What Is Going to Happen to American Fertility?

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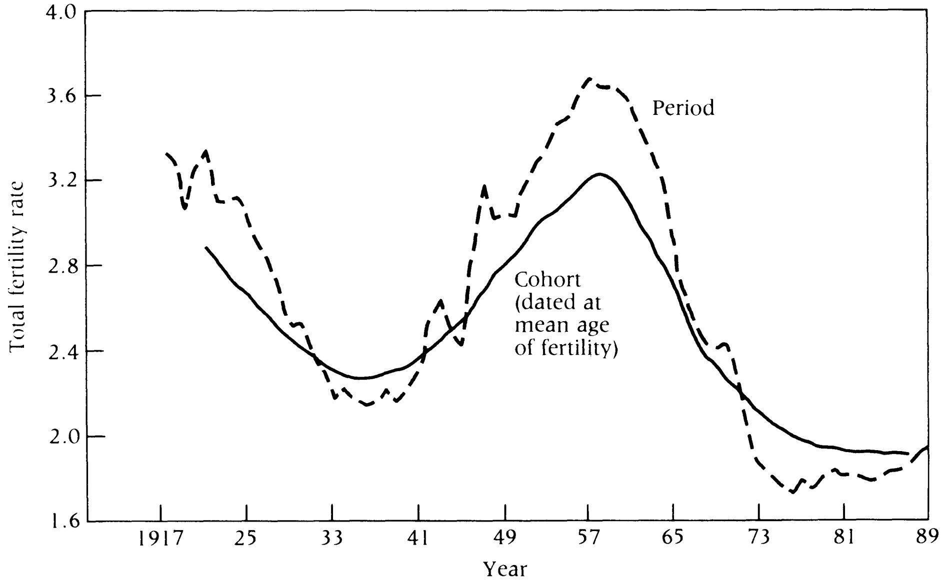
ONE OF THE MOST USEFUL PRODUCTS social science has to offer is the population forecast. Yet there has been little basic change in the format of the procedure since its precepts were first formulated, independently, by A. L. Bowley (1924) and P. K. Whelpton (1928). The most problematic component of the forecast is fertility. In the past 60 years we have learned a lot about fertility, not least about how to measure it, but little of this seems to have trickled down into the mundane world of the forecast. It remains an old-fashioned art form, somewhat embarrassing to the profession, like a disreputable relative.

To begin with, a few words about terminology. Demographers are coy about what they are doing. They never say "forecast." Their preferred term for the product is "projection," meaning that one works out the consequences of a set of assumptions, but disavows responsibility for the credibility of those assumptions. Now, "projection" is a fancy Latin word, meaning "the act of throwing forward," or what plain folk would call a forecast. Moreover, the projection product is always accompanied by a narrative written to make the choices of assumption sound reasonable. And the consumers of the product certainly use it as if it were a forecast. The preference for "projection" rather than "forecast" to describe the procedure is probably no more than a way to advertise that the result is error-prone.

The central question in the conventional forecasting procedure is, What is going to happen to the period total fertility rate—the sum of age-specific birth rates in a given year? Its movements, over the available time series, 1917–89, are shown by the broken line in Figure 1. Only a casual inspection of the graph is needed to show how formidable is the challenge of guessing where it will go next. Not only is the time series highly irregular, but it also has a quasi-cyclical look to it, at least for one "cycle." If we choose simply

FIGURE 1 Total fertility rate, United States, 1917–89



SOURCES: Heuser (1976) and addenda published in the Vital Statistics Annual Report for each year through 1987.

to extrapolate, on the reasonable ground that we do not know any better way to proceed, there is no acceptable statistical analysis to guide the search among alternative ways to do so.

Much of what we have learned about fertility is derived directly or indirectly from Whelpton's efforts to improve forecast procedures. His earliest forecasts were a combination of extrapolation and judgment. If one were to restrict one's attention to the first 20 years of the period total fertility rate depicted in Figure 1, the assignment would not seem too difficult, particularly in light of other historical evidence that there had been close to monotonic decline throughout the nineteenth century. Whelpton's judgment that the forces responsible for that decline would continue was by no means idiosyncratic. The overwhelming professional consensus was that fertility declined with modernization. Since nothing was perceived to stand in the way of that juggernaut, the prognosis was still further reproductive retrenchment. It was the demographic version of Spengler's *Decline of the West.*

To exemplify, consider a forecast published by Whelpton (1936) using birth data through early 1935. The most recent observed period total fertility rate (for native white women) was 2.177 for 1930–34; his medium assumption was 1.9 for 1980. Although that is a little higher than the eventual

1980 outcome, Whelpton did characterize his medium assumption as "probably somewhat too high." The upshot in terms of population size, however, was a "maximum of about 160,000,000 soon after 1980" (p. 471). In brief, the forecast was a fiasco because Whelpton (and everyone else) failed to foresee the intervening baby boom.

Whelpton's first reaction to that unexpected event was to develop the empirical basis for calculating birth rates specific for parity (the number of births a woman has already had) as well as for age, as a logical extension of a sound principle. In doing so, he serendipitously discovered the relevance of the distinction between cohort and period modes of temporal aggregation (Whelpton, 1954).

The basic element for both modes is the birth rate for women of age a in year t, say $b(a,t)$. The period total fertility rate for year t is the sum of such rates over all ages.

$$F(t) = \sum_a b(a, t)$$

The cohort total fertility rate, for the cohort of women born in year T, is similarly the sum of such birth rates over all ages, but in this case the year associated with age a is $T+a$, the year in which members of the cohort are age a.

$$F(T) = \sum_a b(a, T + a)$$

This measures the mean number of lifetime births per woman. The mean age of fertility for the cohort of year T is

$$A(T) = \sum_a a \times b(a, T + a)/\sum_a b(a, T + a)$$

The corresponding calculation can be made for each period. All period measures have artificial meanings, derivative by analogy with their cohort counterparts. The plotting convention followed for cohort measures, as in Figure 1, is to locate the value for each cohort at the time at which it reaches its mean age of fertility, that is, $T + A(T)$. Values temporally aligned with those for periods are then obtained by interpolation.

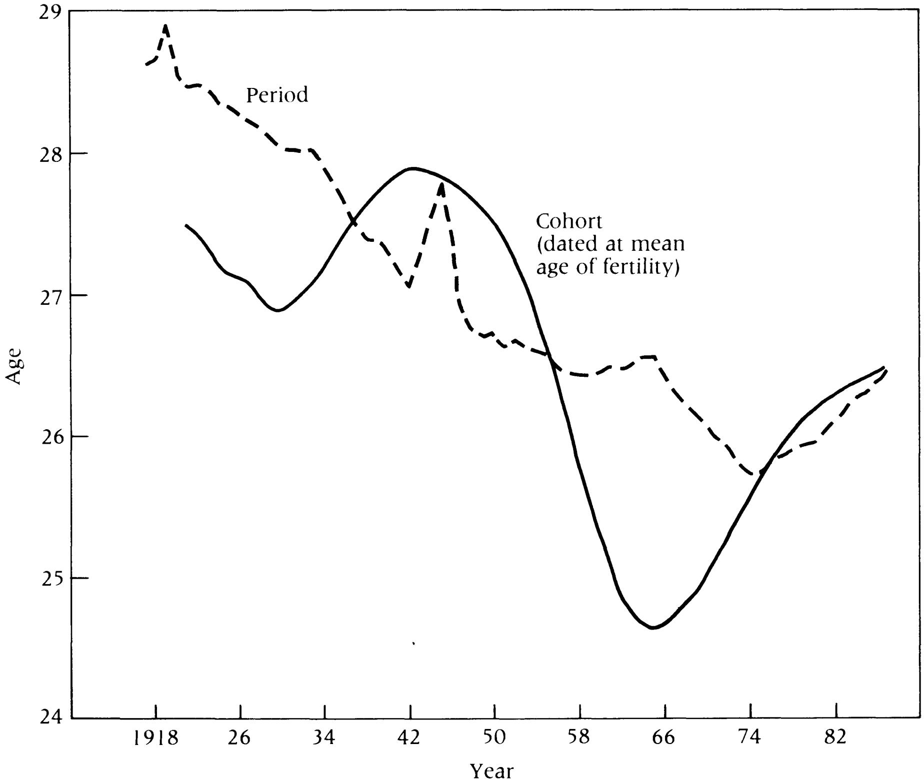
The cohort fertility tables Whelpton originated for the United States are a staple of contemporary analysis (Ryder, 1980, 1986). The shape of his cohort tables confronted Whelpton with the problem that the cohorts currently bearing children—the ones most relevant for a look into the future—have histories that necessarily stop with the last year of observation. To complete those histories, he, with Ronald Freedman, launched the first national fertility survey, the Growth of American Families Study, in 1955. Its primary purpose was "to provide the information needed in order to improve population projections for the United States" (Whelpton et al., 1966: 371). Each respondent in the survey was asked, in effect, to make her own forecast of future childbearing. In our judgment, expectations data, as they came to be called, did not help very much (Ryder and Westoff, 1967; Westoff and Ryder, 1977; Ryder, 1984), although there are contrary views (Hendershot and Placek, 1981). Nevertheless, the rest of the information collected in the survey was so rewarding that surveys have continued ever since in the United States, and provided the model for the World Fertility Survey. The analysis of reproductive behavior has been transformed and enriched.

Yet neither the cohort orientation nor the yield of survey data has made much difference in forecasting procedure. Consider the Bureau of the Census projections released in 1989 (Spencer, 1989); they were given the place of honor as the main story on the front page of the *New York Times.* In their fertility projection, it would appear from the accompanying narrative, the Census Bureau adopted a cohort orientation, but this is not true in practical detail. Their procedure may be described briefly as follows. A temporal horizon is chosen by designating a future cohort of whites (those born in 1985), and specifying three levels of F (1.5, 1.8, and 2.2) and one value of A (26.9) to be achieved by that cohort. Nonwhites have the same terminal values but at a later horizon, which is a particular period. The selection of these horizon values is an act of judgment, defended by allusions to analytic findings. Although the judgments are arbitrary, in the sense that equally knowledgeable practitioners might make different choices, that is not the issue here.

Several questions are provoked by the selected value of A. First, since cohorts of completed fertility have had most of their reproductive experience long before the threshold of the forecast, the Census Bureau has used a period value of A instead. (The same appears to be true for the central value of F.) In Figure 2, we show the time series of period and cohort A. They are clearly very different. The coefficient of determination (r^2) of the two series, for the paired values, 1921–86, is 41 percent. (The plotting convention for the cohort values is the same as described for Figure 1.) The source of departure of period A from cohort A is any tendency, over the cohorts concerned, for the total fertility rate to rise or decline, because the respective cohort F values play the role of implicit weighting factors in the age-specific rates observed for a period. (In our judgment, it would be better to estimate cohort A by using extrapolation to complete the histories of some of the more recent cohorts, as described below.)

In the second place, the level of F for a cohort is an influential consideration in the value of A, since that level is closely associated with the number of birth intervals encompassed in the calculation of A. Without explanation, the Census Bureau assumes that A is independent of F. Third, the specification of A does not fulfill the requirements of the forecast pro-

FIGURE 2 Mean age of fertility, United States, 1917–87



SOURCE: See Figure 1.

cedure—age-specific birth rates are needed to estimate births in the way they have chosen. Although the exposition of the way to obtain an age pattern of fertility for the horizon cohort, given the value of *A*, is unclear, internal evidence suggests that the pattern is period-based, and thus problematic.

In summary of the point, although it would appear that the Census Bureau is making a projection on a cohort basis, the arithmetic of the procedure remains period-bound. This is an understandable choice of convenience because, at the beginning of a forecast, only incomplete histories are available for contemporary cohorts, whereas each new year provides a complete cross-sectional story; and at the end of the forecast, the requisite for proceeding with the population projection is the number of births for each new year, which calls for a period cross-section of cohort performance. But this is rationalization, not justification.

I am currently preparing a short monograph, at the behest of Statistics Canada, to resolve the practical problems of employing a cohort orientation in making a fertility forecast. Although the details are unnecessary to the present account, the outline of the approach may be sketched. The data that constitute the contemporary empirical threshold for the forecast are age-specific birth rates, for the separate orders of birth, for a series of cohorts. Since the histories of recent cohorts stop with the most recent period of observation, a summary of current reproduction from a cohort viewpoint requires a compromise between the amount of future fertility to be estimated for older earlier cohorts, and the amount of past fertility to be neglected for younger later cohorts. The dividing line that equates these, in a fixed fertility model, is the cohort which reaches its mean age of fertility at the end of the time series of observations—in these data the cohort born in the year centered on the beginning of 1961. A triangle of fertility rates must be estimated, consisting of one year for age 27, two years for age 28, and so forth. Risk of error is unavoidable, but it is mitigated by the circumstance that, for the younger ages (with higher fertility), the extrapolation distance is short, and where it is long, there isn't much fertility.

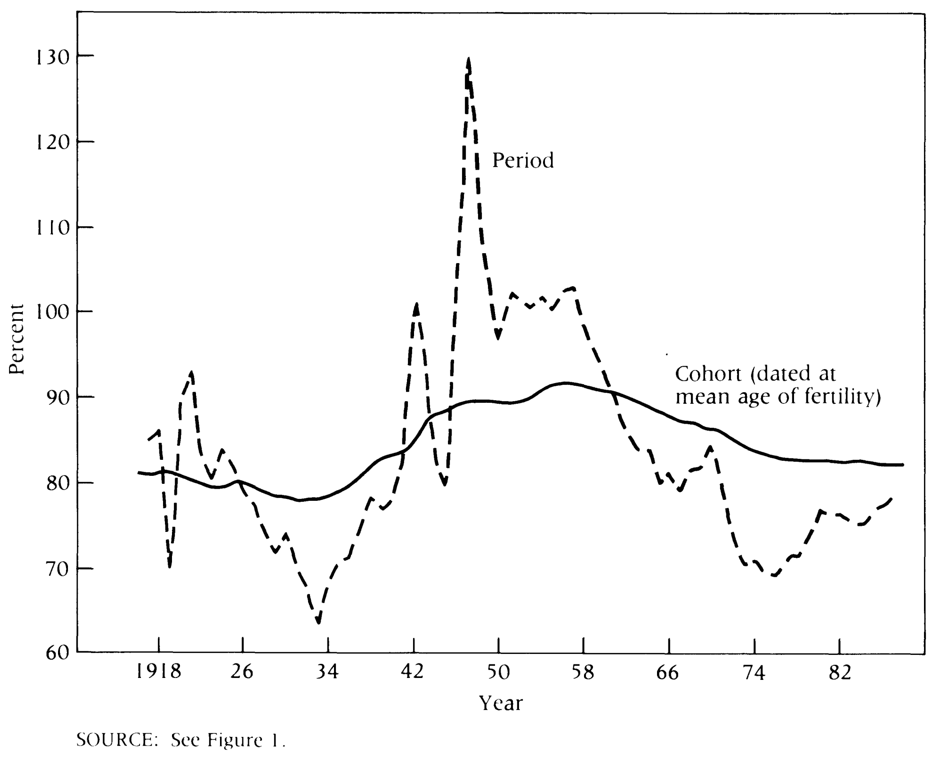
In carrying out this estimation, I decided to work with age-parity-specific fertility rates for the last 15 years, fitting a curve to each, and extrapolating it the required distance. The 15-year base was chosen to be long enough to smooth out short-term irregularity, but not too long—because the further back one goes in time, the less relevant the evidence is for the future. In extension of this principle, I used a weighted (least squares) fit, with heavier weights for rates closer to the present. Moreover, if 15 years is as far back as it is useful to go, it follows that 15 years ahead is as far as one is entitled to look into the future. Accordingly, I constrained the curve to a zero slope 15 years beyond the present. The form of the curve is: $y = a + b(30 - x)^2$, fitted to values of y for $x = 1$ to 15 and extrapolated to $x = 30$. One useful property of the curve is that the change in the fitted values, from $x = 15$ to 30, is one-third of the change from $x = 0$ to 15. In short, the observed slope is rapidly dampened.

By this means, I obtain estimates of current fertility, through the cohort of 1961, in the form of age-specific birth rates for separate orders of birth. These are the sources of the cohort parameters, F and A, depicted in Figures 1 and 2. But these aggregate measures are considered less appropriate for analysis and forecasting than their constituent elements. Consider first the quantum measure, F, the total fertility rate. I have long advocated that this deserves partitioning into successive parity progression ratios (Ryder, 1951). These are the proportions of women who move from each lower to the next higher parity at some time in their reproductive careers. To exemplify the usefulness of this partitioning in a forecast, note that, during the era in which fertility rose from its depression trough to the peak of the baby boom, 90 percent of the increase was attributable to larger proportions moving from parity zero to parity one, and from parity one to parity two, whereas, during the subsequent slump, only 40 percent of the decrease was attributable to decline in those two progression ratios.

There is particular analytic interest in the zero-parity progression ratio, which can also be thought of as the proportion of women in the cohort who are eventually fertile, or the total fertility rate for first-order births. The corresponding measure can be calculated for a period, from the same data. The broken line in Figure 3 shows the total fertility rate for first-order births, for periods. Its movements are chaotic, and its level frequently absurd. In the early 1930s and again in the middle 1970s, it falls below 70 percent, but for 11 of the years since World War II it exceeds 100 percent—an impossible assignment for any real group of women.

Suppose, for sake of argument, that women were limited to one birth each. In such a world, the broken line in Figure 3 would be the period total fertility rate. We doubt that, in such a world, any analyst would waste time on that particular series as a reasonable surrogate for cohort behavior. Only a little reflection would suffice to show that something is perverting the outcome. That something is the way in which the time pattern of first births may change from one cohort to the next, sometimes in momentary response to tribulations—note in the graph the marks of World War I and World War II, and even the Vietnam War—and sometimes systematically as cohort

FIGURE 3 Proportion fertile (in percent), United States, 1916–88



SOURCE: See Figure 1.

reproduction is shifted toward the earlier ages, as occurred during the postwar baby boom. It does not take much of an intellectual leap for a demographer to realize that tempo change is disrupting quantum measurement. After all, becoming a parent is formally almost analogous to dying, in the sense that nearly everybody does it, and the key question is when.

The period total first-order fertility rate is so obviously distorted that nobody pays attention to it. An analogous case can be made against the period total second-order fertility rate, and so on. Yet when summed, these distorted constituents become the conventional measure of choice, the period total fertility rate. In my opinion, the principal reason we still pay attention to period measures of fertility is that they are convenient. I am reminded of the story of the passerby who sees a drunk standing at night under a street lamp looking for his lost wallet. The passerby asks the drunk where he dropped the wallet, and the drunk replies: "Somewhere down the street, but I can't look there because it's too dark."

Consider now the solid line in Figure 3, the total first-order fertility rate for cohorts. That is perhaps most evocative when considered in terms of its complement—the proportion of women who have no children. For the earliest cohorts, that value was 18 percent; it dropped to 8 percent at the peak of postwar childbearing, and has now returned to about 18 percent again. Two papers appeared recently, by David Bloom (1982), and by Bloom and James Trussell (1984), in which the same measure was estimated, using a different model. The data for the first paper were the cohort fertility tables through 1979. The proportion infertile was estimated to rise to 28 percent for the cohort of 1955 (for brevity: c. 1955). The second paper was based on three national surveys (1976, 1978, and 1980); for those data, with the same methodology, the estimate of proportion infertile for c. 1955 was 25 percent. This striking finding quite properly attracted attention. For example, James Sweet and Larry Bumpass (1987: 397) singled it out, in their census monograph, as evidence of a dramatic transformation in family life. Now, with hindsight and a different approach (admittedly not itself unflawed), I come up with a value of 17 percent for the same cohort, certainly much higher than a decade before but not without historical precedent. Bloom and Trussell had bad luck: the data on which they perforce relied came from a brief trough in period fertility, and that caused their otherwise sophisticated model to go awry.

Partitioning is advantageous for tempo measures as well. In Appendix 1, I show how to partition the mean age of cohort fertility, A, into three elements: the mean age of first-order fertility, A_1, the mean length of birth interval, I, and the weight, Q, to be attached to the latter to signify the mean number of birth intervals encompassed in the calculation.

$$A = A_1 + Q \times I$$

The value of the weight, Q, evidently depends on the extent of progression beyond parity one.

Table 1 shows these values for the first cohort in the series (c. 1893), for the cohort with the local peak age of fertility (c. 1915), for the cohort with the local trough age of fertility (c. 1941), and for the last cohort in the series (c. 1961). Although a more elaborate partitioning is feasible, there seem to be compelling advantages in parsimony, particularly for the purpose of making a forecast.

In the table, we can see two episodes during which the mean age of fertility is rising—at the beginning of the series and at the end. In both episodes, the effect of substantial increases in the two tempo components (A_1 and I) is muted by decline in the quantum weighting (Q). In the episode of declining mean age of fertility, to the contrary, the value of Q changed little, and the decline in the mean age of fertility was correspondingly large. As an aside, it is reassuring that the two tempo components show similar variations over time, since interval length cannot in fact be measured directly with the available data. (See Appendix 1.)

There is particular reason for interest in the constituents of change in the mean age of fertility, because that change distorts the period total fertility rate. Consider, for example, the long episode of decline in the cohort mean age of fertility: it drops by three years over a span of 26 cohorts. In effect, 26 years worth of cohort fertility is compressed into 23 periods. (This is a simple adaptation of a formal model described in Ryder, 1983b.) That is why the period total fertility rate was so much higher than that of the cohort for so long (as in Figure 1). Considering the more recent experience shown in Figure 2, the cohort mean age of fertility has risen by two years over a span of 20 cohorts. That has the effect of dispersing 20 years worth of cohort fertility across 22 periods. Consequently, as shown in Figure 1, the period total fertility rate has for the past several decades been lower than its cohort counterpart. It is time we leave the street lamp and go in search of the wallet.

Many years ago, I exploited the partitioning of cohort total fertility and mean age of fertility to make a successful forecast (Ryder, 1958). Just before

TABLE 1 Components of the mean age of fertility for selected cohorts, 1893–1961

Cohort	Mean age of fertility	Mean age at first birth	Mean number of birth intervals	Mean length of birth interval
1893	27.6	23.2	1.76	2.5
1915	27.9	24.3	1.12	3.2
1941	24.7	22.0	1.06	2.5
1961	26.5	24.3	0.61	3.5

SOURCE: See Figure 1.

fertility peaked, I predicted that the birth rate would drop by 20 percent in the next decade, and by even more if economic conditions worsened. The actual drop was 27 percent. There were two bases for the prediction. First, the progression ratios primarily responsible for the baby boom, those for parities zero and one, had risen so high that they were close to their physiological upper limits. Accordingly, any further rise in fertility would have to come from some unprecedented source. Second, period measures had been grossly inflated by the steep decline in the mean age of fertility, and principally in the mean age of first-order fertility. With the continuing increase in the age of leaving school, it seemed much more likely that the mean age of first-order fertility would reverse direction than that it would continue declining. If so, upward distortion would be replaced by downward distortion, and period fertility would subside. The point of this is not to return to the scene of a past triumph—since it would not be mentioned had it been a failure—but rather to show that there can be a forecasting payoff from using the most apt measures (cf. Ryder, 1984).

For the same reasons that I advocate that fertility analysis focus on the separable components of the quantum and tempo of cohort fertility, I propose that these parameters be the object of attempts to forecast fertility. There are two reasonable alternatives. In the first, one selects a horizon cohort some distance in the future, specifies the values of the respective parameters to be achieved by that cohort (using judgment about the hypothesized determinants of their magnitudes as they are expected to change), and produces the intervening values by some process of interpolation.

In the second alternative, an agnostic position is maintained with respect to the future: the development of parameter values out to the horizon cohort is an extrapolation from a threshold of generous length, describing variations in the recent past. For this assignment, I recommend an extrapolation of the same form as described above (using weights and constraints), although in this case it seems better to use a longer base, a higher order of polynomial, and thus a greater degree of constraint (such as first and second derivatives zero at the horizon).

The remainder of the forecast procedure is as follows. With parameters of cohort reproductive histories for future cohorts, obtained by interpolation or extrapolation, some practical problems must be solved. The first step is to transform the respective parameter values into age moments (the total fertility rate, and the mean and variance of fertility by age) for each cohort. This is simply the obverse of the partitioning procedure. The second problem is to translate these cohort measures into their period counterparts. I use the cohort moments to identify the simplest age distribution of cohort fertility that reproduces them, a three-value fertility structure with age locations selected to coincide with period midpoints. This permits the temporal realignment of the resultant values to yield the required period moment measures. Extensive testing has demonstrated the effectiveness of this procedure, a particular adaptation of the formulas provided in Ryder 1983b.

The final problem is that the conventional forecast procedure requires future age-specific birth rates in order to produce estimates of the number of births each year. I have devised a technique, explained in Appendix 2, to accomplish this objective directly from the period moment measures obtained above by translation. In summary, I propose that a forecast should begin with an appraisal of the current reproductive context from a cohort perspective, characterized in terms of quantum and tempo parameters of cohort reproductive histories, as in fertility analysis. Although this stance involves several practical problems with respect to the rest of the forecasting assignment, each problem has a reasonable solution.

The current state of American fertility is very different from what it was a generation ago. The proportion infertile has increased from its historic low of 8 percent to a current 18 percent. The mean number of additional children, for those with at least two, has declined by 60 percent, from 1.8 to 0.7. The diminution of third and higher order births is a trend of very long standing, and it cannot decline much farther. The mean age at first birth has risen from 21.9 to 24.5 years, the largest rise in that parameter in history.

When asked what fertility is now, a demographer is likely to cite the most recently published period total fertility rate, typically a couple of years in the past. I have devised a procedure for estimating that measure as soon as the birth count is available. (See Appendix 2.) The results, plotted as the broken line in Figure 1, are 1.93 for 1988 and 1.96 for the first ten months of 1989. From the last half of the 1970s to the last half of the 1980s, this measure has risen by 6 percent, small but noteworthy because it comes on the heels of a decline of more than 50 percent in the previous two decades. From a cohort standpoint, however, the comparable calculation gives a 7 percent decline. The reason for the contrast is that, a decade ago, the cohort mean age of fertility was rising so rapidly that it distorted period fertility downward by 15 percent, but currently the rise is much less rapid and the downward distortion is only 4 percent. Since the level of cohort fertility is changing only slightly, its decline appears, when looked at through the period prism, as a small rise. The period measure is no less real than the cohort measure, and it is the calculation most directly linked to population growth and age-structure formation. But in the model of reproductive behavior, the driving force is change in cohort fertility. The actors are members of cohorts; their behavior is manifested in cross-section period summations in a distinctive manner because of ongoing change in the way those actors are distributing their reproductivity over time.

One extraordinary feature of the current situation deserves emphasis. It may be that, for the present at least, reproductive behavior has stopped changing. If so, that would be the first such occasion in our statistical history.

Consider the evidence. In the first place, recent change in all of the cohort parameters examined has been at a slower pace than a few years ago. Second, as can be seen in Figure 1, an intersection of the graphs of cohort and period total fertility is imminent. That is the sign of zero slope in the cohort mean age of fertility—the obverse of the account of the source of distortion. Third, as can be seen in Figure 2, there will soon be an intersection of the graphs of the cohort and period mean ages of fertility. Now, the principal source of divergence of the cohort and period mean ages of fertility is change in the level of cohort fertility. Their equality would be a sign of zero slope in the cohort total fertility rate. Although these relations between intersections and slopes must be regarded with caution, given the short-term variability of period measures, they are at least suggestive.

Finally, I have examined the records to date, for cohorts now in their early 20s. Those records are indistinguishable from one another, in both quantum and tempo respects. I made a comparable statement in an earlier publication (Ryder, 1986), for which the data source was the tapes of the detailed birth histories of cohorts, collected in the 1980 and 1985 June Current Population Surveys (US Bureau of the Census, 1982, 1986).

Two decades ago, as part of the work of the Commission on Population Growth and the American Future, I carried out an experiment in population projection (Ryder, 1972). I produced a set of simulations based on a variety of fertility patterns, together with one mortality assumption. In these simulations five reproductive parameters were specified, with five different values for each, and projections were produced for all 5^5 combinations. Then the set of 3,125 projections was examined to determine which one represented the most desirable outcome on demographic criteria. Those conditions were posited as an ultimately stationary population, to be achieved relatively soon (so that population size would be no larger than necessary), a relatively invariant stream of births year by year (to avoid costly irregularity in the age distribution), and a feasible transformation of the reproductive pattern in terms of (a) the amount of change from then-current values and (b) the time allowed for that change to occur.

The relevant data are shown in Table 2. (The three component parameters were calculated by a slightly different procedure from that described above.) The values shown in the first column, for c. 1941, represent those prevailing at the time the projections were made. The values in the second column were those that yielded the optimum population projection, as specified. The parenthetical value for the total fertility rate is the value with immigration taken into account. Although the optimum projection was produced under an assumption of no net immigration (and a total fertility rate of 2.08 required for replacement), a formula was provided to show how much fertility would be required for replacement, in consideration of the contribution immigrant women make to fertility. With that formula, and the

TABLE 2 Three cohort fertility patterns: Observed for c. 1941, currently estimated for c. 1961, and an optimum projection (in 1972) for cc. 1960–64

	c. 1941	cc. 1960–64: optimum	c. 1961
Total fertility rate	2.68	2.08 (1.92)	1.91
Progression, parities 0 and 1	0.89	0.80	0.80
Mean age at first birth	22.2	24.5	24.4
Mean length of birth interval	2.4	3.0	3.4

SOURCE: See text.

current Bureau of the Census assumption of one-half million net immigrants per year, the replacement level of fertility becomes 1.92. The values in the final column are those now estimated for c. 1961.

The Commission was established because of concern about the rapid growth of the American population and the possible need for a policy to do something about that. Once I had determined the fertility pattern required to yield the demographic optimum outcome, I described it as "an outcome which, although by no means certain, is at least not unlikely," and then proceeded to defend that assertion. It was a Pollyanna projection: Everything was going to turn out for the best. The coincidence with the present state of affairs, as shown in Table 2, is remarkable.

If the status quo is maintained, there will be a stationary population fairly soon, at a level of some 330 million. The annual number of births will peak in the neighborhood of 4 million, perhaps in 1990, and then start sliding down. It has been suggested that we should expect a secondary baby boom, as the descendants of the first boom arrive in the childbearing ages. A convenient way to measure this phenomenon is the ratio of the crude birth rate (per thousand per year) to the period total fertility rate (per woman) (Ryder, 1980). That ratio serves as an index of the age distribution, more specifically of its favorableness to fertility. Over the span from 1961 to 1981, the ratio rose from 6.4 to 8.7; then it began to decline and will probably continue declining into the next century. In brief, the generational echo of the baby boom would have happened by now, and it did not. The reason it did not was the decline in period fertility.

What is going to happen to American fertility? If there really is stability at present, does that represent the onset of an equilibrium, or is it just another turning point? The specter that haunts any forecaster in such a situation is the baby boom. In order to speculate on whether something like that may be about to happen again, some explanation is required for why it happened before (Ryder, 1982).

Since the baby boom is of interest and concern because of its consequences, it seems most appropriate to measure it in terms of change in the stream of births. From the available history, using averages for five-year periods to avoid particular idiosyncratic years, the number of births in millions declined from 2.927 in 1917–21 to 2.370 in 1933–37. Then it rose to 4.272 in 1957–61 (the baby boom) and declined again to 3.173 in 1972–76 (the baby bust). These swings in cohort size may have something to do with the subsequent reproductive behavior of the cohorts in question. For the initial phase of decline in cohort size (of 1.3 percent per year) there was an associated rise in the cohort total fertility rate (of 1.1 percent per year); for the subsequent phase of rise in cohort size (of 2.5 percent per year) there was an associated decline in the cohort total fertility rate (of 2.0 percent per year). The correlation betwen cohort total fertility rate and cohort size is $r = -0.88$. Comparable correspondence can be found with respect to the cohort mean age at first birth, although in this case direct rather than inverse. The initial decline in cohort size is associated with a decline of 1.9 years in the mean age at first birth; the subsequent rise in cohort size is associated with a rise of 2.1 years in the mean age at first birth.

The first question to be addressed, accordingly, is whether the decline (of 2.1 percent per year) in cohort size from 1957–61 to 1972–76 presages yet another rise in the total fertility rate and another decline in the mean age at first birth. It is well known that correlations of time series, unsupported by reason, can be highly misleading. In this case, however, a somewhat respectable argument has been advanced that the connection is more than coincidence. The marginal status of young adult males in the labor force makes them highly sensitive to the arithmetic of the market. Should a cohort on entry into the work force happen to have fewer numbers than its immediate predecessors, its members would have a better chance at a better job, ceteris paribus. Since that would increase their ability to afford children, the inference is drawn that they will tend to father more children.

On closer scrutiny, the argument is flawed. First, from a statistical standpoint, the proposition is not that cohort fertility and cohort size are inversely related, but that change in cohort size may cause change in the cohort total fertility rate. The theory concerns not the trend but fluctuation about the trend. For relative first differences, the correlation turns out to be only -0.44. A modified version of the hypothesis is that entry into responsible parenthood is conditional on achieving some income threshold; if labor market conditions make that possible sooner, the mean age at first birth should decline. The correlation between relative change in cohort size and change in the mean age at first birth is $+0.54$. And since there are close connections between the quantum and tempo of fertility, it is appropriate to calculate the partial of each with the other controlled. The partial correlation of change in cohort size and change in total fertility rate, with change in mean age at first birth controlled, is a statistically nonsignificant -0.13.

The partial correlation of change in cohort size and change in mean age at first birth, with change in total fertility controlled, is a statistically significant +0.36, but substantively weak. (All correlations are for 52 observations.) Note finally that cohort size has been declining for those now in their early 20s, yet the record shows no difference in level and time pattern of their fertility to date. If there were to be an effect of declining cohort size, it should have manifested itself by now, but it has not.

With respect to the theoretical argument, the first point to make is that the economic success of cohorts on entry into the work force depends on much more than their numbers. While those cohorts born in the 1930s did have a favorable labor supply situation, they also had the good fortune to enter the work force at the peak of post–World War II economic recovery. Moreover, they had been socialized during the Depression, and therefore presumably arrived at adulthood with modest expectations, easily fulfilled in the salubrious economic climate. There is no reason to anticipate the same modest expectations for the cohorts born in the 1960s; moreover, it is not unlikely that, far from entering an era of strong economic growth, we should be prepared for a sluggish development for some time to come. Clearly there is no sense in which we may be described as about to emerge from a Great Depression.

A more basic question needs to be addressed. My interpretation of the prevailing reproductive ethos, in the immediate postwar period, is that there were strong normative pressures, of long standing, to marry, to become a parent, and to have at least two children, with the essential proviso that the couple be in an economic position to fulfill their parental obligations. The traditional division of labor by gender prevailed. The primary responsibilities of women were perceived to be domestic. If they took a job, it was generally just for the time being. Moreover, because of wage and job discrimination by gender, the opportunity cost of motherhood was low. Given this normative context, the improvement in the economic status of young adult males was translated into a large rise in the progression ratios for parities zero and one, and a large decline in the mean age of mothers at first birth. Although there is ample evidence that higher income generally implies lower fertility—whether from one country to another, or from one income level to another within a country, or for secular change—it would appear that the improvement in the economic climate in these particular circumstances was strong enough to increase substantially the proportion of couples with sufficient income to meet the economic criteria for responsible parenthood.

How then can we interpret the contemporary reluctance to have two children, or even one child? A mere economic accounting seems insufficient to explain such behavior. The evidence is mounting that there has been a normative transformation. Motherhood is becoming a legitimate question of preference. Women are now entitled to seek rewards from the pursuit of activities other than childrearing. That our species is here today is testimony to past success in the problematic task of population replacement. But our success was probably contingent on the discriminatory treatment of women. It may well be that, by showing respect for the woman who chooses a nonmaternal way of life, we are removing the prop that made possible the past survival of Homo Sapiens. Nevertheless, it seems the just thing to do.

In parallel with the change in relations between men and women has come a major modification in the relations between parents and children. The common themes supporting both directions of change are individualism and egalitarianism. Parenthood is becoming a less rewarding enterprise. The direction of obligation in the intergenerational covenant has been reversed (Ryder, 1983a). Whereas the emphasis was once on what the child owed the parent, it is now on what the parent owes the child. The ties between parent and child have weakened. It matters less to the child's future what the parent does, and less to the parent's future what the child does. It is no wonder that fertility has declined. Perhaps a more compelling question would be what might stop it from falling still further (cf. Caldwell, 1982).

A subsidiary feature of the baby boom was a small increase in the additional children borne by women who already had at least two children. This deserves attention because it reversed temporarily a long steep decline in the measure. Surveys show that the small rise in progression ratios for the higher parities was mostly attributable to unintended births (Ryder, 1982). That happened for two reasons. First, the favorable economic climate encouraged an earlier entry into parenthood, and thus an increased exposure to risk of an unintended child. (The average age at second birth declined, over the course of a single generation, from 27 to 24, meaning three more years of exposure to risk of a third birth.) Second, the rate of unintended fertility rose. In those days, the available means of fertility regulation required high motivation if they were to be successful, because they were all coitus-related. Mere preference did not suffice to provide the spur to careful conduct that had earlier been applied by sheer necessity. But now we are in the era of the contraceptive revolution. The predominant methods of fertility regulation—the oral contraceptive, the intrauterine device, abortion, and sterilization—are all divorced from coitus. For most of the population, the burden of unwanted births has been lifted.

I conclude that there is no reason to anticipate a second baby boom, because the conditions for its occurrence no longer obtain. On the other hand, it would be rash to rule out the possibility of somewhat higher fertility. Despite the continuing improvement in the relative status of women, the cohort total fertility rate seems to be on the verge of ending its decline, and the cohort mean age of fertility on the verge of ending its rise. Perhaps the continuing decline in cohort size will promote some decline in the age of entry into motherhood. Perhaps legal and constitutional decisions will progressively inhibit access to abortion, with the consequence of more unintended births, particularly among the young. Perhaps the composition of the childbearing population will shift in the direction of Hispanic and nonwhite minorities, because of patterns of relative growth and substantial immigration, illegal as well as legal, with aggregate fertility rising somewhat in consequence. And perhaps a reaction may develop to the implications of low fertility, such as a growing concern for the negative aspects of advancing into older ages without progeny. Our grasp of the possibilities in this complex area of human behavior is far too uncertain to inspire much confidence in any conclusion.

How can forecasts be improved? The view has been advanced here that the language of the forecast should correspond more closely with the language of analysis. That means abandoning the period orientation, and describing cohort reproduction as progression from parity to parity, interval by interval. The direction of solution of the technical problems implicit in such recommendations has been sketched above. From a purely demographic standpoint, the cohort fertility tables are inadequate to the task of disentangling the quantum and tempo facets of reproduction, because it is not possible to calculate birth interval length directly. It should, however, be feasible to supply this missing dimension, by using numerators for births by time since previous birth, now available in the annual vital statistics, and denominators from birth histories, collected every five years in the Current Population Survey (US Bureau of the Census, 1982, 1986).

Surveys have provided us with much of the underpinnings of the demographic database, in the form of information about the proximate sources of fertility variation—the ends, means, and conditions. Reproductive intentions apply to tempo as well as quantum. Relative to those intentions, means are used, more or less effectively, to achieve the desired outcome, within a context of the conditions of reproduction, specifically fecundability and sexual intercourse or, as we say euphemistically, exposure to risk. But valuable as these data are, they contribute little to practical solution of the forecasting problem. In large part, their shortcomings stem from the circumstance that the information is customarily obtained through retrospective questions, the topics carry a heavy normative freight, and they are peculiarly prone to change over time. Consequently they are low both in validity and in reliability.

Beyond the instrumental variables, what can fertility surveys tell us about the correlates of fertility in ways that might help the forecaster? It seems to me that two classes of variable dominate the analysis. The first is membership in one or another culture or subculture—people of different religious or ethnic groups tend to have different reproductive patterns. This finding may warrant the partitioning of the assignment into separate forecasts for each subpopulation, but it is a small contribution at best, since these categorical variables are not much more than labels on black boxes. Members of such groups have distinctive reproductive behavior because they have been socialized into adherence to different norms. Surveys are unlikely to help us look inside the black boxes because individuals are inherently incapable of articulating why those norms are what they are, and particularly why they may be changing over time (cf. Ryder, 1983a).

The second class of variable that is prominent in analysis is information about the competitive allocation of women's time, effort, and resources among alternative behavior patterns. It is readily apparent that women now are making different choices among education, work, and motherhood than the previous generation did, but surveys can provide no clue as to why those choices are different. Even if it were possible to come up with a relevant and trustworthy regression equation that had more than trivial explanatory power, it would be less than helpful in the forecasting assignment because the burden would merely be shifted from the lefthand to the righthand side of the equation. The qualifications of demographers as forecasters may be less than impeccable, but they are surely no worse than those of forecasters in other social fields.

Beyond survey data, there is the broad area of speculation associated with the theory of the demographic transition. This appears to offer substantial promise for guidance in forecasting because the central propositions are couched in normative terms, that is, as properties of groups rather than individuals, and the emphasis in the propositions is change through time. Although there is substantial consensus on the broad outlines of the theory, the system of ideas is at best weakly tested, and the hypotheses are no more than directional in content. What the forecaster needs is a specification of how much change over how much time; the transition theorists are justifiably reluctant to accept that challenge. Moreover, when it comes to countries that are below replacement in fertility, like the United States, the theory is moot on their likely future.

It is my view that an adequate model would need to encompass a complex of nondemographic as well as demographic variables, and the latter would be unlikely to occupy center stage. In short, what is called for is a model of societal transformation. It would be sheer hubris for a demographer to take the responsibility for such a daunting task.

In conclusion, the mainstay of the fertility forecast is likely to continue to be the extrapolation of demographic variables, supported somewhat by broad-brush impressions and by judgments of a directional kind. Although there is likely to be some profit in recasting our efforts in the directions proposed above, it would be presumptuous to claim that they guarantee a better forecast. At best, we can avoid some of the errors we have made in the past. We will be well advised to keep our reproductive records up to date, look just a few years into the future, and revise our forecasts frequently. Whenever a forecast is made, the assumptions underlying it should be specified as clearly as possible, and accompanied by a comprehensive defense, in order that we may be in a position, when we fail, to perform a detailed autopsy of the failure. Finally, humility is strongly recommended.

Appendix 1 Components of the mean age of fertility

F is the total fertility rate, and F_i the total fertility rate for birth order i.

A is the mean age of fertility, and A_i the mean age of $_i^{th}$ order fertility.

$$\begin{aligned} FA &= F_1A_1 + F_2A_2 + F_3A_3 + \ldots \\ &= F_1(A_1) + F_2(A_1 + J_1) + F_3(A_1 + J_1 + J_2) + \ldots \qquad \text{where } J_i = A_{i+1} - A_i \\ &= A_1F_{1+} + J_1F_{2+} + J_2F_{3+} + \cdots \end{aligned}$$

R_1 is the i^{th} parity progression ratio. Assume $R_2 = R_3 = \ldots = R$

Then $F_{1+} = R_0(R_1 + 1 - R)/(1 - R)$; $F_{2+} = R_0R_1/(1 - R)$; $F_{3+} = R_0R_1R/(1 - R) \ldots$

Thus $A - A_1 = R_1(J_1 + RJ_2 + R^2J_3 + \ldots)/(R_1 + 1 - R)$

In each interval

$$J_i = A_{i+1} - A_i$$
$$A_i = R_iA_i' + (1 - R_i)A_i''$$

where A_i' and A_i'' are the mean ages of i^{th} order fertility for those who do and those who do not progress to $i + 1^{th}$ order.

$I_i = A_{i+1} - A_i'$ is the birth interval from order i to order $i + 1$.

Thus $J_i = I_i - (1 - R_i) \times (A_i'' - A_i')$

The crucial assumption is that $(A_i'' - A_i') = I_i$.

If so, $J_i = R_iI_i$.

Finally, to estimate an average value of I, let $I_i = I$.

It follows that $A = A_1 + QI$, where $Q = R_1 \times (R_1 + (R^2/(1 - R)))/(R_1 + 1 - R)$

The crucial assumption has been examined, using the birth histories collected in the June 1980 and 1985 Current Population Survey; the approximation is not bad, although it is far from perfect. The most charitable view of the exercise is that the value reported here for interval length is in fact an amalgam of interval length in the legitimate sense of the term, and of the age selectivity of progression $(A_i'' - A_i')$, and these two aspects of tempo cannot be distinguished in the data provided by cohort fertility tables.

Appendix 2 Relation between number of births and total fertility rate in a period

In a conventional forecast, age-specific fertility rates, say $f(a)$, for each period are multiplied by the number of women in each age, say $N(a)$, the product of previous steps in the projection, and the products are summed to give the number of births, B.

$$B = \Sigma N(a) \times f(a)$$

An approximate formula can be obtained by fitting a straight line to the values of $N(a)$, say $c_0 + c_1 a$, and substituting the latter in the above formula. The result is

$$B = F(c_0 + c_1 M_1)$$

where F is the period total fertility rate and M_1 is the period mean age of fertility.

The fit to $N(a)$ is inefficient because it gives equal weight to variations in $N(a)$ at every age, whereas what matters most is a good fit for those ages where fertility is highest. The binomial is a good weighting system, because it approximates the fertility–age function closely, with only one parameter (the value of the mean age). Consider the six quinquennial age groups from ages 15 to 45. The binomial weights are the successive terms in $(q + p)^5$, where $p = (M_1 - 17.5)/25$. If one makes a binomially weighted least squares fit to $N(a)$, the formula becomes

$$B = F \times N/5$$

where N is the binomially weighted average of the six $N(a)$ values. Over a wide range of cases for the United States, this came within one percent of the result of the completely age-specific calculation.

In the work reported here, I actually used a quadratic rather than a linear fit (calling for the second moment of the fertility–age function as well). The increase in order of fit reduces error below that in the basic data.

The result can be used in two ways. (1) The final step in the forecast, the estimation of births, can be achieved from the specified $N(a)$ values, together with the zero, first, and second moments (about the origin) of the period fertility–age function. (2) If we know B, and have estimates of $N(a)$, for the most recent year, we can use the formula to provide an estimate of F, by assuming that the age moments of period fertility correspond to those for the most recent available year.

Note

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