CAN KNOWLEDGE IMPROVE FORECASTS?

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FOREWORD

Low fertility levels in IIASA countries are creating aging populations whose demands for health care and income maintenance (social security) will increase to unprecedented levels, thereby calling forth policies that will promote increased family care and worklife flexibility. The Population Program is examining current patterns of population aging and changing lifestyles in IIASA countries, projecting the needs for health and income support that such patterns are likely to generate during the next several decades, and considering alternative family and employment policies that might reduce the social costs of meeting these needs.

The program is seeking to develop a better understanding of how low fertility and mortality combine to create aging populations, with high demands for health and income maintenance, and reduced family support systems that can provide that maintenance. The research will produce analyses of current demographic patterns in IIASA countries together with an assessment of their probable future societal consequences and impacts on the aging. It will consider the position of the elderly within changing family structures, review national policies that promote an enlarged role for family care, and examine the costs and benefits of alternative systems for encouraging worklife flexibility by transferring income between different periods of life.

Assessments of the societal consequences of tomorrow's elderly populations must begin with population forecasts disaggregated by age. Can the methods used to carry out such forecasts be improved? In this article written at IIASA, Nathan Keyfitz addresses this question and concludes that while current statistical and mathematical forecasting methodology improves our understanding of past trends, the uncertainty of the future continues to pose a challenge to forecasting theorists.

A list of recent IIASA publications on population appears at the end of this reprint.

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Can Knowledge Improve Forecasts?

Nathan Keyfitz

From Auguste Comte onward it has been taken for granted that with more knowledge of how society works would come a greater capacity to predict its future; indeed prediction has often seemed the main object of uncovering the facts and mechanisms of society. Yet rapid increase in knowledge, as presented in scores of journals and hundreds of research monographs, has not paid off in forecasting.

Separation of scholarship and forecasting

In fact there has been a separation of the research that adds to knowledge from writing on the future. With a few exceptions (Bell and Mau 1973), the leading contemporary students of social change have not attempted to forecast for society or for particular subgroups. One kind of forecasting is done not by social scientists, but by journalists and other lay writers, and very few references to the professional literature are to be found in their work. Perhaps the professional literature would be useful for forecasting if only it were seriously applied to that purpose.

The present paper attempts to discover whether there are scientific results that would help forecasting, results that have been neglected. If there are none, what is it in the nature of scholarship that makes it unusable? Surely knowledge is better than ignorance—for forecasting or for any other purpose.

While such an undertaking for the whole of the social sciences would be desirable, this article concerns only a small part of the field: population growth and numbers. I have chosen this area because so much explicit forecasting goes on in it, and because numerical forecasts are easily determined after the event to accord or not with what happened. Judging qualitative forecasts of social structure is more difficult. Does Weber's bureaucracy or Marx's class struggle

provide keys to subsequent history? The difficulty starts with knowing just what Weber or Marx said that can be taken as a forecast and goes on to the even greater problem of deciding just what happened subsequently. No such problems appear in respect of population; certain numbers are forecast and can be compared with actual numbers when a subsequent census is taken. In an earlier article (Keyfitz 1981) I have assembled 1,000 forecasts and compared each with the later realization; the comparisons show ample room for improvement. An objective methodology for the wider field of social science forecasts is worth seeking, but that large task is not undertaken here.

In demography as elsewhere, a knowledgeable forecaster ought to perform better than an ignorant one. And the amount of knowledge that has accumulated in the last two or three decades is great; five major journals and a number of regional ones have printed thousands of articles dealing with every imaginable aspect of population. Many of these articles deal with the relation between population and social change; population and the economy; the way that mortality and fertility are interrelated—all of which would seem to have a bearing on how population will change in the future.

Yet population forecasts, like forecasts in sociology in general, tend to be self-contained; they use available population data and not much else. Ever since Whelpton (1936),[1] the need for considering separately the components of population change—births, deaths, and migration—has been recognized, but each component has been forecast without taking much account of the theoretical or empirical literature (Henry 1972). It is as though forecasters have intellectually isolated themselves from the main body of population studies.

We have in mind less the forecasts of journalists and world modelers than the cautious work of professional demographers. Because these are often employed by official agencies, they protect themselves by calling their work projections. That puts the responsibility for applying them as though they were forecasts where it belongs—with the user.

The projections are often said to be conditional, the arithmetical working out of the assumptions made. Yet anyone who has tried to proceed arithmetically from the assumptions to the projections will find the way blocked at the start. Sometimes the assumptions are not stated at all, sometimes they are stated in general and approximate terms. Data adequate to reproduce the projections, which are said to be merely the working out of the consequences of the assumptions, are invariably lacking.

It is because the projections are used as forecasts, and because the conditions on which they are said to be based are so little emphasized or even stated by their authors, that I am here taking them as forecasts. Often variants are given—high and low as well as medium—but without any indication of the probability that the outcome will be straddled by the high and low projections. Without information on whether the chance is 50 percent or 99.9 percent they do not add much to the middle variant that is ordinarily used as the forecast.

Our examination of possibly relevant theory begins with fertility, future births being the most problematic part of population forecasts.

The fertility transition

The now standard way of looking at the evolution of fertility over the past century is known as the demographic transition (Notestein 1945). Put in the simplest terms it says that with modernization first the death rate falls, then after an interval the birth rate. There are exceptions of course; in France the birth and death rates fell early and concurrently, in a slow decline through the nineteenth century. The logical implications of the demographic transition are unclear. Is it an account only of what happened in the past in Europe and the United States, or can it be relied on for the future? Does mortality decline cause fertility decline, or merely happen to precede it in the particular cases on record? Even if we can be sure that the transition will occur everywhere, we do not know the all-important time interval that elapses between the start and the end. A main issue of controversy is whether that interval—the number of years after the mortality level improves before the birth rate declines substantially—is in some sense determined by per capita income, or is relatively independent of income and is influenced by education, urbanization, and availability of contraceptives.

For many of today's developed countries the long-term decline of fertility began about 1870 and continued without interruption until 1935. During this time the forecasting of fertility was straightforward. Until some ending came into view, it seemed safe to suppose that last year's decline would be repeated this year and next. The decline seemed to be due to the pressure for upward mobility—social capillarity as Arsène Dumont (1890) called it. A couple were ambitious for their children, whose social ascent depended on the quality of upbringing, and especially on formal education; education was expensive in the nineteenth century and could typically be afforded for only a few children. Parents preferred to have one or two children who would rise in the world to half a dozen who would remain poor. The mechanism seemed one that would persist and could be counted on almost indefinitely. Social mobility may well correlate with fertility decline across societies, even though Westoff et al. (1963) did not find such a correlation among families within American society.

Whatever the nature of the motivation for diminished childbearing, its strength cannot be doubted. When the instruments of contraception were hardly known, withdrawal sufficed for Europeans to hold down the number of children to those they could provide with property, or could afford to educate for a career.

So much for fertility within marriage. Perhaps the main way that Europeans restricted their fertility was by deferring marriage. John Hajnal (1965) shows that at least since the eighteenth century late marriage prevailed throughout Western Europe, with the average age of marriage for females rising to 23 or older, and at least 10 percent of women simply remaining unmarried toward the end of the century. Ansley Coale (1978) points out that it was more than reasonable for Malthus to recommend late marriage as the ideal means of fertility control, for it was the method that was being successfully practiced before

his eyes. Susan Watkins (1981) has studied the late phases of Europe's marriage changes, and shows how regional differences persist in their traditional patterns, even though there is a degree of convergence. Le Bras and Todd (1981) also show the persistence of local differences in this as in other demographic matters.

Thinking of the idea of social mobility as an instrument for forecasting is like turning a searchlight on it. Social mobility indeed helps an observer to understand the way people act in their jobs and how they make childbearing decisions, but what else is it able to do? Any further use of the idea of mobility would require that we have some way of measuring its intensity in the individual without resorting to his fertility as a measure of it. If we had a statistical series showing how personal ambition changes over the years, then we could objectively test the hypothesis that ambition is a driving force in the demographic transition; and, if the hypothesis is confirmed, we could use estimates of future ambition to predict future childbearing. Merely to state the problem is to show that the idea of social mobility does not really do much beyond giving one the subjective feeling that he understands a historical process.

Kingsley Davis (1963) suggests that the problem of too many people appears to individual members of the population not as calling for contraception alone, but rather for a number of functionally equivalent responses. These include alternative means of limiting population growth—abortion, deferred marriage, rural to urban migration, migration abroad. The collective problem of overpopulation is transmitted to individual families, and their responses are such as to bring about a degree of homeostasis. One can think of the large system of the nation, or, indeed, of a whole continent, as being linked to the small system of the family, so that the latter is responsive to imbalances in the former. Signals can be passed downward. For example, a shortage of land in a system of private ownership can constitute a signal. Tilly (1978) found that agricultural areas in Europe of the early industrial period contained much smaller families than areas of incipient industrialization. Where shortage of land to inherit inhibits marriage, fertility is held under control; once jobs, even low-paying ones in artisanal industry, are available to men and women they marry and have children. No one quite knows whether some similar relation applies to the present-day developing countries.

The relation of the larger system—the country or the world—to the smaller decision-making group—the couple—is a perpetual problem for demography. In what times and places is there homeostasis, so that population pressure in the larger system is answered by restraint in the smaller group? With such negative feedback comes stability. In the mechanism expounded by Rose Frisch (1978), fecundity falls in times of food shortage, resulting in fewer births. Were such a mechanism operative—and many dispute it—it would provide the kind of negative feedback conducive to stability. But we know that in some times and places poverty begets reproduction, which makes for more poverty, a destabilizing positive feedback. If we knew more about the relations

between the two systems, large and small, forecasting would be facilitated. As we have seen, the relation of childbearing to social psychological states like ambition or mobility is even more problematic.

Heterogeneity of underlying causes

During the 1940s and subsequently Western populations began to grow rapidly through natural increase. But this upswing lasted for only two decades, until in the 1960s the downtrend resumed. Was the resumption of the downtrend the renewed effect of the same causes as had operated up to 1935? Apparently not, argues Philippe Ariès (1980). The earlier fall in fertility represented a positive interest in children. The interest showed itself not in having many, but in the welfare and the progress up the social scale of the few that one did have. That attitude has altered in recent years—social mobility is not the engine of progress that it was in the nineteenth and the early twentieth centuries. Today it is hard for us even to imagine the degree of ambition for social advance that drove our parents and grandparents.

Instead a new motive has come into view, an extension of the individualism that had been developing in Europe during the age of romanticism. People now search for a personal identity; they are less inclined to pattern their lives on social prescriptions. And seeking to do their own thing means several kinds of change: women want jobs, careers, their own sources of income—in short, scope for expanding their own personalities. Corresponding changes are taking place in men. For adults of both sexes children become a handicap to personal expansion if the marriage continues and to a job or remarriage if the marriage breaks up.

Low fertility seems here to stay. A reversal of all aspects of the change that goes by the name of women's liberation is unimaginable, and since the several aspects seem to be related, even less can we envisage a change in one without the others also changing. Those who do not see women quitting their jobs and rediscovering the virtues of domesticity, subordination to a husband, and prolific childbearing will produce projections for developed countries with continued low births.

What does this mean for the long-term future? Bourgeois-Pichat (1979), Easterlin (1973), Clark (1980), Spengler (1978), and other writers see fluctuations in fertility. We will have a stationary population over the long run, but with periodic ups and downs. The swings will presumably be imposed on a population that over time just replaces itself. It has been thoughtlessly assumed that the limits of the natural world, which certainly impose long-run stationarity, also impose constant births and deaths, a conclusion that hardly follows. Yet how can one forecast the waves that, as the postwar experience shows, can be of substantial amplitude and irregular length? The Lotka theory, based on constant age-specific births and deaths imposed on an initially nonstable age distribution, would give waves of one generation in length. We will

look later at the economic theory of fertility, from one version of which waves of two generations in length are deduced. To use such theory for forecasting would require relatively exact knowledge of wave lengths and amplitudes.

First effects of development

For developing countries some increase in fertility tends to occur on the eve of modernization. This can be due to improved health and so higher fecundity, earlier and more nearly universal marriage, increased illegitimacy, and the substitution of bottlefeeding for breastfeeding. Modernization causes the abandonment of various traditional practices that curtail fertility, including postpartum sexual abstinence, common in traditional African societies and elsewhere. Anatole Romaniuk (1980) offers a useful discussion of such matters in application to a rise in fertility in the early stages of modernization in Zaire. In a later paper he shows that the same applies to the Canadian Indians, and the fertility rise appears to be due to the abrupt shift from prolonged breastfeeding to bottlefeeding that took place prior to the large-scale practice of birth control (Romaniuk 1981).

But timing is the problem here, as with the subsequent phases of the demographic transition. The thesis seems correct as an explanation of the past, which is all that it claims to be, but because of the unknown time scale, forecasters cannot easily apply it.

Direct and indirect effects of development

The relation between the later stages of development and fertility has been studied by many authors. The work of Heer (1968) can be taken as representative. Heer found that the direct effect of development is increased literacy and declining infant mortality, and that these lower fertility. The study has been subject to some criticism, especially because it used cross-sectional material to infer longitudinal effects, but its conclusion seems to be consistent with other results, and we may suppose here that it is correct.[2]

If it is correct we ought to be able to predict fertility change by predicting literacy and infant mortality. But we run up against the same difficulty as elsewhere: predicting changes in literacy and infant mortality is not easy. Moreover, the response to literacy is probably nonlinear; somewhere there may be a threshold, so that it takes a good deal of change in these independent variables to bring about the change in fertility. But the threshold is difficult to estimate. This makes the timing of the effect hard to state on the basis of existing theory and data.

Mexico illustrates the problem.[3] It has appeared that the persistence of high fertility in Mexico, at least until very recently, did not accord with its relatively high income, education, and urbanization. Kirk did indeed find some regularity, and demonstrated that the transition proceeds increasingly quickly in the present period; what took 60 years for the United States (say, 1870 to

1930) could well occur in a third of that time today. But when it will occur is still not statable.

Education and fertility

Much progress is being made throughout the Third World in the spread of education. Even countries whose per capita incomes remain low are building schools for primary, secondary, and higher education, and are moving quickly toward the often mentioned goal of wiping out illiteracy. On the whole they are able to outdistance population increase, so the percentage of successive cohorts that attends school is growing. This spread of education seems bound to affect fertility independently of the growth of income. Barbara Janowitz (1976) lists the cross-national studies, most of which show negative correlations between fertility and education, then goes on to argue for both a direct and an indirect effect through labor force participation and age at marriage.

As Caldwell (1980) puts it, there are five ways in which education acts:

1 It reduces the child's potential for work inside and outside the home, not only because of the absence of the child from the family while at school, but also because traditional tasks come to seem both to him and to his parents unfitting for a person of his status.

2 It increases the cost of children for fees, uniforms, and stationery, for extras that will enable the child to participate equally with other children, for new kinds of food and other unfamiliar expenditures supported by the authority of the school and sought by the child.

3 While he thus develops initiatives and becomes an independent moral force in the family, the child's status as a material dependent in the family is emphasized by the school. Laws to protect the child, including protection against the pressures of parents, are passed simultaneously with plans for universal education.

4 Schooling speeds up cultural change and imposes middle-class values; these include having few children but ones in whom heavy investment is made. One can talk about having schools that will not strain the social fabric, that will accept peasant values and prepare the child for the peasant society in which he will have to live, but the fact is that the teacher, almost always possessed of middle-class aspirations, is hardly likely to strengthen values that are opposed to his own.

5 More specifically, in the contemporary world the school serves as the means for propagating values of the Western middle class. These include the view that girls are as important as boys and just as much to be educated, that white-collar work is better than farming, and that certain kinds of food, clothing, and housing are essential.

School textbooks make little mention of traditional practices, rather supposing a modern (i.e., Western) way of life. And the main message of the

schools is not written down in the textbooks. An unspoken thesis is accepted by parents, teachers, and pupils: a new way of life has been discovered that is at variance with tradition, and it is better than the traditional way. The school weakens the subordination of the young to the old, and in this way helps to weaken the family as an economic unit.

The new sort of person produced by the schools demands more from the older generation and is less an asset to that generation. Caldwell (1977) has shown how the traditional society's way of using the young to add to the income and wealth of the old pushes couples to build large families. It is the reversal of the traditional flow of wealth, whereby the young become an expense rather than a source of material gain, that is responsible for fertility decline.

In summary, according to the thesis developed by Caldwell, the flow of wealth upward from the younger to older generations makes for large families, and the flow downward for small ones. The modern expansion of education has an important role in reversing the flow. The thesis concerning the direction of the flow of wealth is convincing, and adds to our understanding of the demographic transition. I know of no reference to it in the forecasting literature.

Urbanization

The cities of the poor countries have increased enormously in population. This is shown by the projections of the United Nations and discussed in some detail by Blayo (1976). Between 1950 and 1970 the populations of cities in the developing countries grew by an average of 4.4 percent per year, while the growth of their rural population was only 1.6 percent. By 1975 the urban population averaged 27 percent of the total in the developing countries, much more in Latin America, less in Africa. How does urban growth affect fertility?

It is argued that people only come to the city because land shortage prevents them from earning a living in the countryside; those immigrants to the city who live in shantytowns, and have not put their hand on the lowest rung of the ladder of upward mobility, are in the same primitive condition as they were before moving, and are cut off from the genuine urban life that would make them truly modern. In this situation there is no reason to expect them to curtail their childbearing.

In Europe and the United States urban fertility was typically lower than rural, and part of the way that the demographic transition operated was through the rural exodus that lowered fertility. No such clearcut relation between migration and fertility applies around the world today. In some instances the fertility of the urban migrants remains as high as that in the countryside from which they came. In other instances there is some difference between the two levels of fertility, but this may be due to simple selection, and if so the move has no effect on overall fertility. Recent work reported by Sidney and Alice Goldstein (1981) found that the current fertility of migrants was consistently

higher than their own earlier fertility and higher than that of nonmigrants in urban areas. On the other hand, the past fertility of migrants was lower than that of nonmigrants. The combination suggests both a selective effect and a direct influence of migration on childbearing, again a proposition useful for understanding city populations but not for forecasting them without some measure of the speed of the migration process and the speed of fertility decline.

Income equality and fertility

If fertility were a simple function of average income, and income were rising at a known rate, we would have an effective forecasting machine. In fact the relation between income and fertility is not simple. The crudity of forecasting fertility from aggregate measures like income per head or energy consumption per head is pointed out by Repetto (1974), Bhattacharyya (1975), and others, who claim that the *distribution* of income has a decided effect. Comparison of China and Brazil suggests that the mean and dispersion of income distribution may well act independently. The dispersion can be taken as merely the difference between rural and urban averages, but the more sophisticated index developed by Kuznets (1975) would seem more appropriate.

Repetto provides evidence on the effect of income equality in inducing fertility decline, mostly in the form of cross-sectional regressions among countries in which disturbing variables are held constant. The point may have a degree of validity, although it is not easily integrated with alternative economic hypotheses, or with psychological, sociological, and other mechanisms.

The primordial model: population presses on land

Malthus did not set out to forecast population, being content to urge restraint in childbearing, which is to say, applying the preventive check in order to forestall nature's application of the positive check, an increase of mortality. His model relates to policy rather than to forecasting. Sometimes, especially in the first edition of his Essay, Malthus seems pessimistic enough about people's ability to apply the preventive check that one can read a forecast of misery into his writing, but at other times, and increasingly as he grew older, he seems hopeful that with the advance of education people will indeed restrain their childbearing and climb above mere physiological subsistence.

If Malthus himself was not able to use his model for forecasting, it would be rash for us so to use it, especially now that a century and a half have gone by, during which much more sophisticated models have appeared. Malthus provides no assistance in forecasting because his writing on population, when read in context, is conditional. One may think otherwise after reading the first few pages of the first Essay, but in effect he is always saying that if people behave in a certain way—which he would deplore their doing—then certain

bad consequences will follow. Malthus's model and all his writings were oriented to policy, and only on superficial reading does he appear to be making predictions.

Human capital

A distinct way of viewing fertility change is provided by the human capital school.[4] It has long been noted that a more highly trained labor force is the source of much, perhaps most, economic advance. The human capital school adopts this proposition and incorporates it in a model in which the household is the key decision-making unit. The household has a production function whose most important argument is the time of its members. When wages of male workers are much higher than those of females, the wife will look after the home and the husband will have a job. As wages come closer to equality the wife can use her time more effectively, can gain a greater utility by herself taking a job. In doing so she has weighed the satisfactions of the labor market against those of the home and found the former greater.

Supposing that the link of fertility to the price of time is correct and quantifiable, how might it help forecasting? Presumably only to the degree in which the value of time can be forecast. But however elusive future population may be, the future value of economic variables is more so. Nonetheless it has often been said that models developed to represent the entire economy cannot leave out population, and one should take population as an endogenous variable, so that it would be determined by the system of equations in the model, just as are interest rates or employment, rather than being imposed from the outside. What people say in this regard is different from what they do; until the equations relating population to economic variables are known more precisely, the gains from endogenizing population will be difficult to secure.

The Easterlin effect

An alternative economic model to explain the change of fertility in the later phases of the demographic transition is provided by Easterlin (1968 and subsequent papers). He emphasizes that a family's economic status relative to its aspirations, and relative to its parents' incomes, is decisive for its fertility performance. Average income for the United States as a whole was higher in the 1960s than in the 1950s but fertility was lower. Thus average income for people of all ages could not explain the baby boom of the 1950s. But relative income could. The young adults of the 1950s were a small generation, being the cohorts born in the 1930s, and they were correspondingly advantaged. They were able to enter college without great competition for places; they had no trouble finding a first job, since there is a certain age-complementarity in production, and young people are needed for certain work; and as they were followed by more numerous younger cohorts they were quickly pushed up into positions of supervisor, teacher, and so on. Thus at every stage of their careers they found

themselves relatively advantaged. The resultant optimism encouraged them to have more children. This is the most convincing explanation of the baby boom that we have.

From it follows the obverse: the people born in the 1950s, being a large cohort, would have all the disadvantages that correspond to the advantages of small cohorts. If the reverse cause works in the reverse direction, they would tend to have few children, and that is an explanation of the low birth rate in the 1970s. The test will come later in the 1980s, when the birth rate ought to rise as the small cohorts of the 1960s decide to have many children. There has already been some rise of fertility in the United States and other Western countries, but the degree to which it will persist remains to be seen.

Paul Samuelson (1976) elaborates the Easterlin model in a nonlinear form that does not possess the property of proceeding asymptotically to stability, a feature that contrasts with the familiar ergodic properties of linear models. The Easterlin model has influenced some forecasts, and the US Bureau of the Census is experimenting with it. Ronald Lee (1976) discusses the matter and presents some hopeful considerations.

Accepting that the Easterlin effect really exists, which I firmly believe, the question is whether it will show itself in actual birth performance beyond the single case of the baby boom of the 1950s. Could it not in the future be swamped by some other mechanism, for instance the movement of women into the labor market, so that even though fully operative it would not be evident in the actual birth series?

What makes forecasting genuinely difficult is the operation of mechanisms that are competing below the surface of demographic phenomena. I do not refer to the competition of opposed academic theories, which we also have, but to a genuine, albeit hidden, operation of different and opposed causes, with sometimes one cause emerging to the surface, sometimes another.

Notice that the "new home economics" (Becker 1960) explains fertility decline primarily by the rise in the value of women's time, as measured by wages for those women who work. For Easterlin the wages and employment possibilities of husbands have a positive bearing on fertility, while for Becker it is the wages of wives that count, and they are a negative factor. Throughout the economic cycle wages of husbands and wives rise and fall together, so we need to know which it is that counts. The fertility statistics of the 1980s will tell us more about this matter, and demographers are watching them from month to month. We have here competing theories, and we cannot be ready to apply them to forecasting until we have decided which effect is stronger. A fine recent account of the economics of population is presented by T. Paul Schultz (1981).

Opportunity cost

The relation between the economy and fertility can be expressed in terms of opportunity cost; women will engage in less childbearing the more they are

paid for their time outside the home. This hardly explains why in good times fertility rises, an unquestioned feature of the economic cycle. But it does help us to understand why women took jobs in greater numbers in the 1960s and correspondingly had fewer children than, say, in the 1920s; the economy had changed so that there was a demand for their services. In addition, the work had become much less unpleasant than the typical factory job that was a main possibility for women in earlier times.

Prosperity and fertility

That the birth rate declines in periods of depression and rises in times of prosperity is illustrated by US and European history, although demographers point out that the amount of rise and fall varies considerably. In a French survey conducted by the Institut National d'Etudes Démographiques (Girard and Roussel 1979), 78 percent of the public considered that uncertainty of employment would cause couples to defer having a child that they wanted; 63 percent that it would delay marriage; and 30 percent even thought that it would cause parents to give up the idea of having a child altogether. That the public "knows" this helps to bring the condition about. Let us disregard any complexity in the relation, and suppose simply what the public supposes—that fertility varies in a simple way with the economic cycle. Can that relationship help forecasting?

Not easily, for in order to say what the future population will be we would have to know how the economy will behave. To rely on this is to try to solve a relatively easy question by first answering a more difficult one. What the oscillations of the economy will be over the next 20 or 50 years is much less knowable than what the population will be. The usual economic forecasts made for three months or one year ahead do not take us far.

An alternative explanation of post-transition waves

Jean Bourgeois-Pichat (1979) reviews the prospects for an increase in European fertility that statistics of births since about 1978 suggest may be starting. He cites in regard to the possibility of waves in the post-transition phase, beyond the writers mentioned here, a thesis proposed by John Grauman. Individuals form their impression of the desirability of few or many children in their own childhood; a person who was brought up in a family of two children sees child upbringing as no very arduous task, and is in his turn willing to go further and have three or four. On the other hand, one who was brought up in a household of three or four children could see the drawbacks of so large a number. Such a psychological mechanism produces waves of two generations in length, and in this respect resembles Easterlin's relative income mechanism. If two-generation waves indeed appear, is there any way of making the data tell us whether these were produced by the Easterlin or the Grauman mechanism? We ask the

question to suggest that very different kinds of mechanisms may be equivalent for forecasting purposes.

Subgroups

A natural way of forecasting is to take account of subgroups in a population and to suppose that their shifting relative numbers will determine overall fertility. Changing fertility is the result of the variation in the mix of the several groups in the population through time. If Hispanics have higher fertility than English speakers and they are increasing as a fraction of the population, then the fertility rate will rise on that account. Such a notion is hardly entitled to be called a "theory" of fertility, and yet it underlies a certain amount of demographic work.

The case of religion shows how far afield one can go in applying this assumption. For some time Catholic fertility was higher than Protestant. Yet to assume the persistence of that differential into the present generation would be simply wrong (Westoff and Jones 1979). Starting from a situation in which Catholic fertility was very little higher than non-Catholic, the differential increased markedly during the baby boom, and then declined to the point where the two nearly come together in the mid-1970s. To count on fixed rates holding among any of the set of subgroups is a thin reed to which to tie a forecast.

Childbearing intentions

Perhaps the theories that we have discussed are too remote and general, a feature that lessens their value for forecasting quite apart from whether they are true or not. We may well need auxiliary data that come closer to the subject, and these may be provided by specially designed surveys.

Around 1940 it occurred to several students of population, who saw how difficult it was to forecast from available demographic series alone, that a new kind of information might lead to a higher order of accuracy. If one wants to know how many children will be born in the years ahead, can one not just ask the women who will be the mothers what their intentions are? And so began a long line of investigations, of which the first and most famous was the Indianapolis Survey, in which married women were asked a series of questions to elicit their plans for childbearing. Today, after 40 years of effort, this instrument has come to seem uncertain, even controversial (Hendershot and Placek 1981), although it has been incorporated in the procedures of the US Bureau of the Census for official US forecasts.

Most of the surveys have been confined to married women. To extend the questioning to women who are not yet married is not likely to add much information; a girl of 15 can hardly give a meaningful answer to the question of how many children she intends to have. Presumably the intentions develop in the course of interaction between spouses, and even within marriage they change over time. The difficulty is that most children are born within a few

years of marriage, usually less than a decade. In a typical case a woman marries at 23 and has whatever number of children she is likely to have by 30. Thus the intentions would at best tell something about fertility for a decade ahead. And demographers are not especially interested in short-term forecasts; their public calls for forecasts 20, 50, even 100 years ahead.

But even within the short term the surveys have not been able to anticipate turning points. (Turning points are what count; insofar as the curve of births follows a clear and smooth trend we do well enough with extrapolation, and there is no need for expensive surveys of intentions.) A change in the economic or social configuration seems to cause couples to change their minds. Their expression of intentions is contingent. Put another way, their intentions imply a forecast of economic conditions, perhaps the assumption that those conditions will remain as they are at the time of the questioning. Insofar as this is so, and insofar as we have better ways of forecasting the economy than asking young married women, we need at least to interpret data on childbearing intentions in a more sophisticated way.

Ronald Lee (1974) proposes such an interpretation. He would not splice the intentions data onto the actual birth series, but would consider the intentions themselves as a series, and draw conclusions from the way they change.

A more thoroughgoing change in the procedure is proposed by Clyde and Lolagene Coombs (Coombs et al. 1975). They do not stop with one question on number of children intended, but seek the entire preference scale of the couple, with respect both to sex and to number of children, since these are not independent. The woman may say she intends to have two children; she is then asked whether she would prefer one child as against three, and so on. A map of the entire preference set is more informative than the single number representing its maximum value.

This article, studying the relation of knowledge to forecasting, has confined itself to the knowledge side and has not discussed forecasting at any length, beyond the asssertion that the level of theory here taken up is not mentioned by forecasters. I have not seen many such references in the published official forecasts, and even less do they appear in the innumerable unpublished consultants' memoranda and reports that constitute the largest part of the total forecasting activity.

Yet some account of how forecasting is done, and might be done, with emphasis on numbers, is here required.

Statistical methods

Should one abandon the effort to bring demographic knowledge and theory into forecasting work and simply use statistical methods? Some experiments suggest this. Lee (1974) applies the technique of Box and Jenkins (1970), based on analysis of the internal structure of a time series, to a series of births and finds a good fit. The Box and Jenkins method starts with calculation of the serial correlations, and assumes that these are constant through time. The work of

Lee, as well as of McDonald (1979), shows that under certain circumstances the results of this method are superior to those using more conventional demographic methods. This is also the conclusion reached by Saboia (1977).

But we must not take for granted that statistical methods, of Box-Jenkins or other description, can be applied to birth series. The constant underlying structure assumed does not persist long enough in demographic data for the fitting to be effective. This is why Lee argues only for provisional acceptance of the method into the demographer's tool kit.

Mathematical methods

The use of curves for extrapolation is the classic method of forecasting. Some populations look as though they were growing exponentially, and indeed for the very short term simply applying the past ratio of increase does not do badly. Once it was thoroughly understood that exponential growth over a long period is impossible, demographers resorted to logistic growth,[5] which at least recognized a limit on the possible size of the population. One of the objections to such methods is that they deal with the population as a whole, instead of considering the separate components of birth, death, and migration, and that they disregard age.

Work by Brass[6] over the past decade has provided by far the most effective examples of what I am here calling mathematical methods. His relational approach, first developed for mortality and then extended to fertility and to marriage, transforms age-specific rates in order to make their variation with age more tractable, indeed brings it close to a straight line, and then sees how the parameters of this straight line have been changing over time. The approach is a powerful one, as has been shown in extensive applications to populations on all continents.

Although not developed specifically for forecasting, the method of Coale and McNeil (1972) for dealing with nuptiality has potential in this direction. The model supposes that women reach a marriageable age and then are subject to delays, random in length, corresponding to meeting a future husband, becoming engaged, and getting married. If the delays are suitably distributed, Coale and McNeil show, one ends up with a three-parameter curve that is a good fit to the distribution of ages at marriage.

If one is frankly proceeding by extrapolation, then the crucial question becomes what elements descriptive of mortality or fertility one ought to extrapolate by. If one were to extrapolate the age-specific death rates, age by age, using virtually any formula, one would obtain highly irregular rates within a very few cycles of projection. On a straight-line projection age by age, many ages would soon show negative death rates. One plainly ought to summarize the rates into some minimum parameter set, and this is what Brass did in initiating his promising approach. An alternative procedure was employed some time ago by Ledermann and Bréas (1959), who found that two or three dimensions adequately describe the underlying data set. Their work has re-

cently been applied to forecasting by Le Bras and Tapinos (1979). They used three factors, expectation of life at birth, juvenile mortality (death rate for the age interval 10–25), and mature mortality (death rate for the age interval 50–75). These accounted for 97 percent of the variance of the logarithms of the age-specific death rates.

Distinguishing between a behavioral model and extrapolation is not as straightforward as the words suggest. The logistic curve is behavioral, in the sense that a story can be attached to it: populations increase rapidly when they are a negligible element in their environment; as they increase and take up appreciable portions of the resources available their growth slows; when they come close to the ceiling above which there are no resources left for their expansion, they gradually slow to a zero rate of increase. Raymond Pearl (1924; Pearl and Gould 1936) told the story at much greater length. One could safely count on moving forward on the logistic curve as long as resources remained constant; when resources took a jump, population moved up to a higher curve. The American Indians moved along a logistic suited to their manner of exploiting the environment; industrial civilization moves on a much higher logistic for the same territory. Experiments with fruit flies having limited food and contained in bottles seemed to verify the mechanism that the logistic described.

Yet is this account of behavior more than a description of the mathematical curve? How much does the description increase our confidence that the curve will be followed in the future? Perhaps it does subjectively, but in the case of the logistic there is little evidence that the curve following this rationale fits better to historical series than the cumulative normal, which resembles it enough that one could not tell the difference with the naked eye. Does the story really help when, as it turns out, other curves to which no story is attachable fit population series almost as well as does the logistic?

The recalcitrance of the data, their unwillingness to distinguish between theories, is one of the frustrations of empirical study that demography shares with other fields. The problem emerges in extreme form when we fit a logistic, which on the face of it is a possible trajectory, and compare it with a fitted hyperbola, which cannot possibly represent the future evolution of the population, since it goes off to infinity within a generation or two.

Discussion

In this review of the most prominent theories of population growth we have found far too little that helps in the day-to-day work of the forecaster. There seem to be at least six reasons why perfectly valid research that produces an important theoretical relation cannot be usefully incorporated in forecasts.

1 Much theory is deliberately cast in the form of comparative statics and provides conditional results that are important, but these are neither intended nor usable for prediction, which requires dynamic and unconditional

models. Data are often assembled in the form of cross-sectional comparisons, while what the forecaster is tied to is longitudinal sequences, which can be very different. The comparative statics models are true, given their conditions, but they cannot be separated from those conditions; the empirical cross-sectional results are true for the material from which they were derived, but cannot be counted on for other times and places, and certainly not for future times.

Many studies have been of the form "If education increases, then after a certain point, everything else remaining constant, fertility will fall." That is a valuable kind of knowledge, even though it is not very helpful for forecasting: we know that everything else will not remain the same.

2 The theory may be irrefutably true, either because it is a logical relation or because it is based on incontrovertible data and relevant to the future, and still its effect may be swamped by other relations. The Easterlin effect does exist: couples do tend to have more children when their cohort is small and their financial position is relatively good. We have always known that in times of economic depression births are postponed, and some are never made up, and that in times of prosperity births increase. The Easterlin hypothesis specializes this to couples of childbearing age, and takes their incomes not absolutely but in relation to the incomes of their parents. The effect did appear in the baby boom of the 1950s. But since the 1960s a force has arisen that opposes this effect, which, for brevity's sake, can be called women's liberation. If women want to work, and have their importance in the world outside the confines of the family as men have always had, then they will have fewer children. The Easterlin effect is still there, but it is masked by the liberation effect. A few more years into the 1980s will tell us which of the two effects dominates. Meanwhile the forecaster does not know which one to trust, yet they point to very different numbers of births.

3 The theory may be true and relevant and not opposed by contrary effects, but its timing may be obscure. Thus in present poor countries, births will certainly fall once deaths have fallen, but how can we tell whether they will fall in five years or in 30 years? If in 30 years, the subsequent population could be more than twice as great as if the fall occurred in five years; the effect of timing is not negligible.

4 The theory may relate fertility to other variables in a perfectly valid way, but we have no means of forecasting those other variables. Thus, even if we could be certain that equality in a society contributes to its capacity to reduce its birth rate, and that in the later phases of development equality increases, we still could not easily forecast how rapidly this would happen. We know that if women in poor countries stop using packaged formulas and nurse their babies, the birth rate will fall, but we have no way of knowing when the change in infant feeding will occur. For advanced countries we may conjecture that there will be a rise in fertility once the present recession lifts, but we do not know when the recession will lift. Without some indication of the timing, the relation is interesting to work out but not usable for forecasting.

5 Even if we know the timing, the relation can change unpredictably.

The end of the demographic transition in the 1930s gave way to the baby boom of the 1940s and 1950s. And after that, as Ariès showed, nineteenth-century "familism" gave way to late twentieth-century individualism; and from the 1960s onward people came to be concerned with their personal identities, in which children fit less well. No one can say now whether there will be a return to the past in this respect, or a change in some wholly new direction.

6 Finally, everything about the relation may be known, but of too short term for the forecasting intervals that are of greatest interest. Economic forecasts of next year's income will help only a little, and women's statements about their childbearing intentions not much more. Forecasts of weather and earthquakes, where the next few hours are the subject of interest, and of unemployment, where the next year or two is what counts, are difficult enough. Population forecasts, where one peers a generation or two ahead, are even more difficult.

The scholarly literature aims at understanding

Much of the literature is designed to help us to understand past trends and developments. Caldwell provides a clear idea of the way education and the flow of wealth contribute to the fall of the birth rate. Dumont's social mobility (called by him capillarity) helps us to understand the fertility decline up to World War II, and Ariès's (1980) liberation effect accounts for the current decline to well below replacement fertility. The Harris and Todaro (Todaro, 1976; Rempel, 1981) lottery for city jobs gives a rational explanation of why people migrate to the city and why they remain there even when they find no jobs. Like the economist's opportunity cost of women's time, these hypotheses are intended to give certitude to our thinking about qualitative aspects of demographic change. That is a different order of knowledge from what the forecaster requires.

To ask of an article in this or any other professional journal how much help it offers in forecasting the future is undoubtedly too severe as a general criterion. At best we will take a very long time to build up the stock of knowledge required. Policy advice can be offered on the basis of a different and easier kind of knowledge, which might be called conditional. It answers such a question as, if everything remains the same, what would be the effect of lowering the age at which people can start to draw social security. Or if everything (say in India) remains the same, what would be the effect of raising the minimum age of marriage? These questions can be answered satisfactorily (indeed answering them is what the demographer does best) without saying what will actually happen in the future.

At one time demographers thought that they could get away with such conditional forecasts for future population, but now they realize that their public wants to know what will really happen. Perhaps the user is a government

concerned with the solvency of its highway fund, and it wants to know the size and distribution of population over the next generation. A telephone company has a similar interest. A toy manufacturer wants to know the number of children over a decade to come. The financial authorities try to anticipate the condition of the Social Security and Medicare funds. To such users a conditional answer is an evasion. They have no way of dealing with it in the models they construct for their particular purposes.

For policy purposes causal knowledge is essential; for forecasting it is desirable, of course, but the forecast is not necessarily a failure if the causal mechanism remains undiscovered. Observed regularities serve perfectly well for forecasting as long as they continue to hold, and such successes as there have been in forecasting are based on observed regularities. It is not necessary to understand everything that has happened in the past to extrapolate the relevant series into the future. Statistical and mathematical methods applied to the birth, death, and migration series, making no reference to other series or to behavior, up to a point circumvent the need for causal understanding.

What emerges from all this is the realization that forecasting is more problematical than demographic research, which tends to center on understanding the past—no mean task in itself—and on inferring conditional causal relations that are useful for policy advice. We have found few instances in which understanding of the past and the conditional relations can be brought to bear on forecasting.

Forecasting is difficult, and no one should pretend to develop "methods" for doing it. A method that would take account of future wars, plagues, and prosperity is no more likely to be discovered than a method for changing the earth's orbit. What we can seek with some hope of success is a statistical procedure that will marginally reduce the error made in past forecasts.

Thus at the end of this lengthy search we are driven back to statistical and mathematical methods that in one form or another, since they do not depend on outside knowledge or relations beyond the demographic series themselves, can only be called extrapolatory. Pending the discovery of a truly behavioral way of estimating the future, we cannot afford to be ashamed of extrapolating the observed regularities of the past.

Knowledge in the field of population is evidently not directly usable in forecasting. Does it follow that an ignorant person will do as well as a highly trained one? After all, the methods of statistical forecasting were not developed with population in view, and to apply them calls for no special knowledge of what makes the birth rate rise and fall.

Yet in fact the most trustworthy forecasts are carried out by demographers, who follow the population literature closely. We trust their results because at the many points where judgment enters, the demographer's intuition has been sharpened by exposure to the literature. The result may well be influenced by demographic theory, even though no one can say at just what point in the calculation the theory was used.

Notes

1 Pascal Whelpton experimented extensively with the components method in the late 1920s and 1930s. Among Whelpton's predecessors were Arthur L. Bowley (1924) and Edwin Cannan (1895).

2 For a comment on the Heer study see Massey and Tedrow (1976).

3 For example, see Oechsli and Kirk (1974), Seiver (1975, 1976), and Hicks (1974).

4 See Becker (1960), Schultz (1974), and Willis (1973). An important alternative formulation is that of Leibenstein (1957).

5 Originally devised by the Belgian statistician Verhulst (1838), it was rediscovered and extensively used by Pearl and Reed (1920).

6 Brass et al. (1968), Brass (1971, 1974). See also Carrier and Hobcraft (1971). Extensions are provided by Zaba (1979) and Stoto (1979).

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