births per Woman for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

	Total Births		Percent Change	
Years	RMC	SMC	RMC	SMC
1940–1944				
	2.94			
1945–1949			+ 4	
	3.04			
1950–1954		3.40	+ 8	
	3.29			+ 8
1955–1959		3.67	+ 1	
	3.31			+ 0
1960–1964		3.68		
				−30
1965–1969		2.56		

Tables 2 and 3, we show the division of that total into its unintended and intended components. There is a moderate rise in the total across the real cohort time span; in the synthetic cohort series—lagged behind that for real cohorts—an early rise becomes transformed in the final quinquennium into an abrupt decline. An examination of the component tables reveals that relative changes in intended fertility have been much smaller than those in unintended fertility and became negative while the latter were still positive. Except for the final quinquennium, absolute changes in intended births per woman have also been smaller than those in unintended births, by and large. Indeed, it would require no exaggeration to assert that the level of intended fertility remained constant throughout the baby boom. Only a small disposition to misreport unintended births as intended would be needed to erase the slight tendency upward in the early part of the real marriage cohort series shown in Table 3 (Ryder, 1976a).

Accurate interpretation of the synthetic marriage cohort columns in these tables requires appreciation of the way in which those values manifest changes in the time pattern of real cohort childbearing as well as its level. In Table 1, note how much higher are the values for the synthetic cohorts than for their temporal counterparts in the real cohort column. That upward displacement was caused by a concomitant drift of real cohort fertility toward the younger ages. We can see the same kind of result in official vital statistics for birth cohorts. Whereas the total fertility rate on a period basis reached 3.68 in 1957, the peak for real cohorts was only 3.22 (for the birth cohort of 1933) (Ryder, 1978). Likewise, there is downward distortion of synthetic cohort values when real cohort fertility is being displaced toward the older ages. Accordingly, a substantial part of the 30 percent decline in the synthetic total, from 1960–1964 to 1965–1969 (in Table 1), may be attributable to a change in the direction of movement of the time pattern of fertility, a change which would manifest itself in a transition from positive to negative distortion, with a consequent slump in period fertility.

Table 2.—Unintended Births per Woman for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Unintended Births RMC	Unintended Births SMC	Percent Change RMC	Percent Change SMC
1945–1949				
	0.34			
1950–1954		0.46	+20	
	0.41			+24
1955–1959		0.57	+32	
	0.54			+20
1960–1964		0.69	+20	
	0.64			−38
1965–1969		0.43		

Table 3.—Intended Births per Woman for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Intended Births RMC	Intended Births SMC	Percent Change RMC	Percent Change SMC
1940–1944				
	2.60			
1945–1949			+ 2	
	2.65			
1950–1954		2.93	+ 4	
	2.75			+ 6
1955–1959		3.10	− 3	
	2.66			− 4
1960–1964		2.99		
				−29
1965–1969		2.13		

The same problem of interpretation arises with respect to the synthetic cohort series in Table 3. To put this important point in another way, the paucity of intended births in 1965–1969 may reflect not only a reduction in the number of births intended by women but also a disposition to postpone some fraction of the intended births into the next quinquennium. That this was indeed occurring is documented by evidence to be presented below. As a footnote, the phenomenon of distortion of the synthetic cohort series is essentially irrelevant to interpretation of the data for unintended births per woman, in Table 2.

In terms of our model of fertility by planning status, Tables 1 and 2 are output tables: they show the consequences of variations in the set of nine input variables, the first of which, *E*1, has been displayed in Table 3. What we have observed about the determinants of postwar fertility, to this point, is virtually no change in the intended quantum of fertility, except for the final quinquennium. That exception is partly spurious: the synthetic cohort construction is inherently unsatisfactory for this assignment because it is highly responsive to change in the intended tempo of fertility.

Tables 4 and 5 display the other two intention inputs (*E*2 and *E*3). These represent the temporal counterpart of the quantum intention (*E*1) shown in Table 3. The proportion of intended births not wanted as soon as possible (*E*2) is a measure of intended tempo in the sense that, for its complement (those who want their next birth as soon as possible), the length of intended use of contraception to delay a birth (*E*3) is zero. Whether one considers the real or the synthetic series, this proportion has risen throughout the entire postwar era. (These synthetic cohort results are not subject to distributional distortion.)

The more important indicator of the intended tempo of fertility is the length of

Table 4.—Proportion of Intended Births Not Wanted As Soon As Possible, for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Proportion of Intended Births Not Wanted as Soon as Possible		Percent Change	
	RMC	SMC	RMC	SMC
1940–1944				
	0.639			
1945–1949			+13	
	0.722			
1950–1954		0.725	+ 4	
	0.754			+ 4
1955–1959		0.756	+ 2	
	0.766			+ 2
1960–1964		0.774		
				+ 5
1965–1969		0.812		

Table 5.—Mean Length of Intended Use to Delay (in Years) for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Time Period	Intended Delay		Percent Change	
	RMC	SMC	RMC	SMC
1940–1944				
	3.41			
1945–1949			-11	
	3.02			
1950–1954		2.69	-11	
	2.69			- 7
1955–1959		2.49	-10	
	2.45			+ 9
1960–1964		2.73		
				+15
1965–1969		3.13		

delay planned for each intended birth; the estimates for this are presented in Table 5. Across the real cohort span, there is a considerable decline in this interval by almost one year overall. This is the principal source of the upward distortion of synthetic relative to real cohort values, as observed in Table 3. We can bring the account closer to the present with the synthetic cohort series in Table 5. The recent marked reversal of trend is strong evidence for our interpretation, with respect to the results of Table 3, that the abrupt decline in intended births from 1960–1964 to 1965–1969 was, in part, a postponement phenomenon. Definitive documentation of its magnitude must await the extension of the real cohort series, since synthetic cohort indices of tempo are also subject to distortion (Ryder, 1964).

Table 6 contains the first of the measures designated as reproductive means (M1), the proportion of births intended only after a delay which are, in fact, preceded by use of contraception. It is appropriate to include this in the context of discussion of the measures of intended reproductive tempo, since one's intentions in that regard go for naught if they are unaccompanied by the appropriate action. The measure changes little throughout the series, until the most recent decade. Note, however, that the small magnitudes of percentage increase re-

Table 6.—Of Births Intended Only After a Delay, the Proportion Preceded by Use of Contraception, for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Proportion of Births Intended Only After Delay Preceded by Use of Contraception		Percent Change	
	RMC	SMC	RMC	SMC
1940–1944				
	0.847			
1945–1949			- 1	
	0.842			
1950–1954		0.843	- 0	
	0.841			- 0
1955–1959		0.840	- 0	
	0.839			+ 2
1960–1964		0.860		
				+ 6
1965–1969		0.910		

cently are reflective of the way the calculation is made. More appropriate in the circumstances would be the statement that the proportion not using contraception to achieve an intended delay has been reduced from 16 percent in 1955–1959 to 9 percent in 1965–1969, a most appreciable relative attrition in nonuse.

The product of the entries in Tables 4, 5, and 6 constitute an overall measure of intended delay. The product of the value in Table 4 and that in Table 6, for any cohort, is the proportion using a method in order to delay—and the value in Table 5 specifies the intended length of that use. Over the real marriage cohort span, this overall measure declines by 15 percent (from 1.85 to 1.57); over the last decade of the synthetic marriage cohort series, the same measure rises by 46 percent (from 1.58 to 2.31). This is a reasonably good index of the extent of deceleration of tempo during the 1960s, i.e., the kind of change which produces downward distortion of intended synthetic marriage cohort fertility.

Tables 7, 8, and 9 constitute the three fertility rates included in the input indices. The value in Table 7 is *M*2, the pre-termination fertility rate for users, in other words, the rate of accidental conception for those using to delay. The value in Table 8 is *M*3, the post-termination fertility rate; for reasons described in the preceding text, this is regarded as a measure of the changing extent and effectiveness of fertility regulation (on the assumption that there is little variation in post-termination fecundability). The value in Table 9 is the pre-termination fertility rate for non-users (*C*2), labeled a reproductive condition, because, in an ideal measurement context, this would be an indicator of fecundability and, as such, unlikely to change appreciably over time. What we see in Table 9, on the contrary, is a rather substantial amount of variation. Moreover, the changes are broadly similar in configuration to those registered for fertility despite use, in Tables 7 and 8. This raises the suspicion that what we see in Table 9 is, at least in part, a measurement flawed by the inclusion of a substantial number of unreported users.

Tables 7 and 8—and, if the preceding interpretation is accepted, Table 9 as well—provide strong evidence for an appreciable decline, in the first two decades after the war, in the effectiveness with which fertility was regulated, whether for delay or for termination. It would not seem unfair to characterize the consensus of demographic opinion about the baby boom as an era of rise in the number of children wanted without much change in

Table 7.—Pre-termination Fertility Rate for Users of Contraception to Delay Births, for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Pre-termination Failure Rate		Percent Change	
	RMC	SMC	RMC	SMC
1940–1944				
	0.205			
1945–1949			- 9	
	0.186			
1950–1954		0.217	+15	
	0.215			+17
1955–1959		0.254	+10	
	0.237			+ 7
1960–1964		0.273		
				-48
1965–1969		0.141		

Table 8.—Post-termination Fertility Rate for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Post-termination Fertility Rate RMC	SMC	Percent Change RMC	SMC
1945–1949				
	0.044			
1950–1954		0.062	+19	
	0.052			+22
1955–1959		0.075	+32	
	0.069			+20
1960–1964		0.090	+10	
	0.076			−45
1965–1969		0.050		

the practice of fertility regulation. For all their flaws, the data we present here are the best evidence there is about that period; they show that the baby boom was an era of decline in the practice of fertility regulation without much change in the number of children wanted.

The recent improvement in the practice of fertility regulation has received much attention in various of our previous publications. We are particularly interested in the estimate (from Table 8) that post-termination fertility dropped from 90 to 50 per thousand between 1960–1964 and 1965–1969. In a previous publication, we had reported rates of 55 and 35 per thousand, for 1961–1965 and 1966–1970 (Ryder and Westoff, 1972). The earlier estimates juxtaposed data from the 1965 study with data from the 1970 study; because we were apprehensive about possible noncomparability between the surveys, we adopted procedures which would avoid several sources of noncomparability, but at the expense of producing estimates which we judged would be on the low side for both periods. It is gratifying to see in the present findings—for which the methodology is superior in several respects to that used earlier—not only that the surmise was correct that the earlier estimates were low but also that the relative change from the early to the late 1960s as reported before was approximately correct, and not merely an artifact of uncontrolled differences between the two surveys. (The relative decline from 90 to 50 per thousand shown in Table 8 is based exclusively on data from the 1970 study.)

Tables 10 and 11 present the two amendments to the length of exposure, the former an addition at the beginning for estimated premarital exposure, and the latter a subtraction at the end for post-operative sterility. Except for a small decline in the first phase of the experience, there is a strong rise in premarital exposure, to a level of almost one year per couple in the late 1960s. Although we have many reservations about the quality of this estimate, our judgment, as expressed previously, is that this is an underestimate of the extent of premarital exposure.

Table 11 is a dubious set of results. On face value, it would suggest either a steady decline in the incidence of noncontraceptive operations (or delay in their occurrence, since that would have the same effect) until the most recent quinquennium. This measure is based not only on a behavioral report (whether or not an operation occurred) but also on an attitudinal report (whether or not the intention was contraceptive), and the latter is evidently subject to bias. It is fortunate for the present exercise that none of the values is large

Table 9.—Pre-termination Fertility Rate for Nonusers of Contraception for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Years	Pre-termination Fertility Rate Rate for Nonusers of Contraception RMC	SMC	Percent Change RMC	SMC
1940–1944				
	0.599			
1945–1949			+24	
	0.740			
1950–1954		0.778	+ 0	
	0.743			+ 6
1955–1959		0.827	+ 6	
	0.790			− 4
1960–1964		0.794		
				−26
1965–1969		0.586		

Table 10.—Mean Number of Years of Premarital Exposure for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Time Period	Mean Years of Premarital Exposure RMC	Mean Years of Premarital Exposure SMC	Percent Change RMC	Percent Change SMC
1935-1939	0.45			
			-18	
1940-1944	0.37			
			+21	
1945-1949	0.45			
			+12	
1950-1954	0.50	0.48		
				+34
1955-1959		0.65		
				+19
1960-1964		0.77		
				+21
1965-1969		0.93		

enough to influence other parameters much.

In order to achieve a reasonably lengthy time series, with a reasonably adequate coverage of the range of ages at marriage, we were obliged to curtail the coverage of duration of marriage to the experience prior to duration 15 to 20 years. Before turning to the subject of the next section, we report the kinds of amendment which would be introduced in Tables 1-11 had we been more successful in coverage of the entire reproductive span. For two real marriage cohorts (1935–1939 and 1940–1944) and for two synthetic marriage cohorts (1955–1959 and 1960–1964), the data sets from our two studies permit the calculation of the array of input and output indices for duration limit 20 to 25 years as well as 15 to 20 years. The significant changes produced by that extension of the reproductive span are as follows. Mean parity is increased, on the average, by about 4 percent, as a consequence of a 3 percent increase in intended births and a 9 percent increase in unintended births. (One-third of the births occurring between duration 15 to 20 years and duration 20 to 25 years are classified as unintended.) Thus, the principal amendment required in the above analysis by more comprehensive coverage of the reproductive life cycle would be an augmentation of the relative role of unintended relative to intended births. Only two other indices are affected by more than a trivial percentage. As would be expected, the mean years of sterility double in size (from approximately 0.7 to approximately 1.4). The rate of unintended fertility declines by approximately 28 percent. The inference from that change, in conjunction with the previous observation of increase in the numerator of the rate by 9 percent, is that there is a substantial inflation of the denominator (the amount of exposure to risk), again as one would expect from an enlargement of the upper duration limit from 15 to 20 to 20 to 25 years.

STRUCTURE OF CHANGE IN FERTILITY

In the preceding section, we presented two sets of indices derivative from observations made on each of four real and four synthetic marriage cohorts. Tables 1 and 2 displayed the two outputs (mean number of births and of unintended births), and the remaining tables displayed the nine inputs (three ends, three means, and three conditions). In this section, we utilize the algebraic dependence of each output on the nine inputs to characterize the structure of fertility, and the

Table 11.—Years of Sterility Following an Operation Not Intended for Contraception for Real (RMC) and Synthetic (SMC) Marriage Cohorts, and Percent Change per Quinquennium: United States

Time Period	Years of Sterility Following an Operation Not Intended for Contraception RMC	Years of Sterility Following an Operation Not Intended for Contraception SMC	Percent Change RMC	Percent Change SMC
1945-1949				
	0.75			
1950-1954		0.73	- 9	
	0.68			-14
1955-1959		0.63	- 3	
	0.66			- 7
1960-1964		0.59	- 2	
	0.65			+ 5
1965-1969		0.62		

components of its change during the postwar era. Because the outputs and the inputs are derivative from the same set of observations, it is possible to express each output as a function of the nine inputs. The development of these expressions is presented in the appendix.

This algebraic formulation permits us to display the structure of relationships between an output variable and the set of input variables by using the device of a coefficient of elasticity (also called a sensitivity index). That is calculated by dividing the partial derivative of y (the output) with respect to x (the input) by the ratio of y to x. The result shows the percent change in the output as a consequence of a 1 percent change in the input, ceteris paribus. We have calculated such measures for the four real and four synthetic cohorts represented in Tables 1–11. These measures differ little among the eight cohorts. (The coefficients of variation, with one exception, are approximately 10 percent. That one exception is $C1$, the mean years of premarital exposure, which is not only the index with the lowest elasticity, but also the one which has shown a tendency to bulk larger with the passage of time.) Accordingly, we have chosen merely to report the mean values of each over the eight cohorts, for total births and for unintended births, in Table 12.

From the "total births" column of Table 12, we see that, although the total number of births per woman is affected by changes in each of the nine inputs, only two have any substantial strength of relationship—the number of intended births ($E1$), and the rate of post-termination (unintended) fertility ($M3$). To exemplify the meaning of these elasticities, if the number of intended births were reduced by 25 percent, the elasticity for $E1$ indicates that the total number of births would (in the absence of any other changes) be reduced by 17 percent. Similarly, if the rate of unintended fertility were cut in half, that would reduce total births per woman by approximately 7 percent. All of the other inputs have their effect on total births through their consequences for the length of exposure to risk of unintended fertility. Accordingly, we turn to a discussion of the "unintended births" column of Table 12.

The number of unintended births is affected appreciably by changes in all of the input variables except for the two conditions ($C1$, years of premarital exposure, and $C2$, years subsequent to sterilization). Except for the rate of unintended fertility itself, $M3$, each input has an effect on the number of unintended births per woman through its influence on the number of years of exposure to risk of an unintended birth. Thus, the three measures character-

Table 12.—Coefficients of Elasticity of Total Births and Unintended Births with Respect to Each of the Nine Input Variables, Mean Values for Eight Cohorts: United States

Input Variable[a]	Total Births	Unintended Births
$E1$	+0.676	-1.081
$E2$	-0.048	-0.306
$E3$	-0.035	-0.224
$M1$	-0.048	-0.306
$M2$	+0.033	+0.210
$M3$	+0.147	+0.939
$C1$	+0.011	+0.068
$C2$	+0.049	+0.320
$C3$	-0.012	-0.079

a - $E1$: Intended births.
$E2$: Proportion of intended births not wanted as soon as possible.
$E3$: Mean length of intended use to delay birth.
$M1$: Proportion of births intended only after a delay, which are preceded by use of contraception.
$M2$: Pre-termination fertility rate for users of contraception.
$M3$: Post-termination fertility rate.
$C1$: Years of premarital exposure.
$C2$: Pre-termination fertility rate for nonusers of contraception.
$C3$: Years of sterility following an operation not intended for contraception.

ized as constituents of the tempo of intended fertility ($E2$, $E3$, and $M1$) are negatively related to unintended births, whereas the rates of pre-termination use and nonuse fertility, $M2$ and $C1$, are positively related (since higher fertility implies shorter intervals and, therefore, arrival sooner at the time of the last intended birth).

Particularly dramatic is the relationship between the number of intended births and the number of unintended births. A decline of 30 percent in the number of intended births would, all other things being equal, yield a rise in unintended births of approximately one-third, by virtue of the extension of the time of exposure to risk of an unintended birth. Of course, the rate of post-termination fertility is of substantial importance, but changes in it can readily be frustrated by counterbalancing changes in the inputs which determine the tempo of reproduction.

The structure of elasticities can be regarded from a policy standpoint as an indicator of the kinds of change in the array of input variables which would be required to achieve a particular goal with respect to total births per woman. One warning concerning such an application is that we are displaying an arithmetical, rather than an analytic, result. Presumably the inputs are not, in fact, independent of one another, at least in the sense that changes in underlying variables would be likely to affect more than one of them simultaneously. For example, the variables which are the different components of tempo intentions are probably responsive to the same stimuli. Improvements in contraceptive efficacy would be expected to be manifested in both $M2$ and $M3$.

Although the elasticity structures in Table 12 have remained relatively invariant at the indicated values during the postwar era in the United States, it is doubtful that a similar structure would prevail into the indefinite future in this country, or that a comparable structure would be found in another cultural context. Moreover, if one is interested in the movements of the birth rate (and, thus, of its more sophisticated partner, the period total fertility rate), the discussion would need to be enlarged beyond the present framework to include not only nuptiality, on which the present observations are moot, but also the changing tempo of fertility, a subject within the purview of models like that presented here but not, in fact, incorporated in the present version of the model. Most policy orientation properly focuses on period indices of fertility, which respond to changes in the tempo as well as the quantum of cohort fertility.

The elasticities displayed in Table 12 have a deceptive quality to them in one important regard. There is no clue to the relative flexibility of each of the inputs. An input may have a high elasticity with respect, for example, to unintended births but be of little policy importance because it is highly unresponsive to modification. This thought leads to the final demonstration in this paper, an assessment of the contribution of change in each input to the changes in the two output indices over the course of postwar American fertility. The basis for this assessment is a weighting of the elasticity of each input, with respect to the output variable in question, by the proportional change in that input. Thus, for each input i, we have calculated the proportional change from an earlier to a (five-year) later cohort, say $d(i)$, and also the average of the elasticities for that input in the two cohorts, say $e(i)$. In order to convert these to additive form, to set the stage for calculating the relative weight of each input in the movement of each output, we calculate $f(i) = \ln[1 + d(i)*e(i)]$. Finally, we resolve the conundrum that some inputs work in the direction of increase and some in the direction of decrease, in each intercohort comparison, by calculating the absolute value of each $f(i)$ as a percentage of the sum of the absolute values of all $f(i)$'s. These are the data displayed in Table 13, for total births per woman, and in Table 14, for unintended births per woman.

In Table 13, the sequencing of real

Table 13.—Percent Contributed by Each Input Variable to the Change in the Number of Total Births per Woman in Different Marriage Cohorts: United States, 1935–1969

Input Variables	From: / To:	RMC 1935–1939 / 1940–1944	RMC 1940–1944 / 1945–1949	SMC 1950–1954 / 1955–1959	RMC 1945–1949 / 1950–1954	SMC 1955–1959 / 1960–1964	SMC 1960–1964 / 1965–1969
*E*1		(+) 25	(+) 31	(+) 44	(-) 42	(-) 34	(-) 62
*E*2		(-) 8	(-) 3	(-) 2	(-) 1	(-) 2	(-) 1
*E*3		(+) 5	(+) 5	(+) 3	(+) 6	(-) 5	(-) 2
*M*1		(+) 0	(+) 0	(+) 0	(+) 0	(-) 2	(-) 1
*M*2		(-) 4	(+) 6	(+) 6	(+) 7	(+) 4	(-) 6
*M*3		(+) 36	(+) 53	(+) 35	(+) 34	(+) 44	(-) 23
*C*1		(-) 2	(+) 2	(+) 4	(+) 2	(+) 4	(+) 1
*C*2		(+) 17	(+) 0	(+) 4	(+) 6	(-) 3	(-) 4
*C*3		(+) 2	(+) 0	(+) 2	(+) 0	(+) 1	(-) 0
Percent Change in Output		+4.2	+7.5	+8.1	+0.5	+0.1	-30.4

(RMC) and synthetic (SMC) marriage cohorts is the same as in Table 1 above, dating the real marriage cohorts some five to ten years subsequent to the time of marriage to correspond with the data at which they arrive at the mean duration of fertility. The final row indicates the observed percentage change to which the components have made their respective contributions. The attached signs are the product of the sign of the elasticity and the sign of the change in the input, but enclosed in parentheses because the calculation of the percentage contributions is based on the absolute value of that product. As one would expect, the individual contributions of each partialling operation in turn (on the assumption that no other change occurs) do not precisely reproduce the observed change in the output; because of interactions, they turn out to be a little larger (by less than 1 percent on the average).

Table 13 permits us to give a "causal" interpretation of postwar changes in fertility, i.e., in mean parity per woman (at duration 15 to 20 years). As can be seen from the final row of the table, there are three phases to the movement of fertility: moderate increase in the first half of the series, trivial increase in the next two quinquennial comparisons, and precipitate decrease in the final period. In the first phase, although the contribution of the intended birth component is substantial, it is on balance outweighed by the effect of the rise in the unintended fertility rate; in the second phase (of trivial change), a decrease in response to changes in *E*1 is almost exactly counterbalanced by an increase in response to changes in *M*3. Only in the last quinquennium does the change in intended births per woman clearly exceed in import the change in the rate of unwanted fertility. As noted previously, it is much more complex to interpret changes in births per woman for synthetic than for real cohorts, because synthetic cohorts reflect in the quantum of their intended fertility whatever changes may be occurring in the tempo of real cohort fertility. If the period 1965–1969 can be characterized, as we think it can, as unpropitious for childbearing, in the sense that the participant cohorts not only reduce somewhat their intended number of births but also tend to postpone births that they intend to have subsequently, the postponement as well as the reduction will be reflected in a collapse of both the total births per woman and the intended births per woman. The force of this proposition cannot be tested until we have sufficient information to make real cohort calculations covering the era of the 1960s. We predict that, once these are produced, they will show a much smaller decline in both total and intended fertility than is implied by the currently available synthetic cohort data. For example, the birth cohort fertility data for the United States show a decline of 24 percent in the total fertility rate from the synthetic cohort of 1960–1964 to that of 1965–1969, whereas the estimated decline for the comparable real cohorts (dated at their mean age of fertility) is only 15 percent (Ryder, 1978).

All of the remaining contributions to change in total births per woman are trivial, with the sole exception of the contribution of $C2$, pre-termination nonuse fertility, in the first quinquennium of increase. As reported in Table 9 above, there was a 24 percent rise in this index from the first to the second real marriage cohort. Although we have no evidence to substantiate the view, it is our opinion that the low value for the first real marriage cohort reflects a substantial amount of wartime separation of spouses, an exclusion from exposure not taken into account in our model.

We have conducted the same kind of analysis of changes in the mean number of unintended births, with the results shown in Table 14. Every intercohort change up to the last is characterized by a substantial increase in the mean number of unintended births per woman. Although the principal contribution to that change in every case is the rate of unintended fertility ($M3$), it is of some interest that the other inputs are not negligible; they contribute something like one-half of the influence on the mean number of unintended births. Except for the increase in $C2$ from RMC·1935–1939 to RMC·1940–1944, discussed immediately above, the second strongest influence on change in the number of unintended births is change in the number of intended births ($E1$); in the final quinquennial change, its contribution substantially counterbalanced the effect of decline in the rate of unintended fertility. Again, the precise interpretation of the role of the number of births intended, in a synthetic cohort comparison, is complicated by postponement.

CONCLUSION

This has been a report of some results from the 1965 and 1970 National Fertility Studies. We have designed a set of observations and measures appropriate to summarize fertility by planning status for successive real and synthetic marriage cohorts during the postwar era in the United States. The establishment of defensible estimates for this assignment has required attention to a series of methodological problems. Compromises in the scope of the exercise were forced on us by data which had not been collected, and by data which had been collected but could not be trusted. Special procedures have been developed for estimating the mean years of premarital exposure to risk and the mean length of intended use to delay. A particular feature of the calculations was a set of precautions to ensure comparability with respect to the range of ages at marriage as well as to the range of marital durations, and also with respect to the joint exploitation of two distinctive surveys.

The results have been presented in two forms. One set of tables contained estimates of two output parameters (total

Table 14.—Percent Contributed by Each Input Variable to the Change in the Number of Unintended Births in Different Marriage Cohorts: United States, 1935–1969

Input Variables		RMC	RMC	SMC	RMC	SMC	SMC
	From:	1935–1939	1940–1944	1950–1954	1945–1949	1955–1959	1960–1964
	To:	1940–1944	1945–1949	1955–1959	1950–1954	1960–1964	1965–1969
*E*1		(-) 6	(-) 11	(-) 18	(+) 17	(+) 14	(+) 24
*E*2		(-) 11	(-) 4	(-) 4	(-) 2	(-) 3	(-) 2
*E*3		(+) 7	(+) 7	(+) 4	(+) 10	(-) 7	(-) 3
*M*1		(+) 0	(+) 0	(+) 0	(+) 0	(-) 3	(-) 2
*M*2		(-) 5	(+) 8	(+) 10	(+) 10	(+) 6	(-) 10
*M*3		(+) 44	(+) 67	(+) 50	(+) 48	(+) 56	(-) 50
*C*1		(-) 2	(+) 3	(+) 5	(+) 4	(-) 4	(-) 7
*C*2		(+) 22	(+) 0	(+) 6	(+) 10	(+) 6	(+) 2
*C*3		(+) 2	(+) 1	(+) 3	(+) 1	(+) 2	(+) 0
Percent Change in Output		+20.3	+31.9	+23.8	+19.8	+20.1	-37.9

births and unintended births per woman) and nine input parameters (three ends, three means, and three conditions) for cohorts which covered, in real or synthetic form, the first 25 years of postwar experience. In that era, fertility rose to a plateau and then abruptly declined. During the phase of rising fertility, popularly known as the baby boom, it is approximately correct to say that there was no change in the mean number of intended births. There was, however, a deterioration in the effectiveness with which unintended fertility was prevented, and a consequent rise in unintended births. From the standpoint of the tempo of intended fertility, the baby boom may be characterized as a period of substantial decline in the length of intended delay, accompanied by substantial decline in the ability to achieve that intended delay.

The story of fertility in the 1960s is apparently very different from that, although problems of interpretation of synthetic cohort parameters prevent us from achieving a clear characterization without subsequent information for the real cohorts concerned. From 1960–1964 to 1965–1969, fertility declined abruptly, primarily as a consequence of a drop in the mean number of intended births per woman. Some of that drop in number of intended births is undoubtedly a reflection of the practice of postponement, as distinct from a real reduction in eventual mean parity, although how much this is so it is impossible to say with these data. At the same time, there has been a marked increase in the length of intended delay and in the effectiveness with which respondents achieve their intentions both to delay and to terminate.

The second form in which results have been presented required an algebraic formulation of fertility as a function of the nine input parameters. With such a formula, it is feasible to calculate coefficients of elasticity of fertility with respect to each input, showing the percent change in fertility which would result from a 1 percent change in the input, ceteris paribus. The procedure has been extended to quantify the proportional contribution of change in ends, means, and conditions to intercohort movements of total and unintended fertility. Two variables are dominant in this set of results: the number of intended births per woman, and the rate of unintended (post-termination) fertility. With the exception of the dramatic decline in fertility during the 1960s, a correct interpretation of which requires data for the 1970s for the cohorts involved, these two variables have been of approximately equal importance in their influence on intercohort change in total births per woman.

It is worth considering directions in which the present work may be extended. There would seem to be little prospect of pushing the time series back any further by using the 1955 and 1960 Growth of American Families Studies. The earliest cohort represented in these surveys with an adequate age at marriage range would be the real marriage cohort of 1935–1939, already included in the present account. More promising are the prospects for extending the time series forward by exploiting the output of the National Surveys of Family Growth of 1973 and 1976. Such a task is unlikely to be automatically rewarding; one by-product of the present work is a heightened sensitivity to the difficulties of comparability created by any small modifications in the questionnaire. Another direction of elaboration is the production of comparable sets of observations and indices for subpopulations, such as the Catholics and non-Catholics; the cohort approach is well adapted, within the confines of subsample size, to the study of groups distinguished by ascribed characteristics.

Our own predilection is to repeat the present work using data collected in our 1975 National Fertility Study. With this sample of intact white marriages (with wife's age at marriage less than 25), we can compare the real marriage cohorts of 1951–1955 and 1956–1960, and the synthetic marriage cohorts of 1966–1970 and 1971–1975, for their experience through duration 15 to 20 years. The questionnaire for this survey is superior to its predecessors in the quality and quantity of the information required to classify fertile pregnancies and person-years of exposure to risk by intention and use. We contemplate three directions of adumbration: (a) subdivision of wives by age at marriage; (b) distinguishing nonterminal contraceptive methods from contraceptive sterilization; and (c) developing formulae to express tempo as well as quantum outputs as functions of the array of reproductive inputs. It is our conviction that the determinants of the tempo of reproduction are the most appropriate focus for future American fertility research.

Our experience with fertility inquiry in the United States has been a humbling one. Only gradually are we learning what we need to learn. The interpenetration of quantum and tempo variations, which has confounded simple-minded attempts to infer real cohort behavior from the comparison of successive period cross-sections, is paralleled, within the experience of each real cohort, by the interpenetration of the changing rates of unintended fertility and the changing amount of exposure to risk. In trying to extend our reach beyond the official registration and enumeration systems into the details of individual reproductive histories, we have developed an appreciation for the importance as well as the complexity of the task. Every effort to refine our measurement scheme has been accompanied by new problems of noncomparability across time. Perhaps most disquieting is the limited extent to which our surveys have yielded correlates of the changes through time in reproductive behavior. But at least we now have a reasonably clear sense of what it is we should be trying to explain; the proximate sources of temporal variations in postwar fertility are now documented.

APPENDIX

The nine observations for each cohort, as described in the text, consist of five categories of births (BC, BD, BE, BF, and BU), and four categories of years of exposure (YE, YF, YP, and YS). The inputs are defined in the text as follows:

$$E1 = BC + BD + BE + BF,$$
$$E2 = (BD + BE + BF)/E1,$$
$$E3 = \{c\ln[BE/(BE + BF)]\}/(BF/YF),$$
$$M1 = (BE + BF)/(BD + BE + BF),$$
$$M2 = BF/YF,$$
$$M3 = BU/YU, \text{ where } YU = (YP + 17.5) - YS - BT - (YE + YF),$$
$$C1 = YP,$$
$$C2 = (BC + BD + BE)/YE,$$

and

$$C3 = YS.$$

Thus,

$$\begin{aligned} BU &= M3*YU \\ &= M3*[YP + 17.5) - YS - BT - (YE + YF)] \\ &= M3*[(C1 + 17.5) - C3 - (E1 + BU) - (YE + YF)]; \end{aligned}$$

that is to say, $BU = [M3/(1 + M3)]*[(17.5 + C1 - C3 - E1) - (YE + YF)]$.

In words, the number of unintended births is the product of the unintended fertility rate (discounted for loss of exposure whenever an unintended birth occurs) and the number of post-termination years of exposure to risk. The latter depends on the gross married time (17.5), plus premarital exposure ($C1$), minus sterile time ($C3$), minus one year of pregnancy and puerperium for each intended birth ($E1$), and finally minus the years of pre-termination exposure to risk ($YE + YF$). The last can be derived from the input definitions as follows:

$$\begin{aligned} YE + YF &= [(BC + BD + BE)/C2] + (BF/M2) \\ &= \left(\frac{E1 - BF}{C2}\right) + \frac{BF}{M2}. \end{aligned}$$

Thus, intended births are divided into the use-failures (BF) with a fertility rate $M2$, implying person-years of use of ($BF/M2$) and the rest ($E1 - BF$), with a fertility rate $C2$, implying person-years of nonuse of $(E1 - BF)/C2$.

The final question concerns the value

$$\begin{aligned} BF &= \left(1 - \frac{BE}{BE + BF}\right) * \left(\frac{BE + BF}{BD + BE + BF}\right) * \left(\frac{BD + BE + BF}{E1}\right)*E1, \\ &= [1 - \exp(-E3*M2)]*M1*E2*E1. \end{aligned}$$

The proportion of intended births ($E1$) that are use-failures depends on the proportion not wanted as soon as possible ($E2$), the proportion of those which are preceded by use ($M1$), and finally on an expression which indicates the dependence of failure not only on the fertility rate despite use ($M2$) but also on the length of the interval of intended use ($E3$)—in the elementary sense that, the longer the intended use, the higher the likelihood of failure.

The expression for BU is obtained by combining the expressions for BU, for $YE + YF$, and for BF, as indicated above.

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