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CAN THEORY IMPROVE POPULATION FORECASTS?

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THE AUTHOR

A frequent visitor to IIASA, Nathan Keyfitz, Professor of Sociology at Harvard University and at Ohio State University, is a member of the US National Academy of Sciences and the author of a number of books on mathematical demography, among them *Introduction to the Mathematics of Population* and *Applied Mathematical Demography*.

FOREWORD

Roughly 1.8 billion people, 42 percent of the world's population live in urban areas today. At the beginning of the last century, the urban population of the world totaled only 25 million. According to recent United Nations estimates about 3.1 billion people, almost twice today's urban population, will be living in urban areas by the year 2000.

Scholars and policy makers often disagree when it comes to evaluating the desirability of current rapid rates of urban growth and urbanization in many parts of the globe. Some see this trend as fostering national processes of socioeconomic development, particularly in the poorer and rapidly urbanizing countries of the Third World, whereas others believe the consequences to be largely undesirable and argue that such urban growth should be slowed down.

Projections of present urbanization trends allow national planners to anticipate future population growth and consider its effect on a country's development. Can the methods used to make these forecasts be improved? 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 the related publications appears at the end of this paper.

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ABSTRACT

Scholarly work on population includes many mechanisms accounting for changes in the components of population: births, deaths, and migration. Very little of this is used by forecasters, or even referred to in the forecasting texts. The present review of the more promising theory and empirical reports in demographic journals has as its object their incorporation in forecasting techniques.

Some of the theory relates population to variables pertaining to the economy, to technology, or to social change; to use such models would require forecasts of the independent variables of the models. We know much about how fertility depends on income, but until we can forecast income (say 20 years ahead) that relation tells nothing about future population. Some theory relates population to variables difficult to measure, like the utility of children. Other parts again, like the demographic transition, are of uncertain timing. Much of mathematical demography constitutes comparative statics, which are conditional, whereas the user of forecasts requires unconditional statements.

The perspective of usefulness for forecasting provides an illuminating, if severe, review of contemporary population research.

CONTENTS

CAN THEORY IMPROVE POPULATION FORECASTS?

Population forecasts are needed for national plans, for the plans of enterprises, and for the budgeting of local authorities. Making forecasts may well engage more demographers than any other single activity. No one expects that the future can be known with accuracy; the most that can be asked is that all relevant considerations available at the time the forecast is being made be taken into account. The demographer cannot be held for failure to forecast wars, famines, periods of prosperity or the modernization of traditional economies, all of which affect population, when the experts in the disciplines concerned cannot predict these events.

SEPARATION OF SCHOLARSHIP AND FORECASTING

What the demographer can do is to bring all of his own discipline to bear on his work. A knowledgeable forecaster ought to perform better than an ignorant one. And the amount of knowledge that has been turned up in the last two or three decades is very 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 forecasting work, published and unpublished alike, contains very little reference to this wealth of literature. Forecasts tend to be self-contained within the sphere of past statistical materials; they use preceding population data and not much else. Ever since Whelpton (1936)[*], the need for considering separately the components of population change—birth, death, and migration—has been recognized, but each of these has been forecast without taking much account of the theoretical or empirical literature dealing with birth, death, and migration (Henry 1972). It is as though forecasters have intellectually isolated themselves from the main body of population studies.

Important reviews of existent methods and suggestions for new ones are to be found in Henry (1972) and Brass (1974). The present article does not primarily undertake to survey the forecasting literature but looks rather at demographic theory and empirical work more broadly in the hope of finding items that will improve forecasting.

Two outcomes of the examination are conceivable: the forecasters are neglecting means of making their estimates more accurate, and their attention ought to be redirected; or else the literature has no potential for improving forecasts.

In the latter case some explanation is needed. For Comte it was *savoir pour prevoir pour pouvoir*, know in order to be able to forecast, to be able to make policy, in my translation. Innumerable articles, not to mention proposals for research funding, have sought attention and support in the hope and expectation that the research would enable demographers to know the future better. If the research is not useable to improve forecasts we face a second question: is that because the research results are erroneous, or are they correct but for some other reason incapable of making forecasts sharper?

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

The present article is a preliminary account of what I have been able to find on a partial scanning of four journals: *Demography, Population, Population and Development Review,* and *Population Index.* A more exhaustive survey would cover also *Genus* published in Rome, *Economia y Demografia* of Mexico City, regional journals in the United States and elsewhere, as well as journals in the fields of sociology, economics, biology, which contain much material bearing on population.

It is well to begin with fertility, since future births are by far the most problematic part of population forecasts.

FERTILITY AND AFFLUENCE

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 simultaneously, in a slow decline through the nineteenth century. The logical status of the demographic transition is unclear: is it an account only of what happened in the past in Europe and America, or can it be relied on for the future? Does the mortality fall cause the fall in fertility, 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 determined by per capita income, or is relatively independent of income and goes with education, urbanization, and availability of contraceptives.

For many of the presently 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 Arsene Dumont (1890) called it. A couple were amibitious for their children, whose social ascent depended on upbringing, and especially on formal education; education was expensive in France and elsewhere in the 19th century and could typically only be afforded for one or two 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.

The researches of the Princeton group have thrown doubt on social mobility in relation to the fertility decline of individual families. In *The Third Child* (Westoff et al. 1963), they compared mobile with nonmobile women (using both the gross division of manual-nonmanual and finer occupation groups) and found little correlation even when they held constant religion and other variables. We do not know to what extent the point applies beyond the United States, or to what extent it qualifies the anthropological studies of Arsene Dumont. Mobility could still correlate with fertility decline across societies without correlating among families within a society.

Whatever the nature of the motivation to diminished childbearing, its strength cannot be doubted. The instruments of contraception were hardly known and *coitus interruptus* sufficed for Europeans to hold down the number of children to those they could provide with property, or that they 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) showed how precocious marriage gave way to late marriage throughout Western Europe, with an average age of 23 or older, and how at least 10 percent of females simply remained unmarried. Ansely 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 directions, even though there is a degree of convergence. LeBras and Todd (1981) also show the persistence of local differences in this as in other 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, to internalize the way people act in their jobs and in their childbearing, but what else does it 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 it is at the bottom of the demographic transition as well as using any estimates we might have of future ambition to tell about future childbearing. Merely to state the problem this way is to show that the social mobility idea used in this way does not really do much beyond giving one the subjective feeling that he understands something in history.

Even though questions of causation were never resolved, in the time when fertility was falling forecasts were relatively accurate. Turning points are what one misses without secure knowledge of causes. Because they took the demographic transition as completed, and had no idea of what would follow it, the forecasts of the 1930s and 1940s were grossly low. In the developed countries the baby boom came as a surprise, not accepted as such even when it was patently (to our present hindsight) under way. The less developed countries were also forecast low, because the dramatic fall in mortality of the 1950s and 1960s was not anticipated.

Kingsley Davis (1963) suggests that the problem of too many people appears to those concerned not alone as one calling for contraception, but rather for one of a number of functionally equivalent responses. These include alternative means of limiting population growth—abortion, deferred marriage, outmigration to the cities of the country in question, migration abroad. The collective problem of overpopulation is transmitted to individual

families, and their responses are such as to bring a degree of
homeostasis. One can think of the large system of the nation,
or, indeed the whole continent of Europe, linked to the small
system of the family, so that the latter is responsive to imbal-
ances in the former. Signals can be passed downward by a short-
age of land in a system of private ownership. Tilly (1978) and
his collaborators found that agricultural areas in Europe of
the early industrial period showed 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 avail-
able to men and women they can marry and have children. No one
quite knows whether some similar relation applies to the pres-
ently less developed countries.

The relation of the larger system—the country or the
world—to the small decision-making group—the couple—is a per-
petual problem for demography. In what times and places is there
homeostasis, so that over population in the larger system is re-
flected by restraint in the smaller group? With such negative
feedback comes stability. The mechanism expounded by Rose
Frisch (1978) reduces fecundity in times of food shortage, making
fewer births, which is the kind of negative feedback conducive
to stability. But we know that in some times and places poverty
incites reproduction, which makes for more poverty, a destabil-
izing positive feedback. If we knew more about the relations
of the two systems forecasting would be facilitated. As we have
seen, the relation of childbearing to social psychological states
like ambition or mobility is more problematic.

HETEROGENEITY OF UNDERLYING CAUSES

During the 1940s and subsequently there was a reaction to
population decline, and western populations began to grow rapidly
through natural increase. But only for two decades, until in
the 1960s the downtrend resumed. Was the resumption of the down-
trend the continued effect of the same causes as had operated
up to 1935? Apparently not, argues Philippe Aries (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
19th and the early 20th century. 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, a prolongation of
the individualism that had been developing in Europe during the
age of romanticism. People now search for a personal identity
of their own; 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 finance, in short scope for expanding their own per-
sonalities. Corresponding changes are taking place in men. For
adults of both sexes children become a handicap to personal ex-
pansion if the marrige continues, and to a job or remarriage if
the marriage breaks up.

Low fertility seems here to stay. A reversal of all the as-
pects of the change that goes by the name of women's liberation
is unimaginable, and since the several aspects seem to be related,
we can even less envisage a change in one without the others also
changing. Those who do not see women quitting their jobs and re-
discovering 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? A culture that
changes as rapidly as ours has done over the past generation can
change back again. Bourgeois-Pichat (1979) along with 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 re-
places itself. It has been thoughtlessly assumed that the limits
of the natural world, which certainly impose long-run stationar-
ity, 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 ir-
regular length? The Lotka theory, based on constant age-specific
births and deaths imposed on an initially nonstable age distribu-
tion, would give waves of one generation length. We look later
at the economic theory of fertility, from one version of which
waves of two generations in length are deduced.

FIRST EFFECTS OF DEVELOPMENT

For less developed countries some increase in fertility tends
to occur on the eve of modernization. That can be due to improved
health and so higher fecundity, earlier and more universal mar-
riage, increased illegitimacy, and the substitution of bottle-
feeding for breast-feeding. Modernization causes the abandonment
of various traditional practices that curtail fertility, includ-
ing post-partum sexual abstinence, common in traditional African
societies and elsewhere. Anatole Romaniuk (1980) has a useful
discussion of such matters in application to Zaire. In a later
paper he shows that the same applies to the Canadian Indians, and
the rise appears to be due to the abrupt shift from prolonged
breast-feeding to bottle-feeding that took place prior to the
large-scale practice of birth control (Romaniuk 1981).

But timing is the problem here, as in regard to the subse-
quent phases of the demographic transition. How can one possibly
know, in respect of an African country, for example, how soon the
abandonment of breast-feeding, and other elements that conduce to
shortening birth intervals, will take place? The thesis seems
correct as an explanation of the past, which is all that it
claims to be, but because of the unknown timescale, forecasters
cannot easily use it. They are inclined to think of the temporary
rise as a blip on the curve of fertility that they can pass over
with impunity.

DIRECT AND INDIRECT EFFECTS OF DEVELOPMENT

The relation between the further stages of development and
fertility has been studied by many authors. The work of Heer
(1968) can be taken as representaitve. Heer found that the direct
effect of development is increased literacy and declining infant

mortality, and 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.*

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—that predicting changing in literacy and infant mortality is not easy. Moreover, the response to literacy is probably nonlinear; somewhere there must 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.** It has seemed to be true that persistence of high fertility in Mexico, at least until very recently, did not accord with its high income, education, and urbanization. Kirk did indeed find some regularity, and demonstrated that the transition goes through increasingly quickly as time moves along; 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 stateable.

Beyond all this is the data problem, that has constituted a good part of the controversy. Never mind what will happen in the future—is the Mexican birth rate falling now? It appears to be falling, but the rate of fall is uncertain. The trouble is not that Mexico lacks a birth registration system—she has one. But unfortunately an unknown fraction of the births do not get registered, and examination of the numbers suggests that the fraction is far from constant over time.

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

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

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 tertiary education, and are moving fast towards 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 increasing. 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 that there is a direct effect 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 and out of accord with 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 that the child presses for.

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 which heavy investment is made. One can talk about having schools that will not strain the social fabric, that will accept peasant values

and adapt the child to 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 the specific 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 destroy the family as an economic unit.

The new sort of person produced by the schools obtains more from the older generation and is less an asset to that generation. In another writing 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 flow of wealth, in which the young become an expense rather than a source of material gain, that is responsible for fertility decline.

In summary, on the thesis developed by Caldwell and independently in unpublished form by Norman Ryder, the flow of wealth backward up the generations makes for large families, and the flow downward for small ones. The modern expansion of education has an important role in turning the flow around. The thesis revolving around the direction of the flow of wealth is convincing, and adds to our understanding of the demographic transition. That indeed can be said to be its purpose—to add to understanding. It claims no ability to turn understanding into numbers, and so increase forecasting power.

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 the cities of the less developed countries grew by an average of 4.4 percent per year, while the growth of the rural population was only 1.6 percent. By 1975 the urban population was 27 percent of the total in the less developed countries, much more in Latin America, less in Africa. How does this affect fertility?

Urbanization is a force that in many respects acts in the same direction as education—it exposes people to the modern world. The attitudes of governments, and indeed of some social scientists, is less positive towards the cities than it is towards schools, but other social scientists are positive. On one simple view people would not come to the city unless it makes them better off than they were in the countryside. And since under competition their incomes are proportional to their production, their being better off means that they contribute more to the national product. Harris and Todaro (Todaro 1976) and Rempel (1981) agree that not everyone's lot is improved in the city, because getting a job in the formal sector is a random event that will not occur for all, but all have an initial expected income in the city greater than their expected income in the countryside.

On the other hand, it is argued that poverty in the city is more ugly than in the countryside; that people only come because land shortage prevents them from earning a living in the countryside; those inmigrants to the city who live in shanty towns, 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 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. The positive and negative views, whether people come to the city because they are pulled by its attractions, or whether they come because they are in effect pushed out of the countryside, are not always easy to distinguish, since they are both stating that it is the differential incomes and other advantages that cause urbanization. If

the pull lowers fertility more than the push does, it would be indeed desirable to make the distinction.

In Europe and America 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 of movement and fertility applies around the world today. In some instances the fertility of the movers into the city is every bit as high as that of the countryside from which they came. In other instances there is some difference between the fertility of the movers and that of the people they left behind, 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 that of 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 but not in any obvious way for forecasting.

Mere selection of migrants into the city would not change the national mix, and hence as such would not have any effect on national increase. Urbanization lowers national rates of increase in the degree in which the city itself lowers the fertility of those who have come to live in it. Thus mere data on differential fertility between city and countryside cannot tell us the effect of urbanization on national increase; we would have to subtract that part of the city differential as shown in the statistics that arises as selection of the less fertile rural people for migration.

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 of 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 show 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 due to Kuznets (1975) would seem more appropriate.

Repetto provides evidence on the effect of equality in inducing fertility decline, mostly in the form of cross-sectional regressions among countries in which disturbing variables are held constant. The point clearly has 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 (1960) 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 is related to policy rather than to forecasting. Sometimes, especially in the first edition (Malthus 1959) he seems pessimistic enough about people having the strength of character 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 would indeed restrain their childbearing and climb high 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, in which much more sophisticated models have appeared. Malthus does not help forecasting because his writing on population, when read in context, is conditional. One can think otherwise by 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 there will be certain bad consequences. Malthus's model and all his writing were oriented to policy, and only on superficial reading does he appear to be making predictions.

There is a lesson for later workers here, in the distinction between policy models and forecasting models. "If... then..." statements are not what the client of the forecaster wants, but rather an unconditional estimate of what will happen. After all it is not usually the client's business to influence the growth of population; all he wants is to sell it automobiles, or toys, or wheelchairs. Faced with a policy maker who is going to do something about population, the forecaster is confused and embarrassed. Some of the authorities in China seriously believe that they can reduce the population to 700 million within a generation or two. The demographer may say that they cannot do this, or that they should not do it, but then he is enmeshed in policy. If he cannot make a forecast for a country without an embarrassing judgment on the soundness and the success of a proposed policy then policy making is indeed an obstacle to forecasting.

HUMAN CAPITAL

A distinct way of viewing fertility change is provided by the human capital school.[*]

It had 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 decision unit. The household has a production function whose 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 greater utility by herself taking a job. In doing so she has weighed the

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

satisfactions of the labor market against those of the home and
found the former greater.

The household faces a double constraint: of income and
time—money (from wages and other sources) that buys goods, and
24 hours per day for each of its members for time. Substitution
between goods and time will take place in shifting ratios as
goods become more attractive, and as time becomes worth more.
One of the ways in which the time of an individual is made more
valuable is by investment in education. Such investment in due
course brings considerable returns to the individual as well as
to the community. The earlier in his or her life the education
takes place the greater the total return on it. For the commun-
ity the investment accounts for most of the increase of produc-
tion—many times the increase that the investment in physical
capital accounts for. The more capital is invested in a person
the greater the value of his or her time in the labor market,
and with other things equal, the less disposition there will be
to spend time in the home. Differences of income among individ-
uals are now explained: the people who embody more investment
draw a return on that investment.

For Becker couples are placed before two decisions: the
quantity and the quality of the children that they are to have.
As their income rises they invest more of it in quality. Some
have feared that children might be inferior goods, like bread
of which less is consumed as more meat becomes available.
Becker assimilates children to normal goods: more income causes
one to buy more, and total expenditures on children rise as in-
come rises, even though the number of children declines. The
trouble is that the relation between income and children esti-
mated for one place is not likely to be the same as for another,
nor can we count on its constancy through time. Economists
have complained that the theory is not very helpful insofar as
one has little control over the quality of one's children; if
one has bought a home in a good neighborhhod the children nec-
essarily share it. But the main point is that Becker again
makes children a positive function of income. On this theory
one would need a measure of quality (i.e., price or cost per

unit) and apply this to deflate the expenditures on children to find their number. One could go through a chain of calculations for forecasting purposes, but the uncertainty at each link in the chain throws doubt on the usefulness of such a process.

Supposing the link of fertility to income is correct and quantifiable, how might it help forecasting? Presumably only in the degree in which income can be forecast. But however elusive future population may be, future income 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 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 transition is due to 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 do it. The young adults of the 1950s were a small generation, being the cohorts born in the 1930s, and they were correspondingly advantaged. They had been able to enter college without any 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; as they were followed by more numerous younger cohorts they were quickly pushed up into positions of supervisor, teacher, etc. 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 con-
vincing 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 cor-
respond 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 turn
out to have many children. There has been some rise in the
United States and other countries already, but the degree in
which it will persist remains to be seen.

Paul Samuelson (1976) elaborates the Easterlin model in a
nonlinear form that he shows 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 Census
Bureau is experimenting with it. Ronald Lee (1976) discusses
the matter and presents some hopeful considerations.

Accepting that the mechanism really exists, which I firmly
believe, the question is whether it will show itself in actual
birth performance beyond the one 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 emerge
to make its appearance in the actual birth series?

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

Notice that Becker's theory (Becker 1960) derived from the
"new home economics" explains fertility decline 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. Through 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 opposed theories, and we cannot be ready to apply them to forecasting until we have decided which is right.

OPPORTUNITY COST

The relation between the economy and fertility can be expressed as opportunity cost; women will engage in less childbearing the more they are paid for their time outside the home. This hardly explains why it is that in good times fertility rises, an unquestioned feature of the economic cycle. But it does help to understand why it is that women took jobs in the 1960s and correspondingly had fewer children, say as compared with the 1920s; the economy had changes so that there was a demand for their services, and the work was much more pleasant than the typical factory job that was a main possibility for women in earlier times.

James Cramer (1979) measures the amount of income foregone in having a child, using panel data. For a second or higher order birth he finds a loss of employment of some 400 hours per year, an income loss of US$1050 in 1969 dollars. He finds that the time cost does not depend on birth order, age of oldest sibling, husband's wage rate or wife's potential wage rate. This relative invariance of the time cost of having a child is an advantage for understanding and for forecasting, as it would put the whole variation of fertility into the wage rate. But the trouble remains that we still do not know whether a high wage rate raises fertility, as the high wages at the peak of the economic cycle do, or whether it lowers fertility because it raises the opportunity cost of a woman's time.

PROSPERITY AND FERTILITY

That the birth rate goes down in periods of depression and up in prosperity is illustrated by various periods of American and European history, though demographers point out that the amount of rise and fall varies considerably. The Institute National des Etudes Demographiques (Girard and Roussel 1979) has found that the public accept the relation with little qualification. In its survey 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 the uncertainty with regard to employment would cause parents to give up the idea of having a child altogether. That the public "knows" this is of course what brings 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 rest 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 the next 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 condition, beyond the writers mentioned here, a thesis due to 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 psychological mechanism would produce
waves of two generations in length, and in this respect resemble
that of Easterlin. If two generation waves indeed appear, is
there any way of making the data tell us whether the Easterlin
or the Grauman mechanism actually produces them?

SUBGROUPS

A natural way of forecasting is to take account of subgroups
in a population, and suppose that their shifting relative numbers
will determine overall fertility. Changing fertility is thus
resultant of the mix of the several groups in the population
as the mix varies through time. If Hispanics have higher fertil-
ity than English speakers, and they are increasing as a fraction
of the population, then the fertility rate will rise on that ac-
count. Such a notion is hardly entitled to be called a "theory"
of fertility, and yet it underlies a certain amount of demo-
graphic work. The most conspicuous instance is with respect to
age, where it is supposed that the breakdown of the population
by age will improve the forecast as compared with disregarding
age.

The case of religion shows how far out one can be on 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 where Catholic fertility
was very little higher than non-Catholic, the differential in-
creased markedly during the baby boom, and then declined to the
point where the two nearly come together in the mid-1970s. To
count on any of the set of subgroups for fixed rates is a thin
reed on which to rest a forecast.

CHILDBEARING INTENTIONS

Perhaps the theories that we have discussed are too far
above the subject—somehow 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.

About the year 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 data 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 that would elicit their plans for childbearing. And now, after 40 years of effort, this instrument has come to seem uncertain, even controversial (Hendershot and Placek 1981), though it has been incorporated in the procedures of the United States Bureau of the Census for the 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. And the difficulty is that most children are born within a few years, usually less than a decade, of marriage. In a typical case a woman would marry at 23 and have whatever 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 the childbearing intentions data in a more sophisticated way.

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

A more thoroughgoing change in the procedure is due to Clyde and Lolagene Coombs (Coombs et al. 1975). They do not stop with one question on number of children intended, but find the entire preference scale of the couple, both with respect to sex and to the 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, etc. A map of the entire preference set is more informative than the single number representing its maximum value.

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 due to Box and Jenkins (1970) based on analysis of the internal structure of a time series, and finds it applicable to the series of births. The Box and Jenkins method starts with calculation of the serial correlations, and takes it that these are constant through time. The work of Lee, as well as of McDonald (1979) shows that there are circumstances where the results are superior to 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. Perhaps the constant underlying structure assumed does not persist long enough in demographic material 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 apply-
ing 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,[*] which
at least recognized a limit on the possible growth of the popula-
tion. One of the objections to such methods is that they dealt
with the population as a whole, instead of considering the sepa-
rate components of birth, death, and migration, and they disre-
garded age.

Work by Brass[**] over the past decade has provided by far
the most effective 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 bringing it close to a straight line, and then seeing how
the parameters of this straight line have been changing over
time. The approach is a powerful one, as has been shown in ex-
tensive applications to populations on all continents.

Though not developed specifically for forecasting, the method
of Coale and McNeil (1972) for dealing with nuptiality has poten-
tial in this direction. The model supposes that girls come to
marriageability, 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 extrapolating, then the cru-
cial question becomes what elements descriptive of mortality or

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[*]Originally devised by the Belgian statistician Verhulst
(1838), it was rediscovered and extensively used by Pearl and
Reed (1920).
　　[**]Brass, ed. (1968), Brass (1971). See also Carrier and
Hobcraft (1971). Extensions are due to Zaba (1979) and to Soto
(1979).

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 and so initiated his promising approach. An alternative way was carried out some time ago by Ledermann and Breas (1959) who found that two or three dimensions serve. Their work has been applied to forecasting recently by Le Bras and Tapinos (1979). They used three factors, expectation of life at birth, juvenile (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, where there are no resources left for their expansion, they gradually slow to a zero rate of increase. Raymond Pearl (1924) told the story at much greater length. (See also Raymond Pearl and Sophia Gould 1936.) One could safely count on moving forward on the logistic curve as long as resources remained constant; when resources took a jump then 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 its 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, or
the inverse tangent, both of which resemble 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, its unwillingness to distin-
guish between theories, is one of the frustrations of empirical
study that demography shares with other fields. It comes out in
extreme form when we fit a logistic, which on the face of it is
a possible trajectory, and compare that with a fitted hyperbola,
which cannot possibly represent the future evolution of the popu-
lation, since it goes off to infinity within a generation or two.

SUMMARY

In this review of the most prominent theories of fertility
we have found much too little that helps in the day-to-day work
of the forecaster. There seem to be at least six different rea-
sons why perfectly valid research, which produces an important
theoretical relation, cannot be usefully incorporated in fore-
casts.

1. Much theory is deliberately cast in the form of compara-
tive statics, and provides conditional results that are important,
but neither intended nor useable for prediction, which requires
dynamic and unconditional models. Data are often assembled in
the form of cross-sectional comparisons, and what the forecaster
needs 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 empirical 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 income rises, then
after a certain point, everything else remaining constant, fer-
tility will fall." That is a valuable kind of knowledge, even

though it is not very helpful for forecasting: we do not know that everything else will remain the same, and even if we did we would need to have a forecast of income to use it. This latter point is developed further below.

2. The theory may be irrefutably true, either because it is a logical relation or is based on incontrovertible data, and relevant to the future, but its effect is swamped by other relations. The Easterlin effect positively exists: couples do tend to have more children when their financial position is relatively good. We have always known that in times of economic depression births are postponed, and some are not ever made up, and 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, that can briefly be called women's liberation. If women want to work, and have their importance in the outside world as men have always had, then they will have fewer children. The Easterlin effect is still there, but it is covered over by the liberation effect. A few more years into the 1980s will tell us which of the two effects dominates. Meanwhile the forecaster is in a quandry which one to go on, for 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 in five years, so 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 equality in a society contributes to its capacity to bring down its birth rate, and in the later phases of development equality usually increases, but we cannot easily

forecast how rapidly this will happen. We know that if women
will stop using purchased foods and nurse their young the birth
rate will fall, but we have no way of knowing when the change
in infant feeding will occur. For advanced countries we believe
that there will be a rise in births 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
know but not useable for forecasting.

5. Even if we know the timing, the relation can change un-
predictably. The end of the demographic transition in the 1930s
gave way to the baby boom of the 1940s and 1950s. And then after
that, as Aries 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 change back in this respect, or a change in some wholly
new direction.

6. Finally the relation may be there, and everything about
it known, but it is 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 housewives statements of
their intentions not much more. Forecasts of weather and earth-
quakes, where the next few hours are the subject of interest,
and unemployment where the next year or two is what counts, may
be difficult enough. Population, where one tries for a whole
generation or two ahead, is much more difficult.

THE SCHOLARLY LITERATURE AIMS AT UNDERSTANDING

Much of the literature is designed to help us to an internal
understanding of past trends and developments. Caldwell gives
us a clear idea of the way education and the flow of wealth con-
tribute to the fall of the birth rate. Dumont's social mobility
(called by him capillarity) helps to understand the fertility
fall up to World War II, and Aries's liberation effect accounts
for the current decline to well below replacement fertility.
The Harris and Todaro lottery for city jobs gives a rational
understanding of why people migrate to the city and remain when

they find no jobs there. Like the economist's opportunity cost of women's time these hypotheses are intended to give certitude to our thinking about demographic change. That is a different order of knowledge from what the forecaster requires.

Comte's thought, as quoted above, was that knowledge would be tested by its ability to forecast, and then once it was confirmed by successful forecasts, it could be used for formulation of policy. In fact things have not worked out that way. Forecasting that is unconditional is simply too difficult. At best we will take a very long time to build up the stock of knowledge required, and policy cannot wait. What we need for policy advice is a different kind of knowledge, that might be called conditional, that answers such a question as, if everything else 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, and regards a conditional answer as an evasion.

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, as far as they go circumvent the need for causal understanding.

What emerges from all this is the realization that forecasting is more difficult 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 a few instances in which understanding of the past and the conditional relations can be brought to bear on forecasting. The Easterlin effect, with its determinate two generation cycle, may well be one of these.

We ought to admit freely that forecasting is difficult, and not pretend that we can easily develop "methods" for doing it. A method that would take account of future wars, plagues, and prosperity is no more likely to appear than a method for changing the earth's orbit. What we can seek with some hope of finding 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 past from observed statistical regularities.

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