How Do We Know the Facts of Demography?

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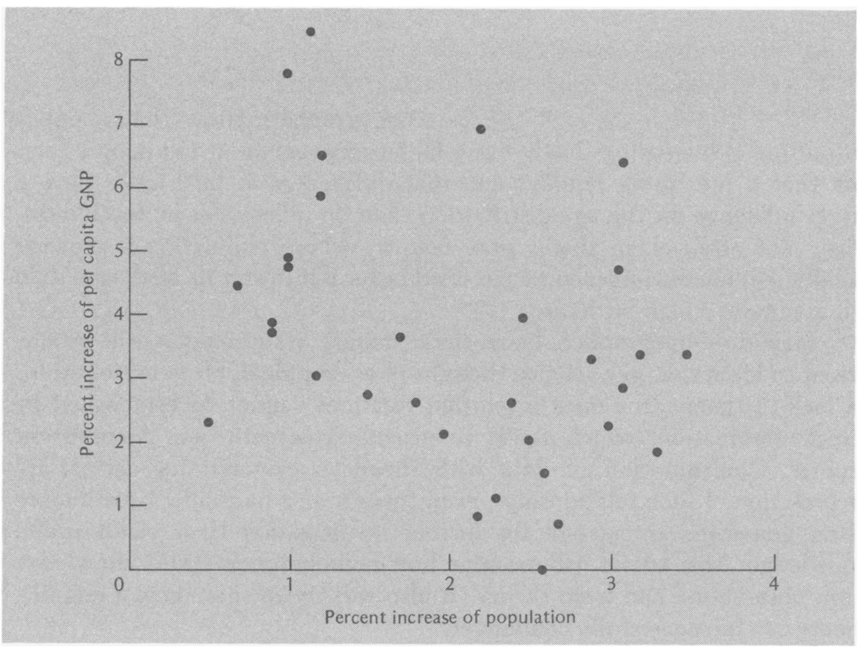
Demographers know that a population that is increasing slowly has a higher proportion of old people than one that is increasing rapidly; and that differences in birth rates have a larger influence on the age distribution than do differences in death rates. They also often claim that a poor country whose population is growing rapidly will increase its income per head faster if it lowers its birth rate than if it maintains a high birth rate.

How do demographers know these things? Many readers will be surprised to learn that in a science thought of as empirical, often criticized for its lack of theory, the most important relations cannot be established by direct observation, which tends to provide enigmatic and inconsistent reports. Confrontation of data with theory is essential for correct interpretation of such relationships, even though on a particular issue it more often generates an agenda for further investigation than yields useful knowledge. This article will examine how demographers distill knowledge from observation and from theory. It also will try to show how a reigning theory can be successfully challenged.

Let no one think these questions are remote or purely abstract. The resolution of the major policy issues of our time depends on the answers. How much of their development effort should poor countries put into birth control if they deem their rate of population growth excessive? Some would put nothing, in the expectation that rapid increase of income will by itself bring population under control. Once people have automobiles, once their countryside is paved over with roads, once enough air-conditioned houses are built, they will lower their fertility. But is this not an overly circuitous way of getting people to use pills and IUDs? Surely direct intervention aimed at lowering fertility would help reach desired developmental goals faster.

Any answer to such questions must take into account the degree to which a low rate of population increase promotes development. That is no simple matter to ascertain. Figure 1 shows the relation between rates of population growth and increase of income per capita. Even the most imaginative viewer would hardly see the negative relation that the dominant theory (later to be summarized) requires. In the pages ahead, the irregularity of empirical data as they appear in charts and tables will be repeatedly contrasted with the clear-cut mathematical relations of theory. Every such contrast presents a puzzle, and tackling puzzles constitutes demographic research.

Figure 1 Average annual increase of per capita GNP and of population for countries with over 20 million population, 1960–72



The theoretical approach can be described as "holding unmentioned variables constant"; the empirical, for example in the form of a regression between measured variables, as "allowing unmentioned variables to vary as they vary in actuality." The difference is first studied with an example in which we think we know the true nature of the relationship between two variables.

Growing Populations Have Smaller Proportions of Old People

The population of Mexico grows at 3.5 percent per year; its proportion at ages 65 and over is about 3 percent. The United States has been growing at less than 1 percent per year; its proportion 65 and over is about 10 percent. The relation can be expressed as a linear equation. For 1966 the four numbers are:

	Mexico	United States
Rate of natural increase (percent)	3.44	0.89
Percent aged 65 and over	3.31	9.42

Call the annual percent rate of increase r, and the percent over age 65 P_{65+}. Then the straight line from the 1966 information on the United States and Mexico is

$$P_{65+} = 11.5 - 2.3r \quad ,$$

which tells us that for each 1 percent by which the rate of increase is higher, there is a decrease of 2.3 percent in the proportion aged 65 and over. With zero increase the percent over 65 would be 11.5; with 3 percent increase it would be $11.5 - 6.9 = 4.6$ percent.

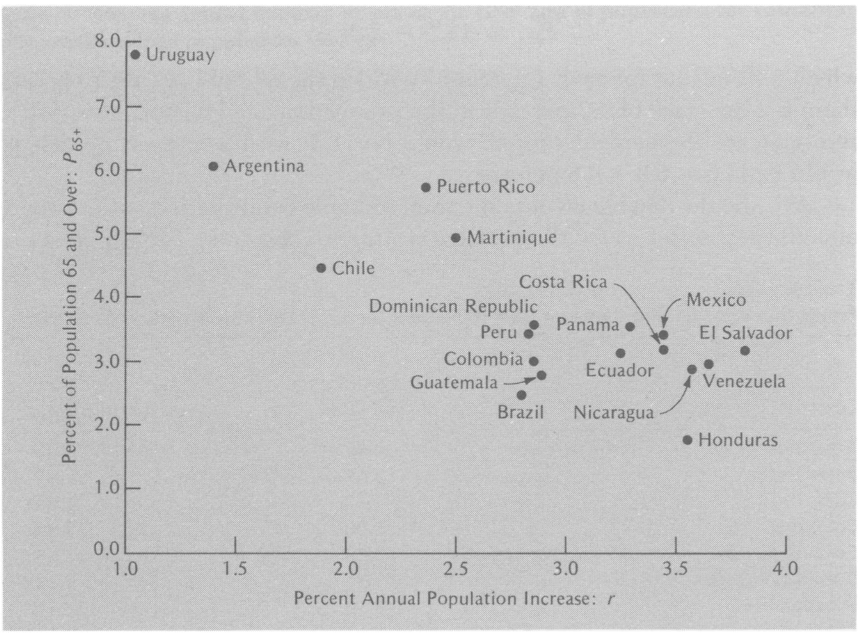
We should be able to obtain a more reliable result with a larger group of countries, so let us try those of Latin America shown in Table 1. The re-

Table 1
Proportion Aged 65 and Over and Rate of Natural Increase, 18 Latin American Countries

Country	Percent Aged 65 and Over	Percent Rate of Natural Increase
Argentina 1964	6.05	1.40
Brazil 1950	2.45	2.80
Chile 1967	4.47	1.89
Colombia 1964	3.00	2.85
Costa Rica 1966	3.18	3.44
Dominican Republic 1966	3.57	2.85
Ecuador 1965	3.16	3.25
El Salvador 1961	3.18	3.81
Guatemala 1964	2.77	2.89
Honduras 1966	1.76	3.55
Martinique 1963	4.96	2.50
Mexico 1966	3.31	3.44
Nicaragua 1965	2.90	3.57
Panama 1966	3.57	3.29
Peru 1963	3.42	2.83
Puerto Rico 1965	5.77	2.36
Uruguay 1963	7.81	1.03
Venezuela 1965	2.99	3.65

sult is $P_{65+} = 8.45 - 1.6r$. Apparently the more homogeneous group gives a less steep slope than the United States and Mexico. Now each 1 percent increase in r is associated with a drop of 1.6 in P_{65+}—only two-thirds as much. The scatter diagram (Figure 2) shows that we could have chosen two countries that would provide almost any given slope. Moreover, much of what correlation exists is due to three countries of the southern cap—Argentina, Uruguay, and Chile—that are culturally distinct from those farther north, along with Puerto Rico and Martinique. To exaggerate a little, it looks as though countries fall into two groups, those with low r and high P_{65+}, and those with high r and low P_{65+}. In short, much of the pertinent information was contained in the comparison of the United States and Mexico with which we started.

Figure 2 Relation of proportion of the population over age 65 to the rate of population increase: 18 Latin American countries



What about taking one country and following changes through time in the two variables? Sweden provides information over nearly 200 years, and also provides a very different regression from any obtained cross-sectionally.

The comparisons and regressions summarizing them are highly inconsistent in reporting how much difference in the proportion over 65 is to be associated with differences in the rate of increase. A large research project could be undertaken to see why they fail to agree; it might reveal that the changing mortality over 200 years in Sweden is confounded by the changing birth rate; that the more homogeneous the group, the lower the correlation and the lower the slope of regression. It happens that in this instance no one will undertake such research because a simple theory is available that will provide a better insight into the nature of the relationship between growth rate and age distribution. Let us use this theory to stand back and take a fresh run at the question.

Older Population as a Function of Rate of Increase When All Else Is Constant

For this more abstract consideration we might start with an extreme stylization. Let us imagine a country in which 100,000 births take place each year, every one lives to age 100, and there is no migration. Then the population at any moment is exactly 10 million, and the fraction over age 65 is exactly 35 percent at all times. This contains the essence of the stable population model—a model describing the structure and dynamics of a "closed" population with constant schedules of fertility and mortality. But the assumptions underlying the example just given need generalizing in two directions.

The first is to a more flexible mortality pattern. To suppose that everyone lives to age 100 is to specify a very special kind of survivorship schedule (or life table), and we can easily improve on it by using the mortality of the country in question. With United States 1972 mortality, taking both sexes together, the fraction over 65 comes down to 15.5 percent.

Let us now also allow for increasing births. Suppose that the fraction of births surviving to age x is given by a fixed survival function $s(x)$, and the annual percent rate of increase of births is r, so that compared with x years ago the number of births is now $(1 + r/100)^x$ greater. Then for each present birth there were $1/(1 + r/100)^x$ births x years ago, and of these past births a fraction $s(x)$ have survived, the surviving individuals being now aged x. Thus the number of present individuals of age x must be $Bs(x)/(1 + r/100)^x$, where B is the number of current births. This applies for all ages, and suffices to specify the age distribution.[1] Since the expression depends on r, it will tell the relation between any given index of the age distribution on the one hand and the rate of increase on the other.

For example, the proportion aged 65 and over is simply obtained by summing up the number of persons at ages 65, 66, 67, and so on, all the way to the maximum age of life (say 100), and dividing this sum by the total population. The latter is obtained by summing up the number of persons at all ages, beginning at age zero. To express this in percentage terms we must also multiply the result by 100:

$$P_{65+} = 100 \frac{B\sum_{65}^{100} \{s(x)/(1 + r/100)^x\}}{B\sum_{0}^{100} \{s(x)/(1 + r/100)^x\}} \quad . \tag{1}$$

If the $s(x)$ is fixed, equation (1) establishes P_{65+} as a function of r and of nothing else. The equation is not very instructive in this form, for we cannot easily see whether P_{65+} increases or decreases with r, let alone by how much. One way to study the matter is to set up model tables of stable populations, in which stable age distributions are in effect tabulated for many combinations of r and $s(x)$.[2]

Another way is to "linearize" equation (1). If r is small, one finds that with good approximation:

$$P_{65+} = 100 \left[1 - \frac{r(m_2 - m_1)}{100}\right] \sum_{65}^{100} s(x) \Big/ \sum_{0}^{100} s(x) \quad , \tag{2}$$

where m_2 is the mean age of those 65 and over in the stationary population described by $s(x)$ and m_1 is the mean age of everyone, also in the same stationary population.

Equation (2) can be applied with a minimum of data, as it involves quantities that vary little among populations. Thus $\Sigma_{65}^{100} s(x)/\Sigma_{0}^{100} s(x)$ —that is, the fraction 65 and over in the stationary population described by the survival schedule $s(x)$—is 0.127 for Mexican males and 0.123 for United States males; the means m_2 and m_1 are usually not far from 75 and 35, so that $m_2 - m_1$ is about 40. Thus, using information that a demographer carries in his head, the expression (2) comes to about

$$P_{65+} = 100\left(1 - \frac{r}{100}(40)\right)0.125$$

or

$$P_{65+} = 12.5 - 5r \quad .$$

A similar expression applies for other ages. For example, the percent 55 and over on the same theory is

$$P_{55} = 23.0 - 6.9r \quad .$$

These theoretical relations largely escape defects of the data. Another advantage of the theoretical approach is that we know exactly its assumptions. In this instance, our model specifies that comparison be among populations closed to migration, with the same life table but different rates of increase; that each of them have had births increasing exponentially during the lifetimes of persons now alive, or alternatively, have had fixed age-specific birth and death rates over a long past period. Consequently, this model does not tell anything about the change through time from one such condition to another; the trajectory from rapid increase to stationarity for a given population requires a more difficult kind of mathematics. That the theory here, like the comparative statics of economics, permits the comparison of stable conditions only is both a strength and a weakness.

Instead of supposing fixed rates in a closed population, the empirical regression takes into account migration, in whatever proportion it has been occurring in the populations whose data are included. Insofar as mortality has been falling, the influence of that fall is also incorporated. Thus it is a better description of the state of affairs covered by the data; it is a worse description of the intrinsic relationship between the stated variables. If underlying conditions are the same in the future, the regression will predict better; if they change substantially, the theory is more dependable. If an underlying interference is by some known and measurable variable, the empirical regression can "partial" it out, and in this degree approach closer to theory, while still remaining empirical.

In another aspect the regression inevitably depends on a data base, and that base is determined by what data are available. One can hardly apply sampling notions to it, since whatever unit is taken, the number of measured populations that are truly independent is small. Moreover, data on many countries are lacking. Even if each entity describable as a nation could be thought of as providing independent evidence, and if all had good data, the collection of nations is not easy to conceptualize as a homogeneous universe.

This simple introductory example shows how uncertain our knowledge would be if analytical tools like the stable model were not available. One can imagine extensive research projects for describing the various extraneous factors, methodological controversies, and schools of opinion, some perhaps taking the view that the relation was really different for different races or different continents. One who has been through the theory would no sooner say that the underlying relation between growth and age composition is different for continents than he would say that the laws of thermodynamics differ from country to country.

Are Births or Deaths Decisive?

The same stable model can help decide whether the age distribution of a population depends more on its births or on its deaths.

Venezuela in 1965 had a greater proportion of children plus old people than Sweden in 1803–07. To compare a contemporary nonindustrialized country with one in the early nineteenth century shows an aspect of the difference in the process of getting development launched then and now. A high dependency ratio (children under age 15 plus adults over 65 as a proportion of the number of working ages 15–65) is a disadvantage for development; Venezuela's dependency ratio in 1965 of 1.021 is two-thirds greater than Sweden's in 1803–07 of 0.589. One would like to know to what extent this is due to Venezuela's lower death rate and to what extent to its higher birth rate. No such decomposition is even conceivable on the observed rates—they show what they show.

The stable model, in which the number of persons aged x is proportional to $s(x)/(1 + r/100)^x$, allows one to synthesize dependency ratios from various combinations of birth and death rates:

Venezuelan births and Venezuelan deaths	1.021
Swedish births and Venezuelan deaths	0.703
Swedish births and Swedish deaths	0.589

Table 2
Features of the Stable Age Distribution and Rates of Increase Obtained by Combinations of Female Birth and Death Rates from Five Countries: Venezuela 1965, United States 1967, Madagascar 1966, England and Wales 1968, and Sweden 1803–1807

	Age-Specific Birth Rates of				
Age-Specific Death Rates of	Venezuela	United States	Madagascar	England	Sweden
		Percent Under Age 15			
Venezuela	47.7	23.9	47.8	23.6	34.2
United States	48.5	24.5	48.6	24.2	34.8
Madagascar	45.0	22.0	45.2	21.8	32.1
England	48.5	24.5	48.6	24.2	34.8
Sweden	43.6	21.0	43.8	20.8	31.3
		Dependency Ratio (Percent)			
Venezuela	102.1	58.8	102.4	58.7	70.3
United States	105.4	61.1	105.6	60.9	72.5
Madagascar	91.3	51.5	91.8	51.3	62.8
England	105.2	60.3	105.5	60.1	72.1
Sweden	85.6	46.7	86.2	46.6	58.9
		Percent Aged 65 and Over			
Venezuela	2.8	13.1	2.8	13.3	7.1
United States	2.8	13.5	2.8	13.7	7.3
Madagascar	2.7	12.0	2.7	12.2	6.5
England	2.8	13.1	2.7	13.3	7.1
Sweden	2.5	10.9	2.5	11.0	5.8
		Stable Rate of Natural Increase (Percent)			
Venezuela	3.9	0.5	3.9	0.5	2.0
United States	4.1	0.7	4.1	0.7	2.2
Madagascar	2.2	−1.1	2.3	−1.2	0.4
England	4.1	0.7	4.1	0.7	2.2
Sweden	2.4	−0.9	2.5	−1.0	0.6

The effect of the birth rate when the death rate is constant is 1.021 − 0.703 = 0.318; the effect of the death rate when the birth rate is constant is 0.703 − 0.589 = 0.114; of the total difference of 0.432, the part due to births was about 74 percent, that due to deaths about 26 percent.

We could alternatively have used as the intermediate term in the decomposition the dependency ratio with Swedish deaths and Venezuelan births, which is 0.856. The death-effect would have been 1.021 − 0.856 = 0.165 and the birth-effect 0.856 − 0.589 = 0.267. Now 62 percent of the difference is due to births, still the larger part. We can say that between 62 percent and 74 percent is due to births, the interval between these numbers being an interaction that cannot be allocated.

Any other feature of age can be similarly analyzed. Sweden's percent under age 15 was 31.3, Venezuela's 47.7; the combination of Swedish births and Venezuelan deaths would produce 34.2 percent. Hence, of the difference of 16.4 percentage points (= 47.7 − 31.3) the amount of 2.9 (= 34.2 − 31.3) was due to deaths and 13.5—over four times as much—to births.

This and other theories show that differences in fertility (birth rates) are more responsible than differences in mortality (death rates) for distinctive features of age distributions. The reader can do a considerable amount of such analysis for himself from the data in Table 2, where age-specific birth and death rates of five countries have been used in all combinations to construct stable age distributions and rates of increase. He will find that fertility differences are always more important than mortality differences.

No Model, No Understanding

A good deal of data is on hand regarding breast cancer. Despite stepped-up efforts to deal with it, expensive operations and other forms of treatment, and widespread publicity urging women to examine themselves and to see their doctors at once if there is any indication, the increase of deaths from breast cancer is considerable in North America and Western Europe, just where the most intensive effort is being made. Breast cancer is the leading cause of death for women aged 35–54 and second only to heart disease for older ages. Some of the increase may be due to more awareness and hence more frequent diagnosis now than in the past, and to better diagnosis in America and Europe than in Asia and Latin America, but apparently this is not the whole cause. Women who bear children early seem to have a lower risk of breast cancer, but no one thinks that having children—early or late—can prevent the disease or account for the differences. Breast cancer is less common in warm climates and among poor populations, but that climate or poverty is a preventive seems unlikely.

Such statistical differentials are mere unsolved puzzles until someone comes along with a model that explains the differences. In the meantime, all that can be done is to continue gathering the data to discriminate among proposed models.

The ratio of male to female births is a similar case, in that there is no obvious model, and no clear-cut result has so far emerged from differentials and correlations. We know that births to young mothers have a higher sex ratio (males to females) than births to older ones, that first births to a mother have a higher sex ratio than later births, and that children of young fathers have a higher sex ratio than children of older fathers. But among age of mother, parity of mother, and age of father, which is the operative cause? The high intercorrelations among the possible causes make it difficult to distinguish. Mechanisms have been suggested involving the relative activity and viability of sperm producing male and female babies, but until some such mechanism is shown to be the operative one, our knowledge has a tentative and uncertain character. Here is just one more question that is unlikely to be solved by any volume of statistics by themselves, although they should be able to discriminate among models based on the biology of the matter, once convincing models are presented.

Too Many Models

India and some other countries have raised the legal age of marriage, partly with the aim of lowering the birth rate. Implicit in the thinking of legislators and others is a theory in which marital fertility age for age is relatively fixed, and the legal minimum age effectively eliminates the part of the fertility curve below that age. Given the curve, the amount of effect is easily calculated. Our sample survey data for India[3] show that out of 18.14 million births in 1961, some 3.24 million or 18 percent were to mothers under age 20 years. If these could be eliminated, the impact on the rate of increase is exactly calculable.

This seems a potentially powerful argument for restriction of marriage, supposing it feasible to raise the age as high as 20 for women. But before one reaches a firm conclusion it ought to be noted that on an opposite model raising age at marriage would be wholly ineffective.

Suppose that married couples are not reproducing to the maximum, but that they want a certain number of children, and will have later what the law forbids them to have sooner. After all, the birth rate of India is now under 40 per thousand, well below the physiologically possible maximum. Under these circumstances, the only gain of a legal minimum age would be a slight delay—perhaps five years or so—which would lengthen the distance separating successive generations and hence lower the increase, but by a small amount. Illegitimacy is also a problem; it is low in India, but one of the reasons parents want their daughters married off early is to avoid their engaging in premarital sex. If the parents' fears are not all imaginary, then there could be some increase in children born outside of marriage.

Yet this argument is in the end unconvincing; one has the impression that couples that lose time before they are 20 may make up some part of the lost ground, but not all, and that extra-marital fertility would remain low. To know the net drop in overall fertility as a result of the restriction requires behavioral data. That alone can discriminate between the competing models and predict the quantitative effects of an induced change in age at marriage.

Promotion in Organizations

Everyone knows that in a fast-growing organization promotion is likely to be faster than in one growing slowly. Neither elaborate empirical data nor a model are required to demonstrate that bare fact. What one would like to know is the quantitative relation: in a fast-growing organization does one get to a middle position a few months sooner than in a slow-growing one, or several years sooner?

One can imagine collecting a good deal of data to settle this point. One would have to give attention to the universe of organizations from which one was sampling—perhaps settling on all commercial, transport, and manufacturing firms in the United States. One would have to define the boundaries of each organization, whether it includes all establishments constituting a firm, or whether each establishment is to be considered a separate organization. A lower limit would have to be set to the size of organization considered, say 100 employees. One would want to distinguish family-run enterprises, since the conditions for promotion in these would certainly be different. If a one-time survey was to be made, then the information on promotion would have to be obtained retrospectively, with the errors of recollection that this entails. On the other hand, a succession of surveys that statistically followed careers of individuals would take time and be expensive. Many decisions would have to be made to establish the universe and to conduct the sampling operation within it.

And when the results were in we would notice that in some organizations there were many resignations, so that promotion was rapid for personnel that remained; indeed, this effect might be strong enough to hide the effect of growth. We would have to classify organizations into homogeneous groups according to their turnover, or else obtain an index of turnover for each and use regression analysis to "partial" it out. This is only one of many disturbing elements that could be expected to make the results, so painstakingly obtained, uncertain in interpretation in relation to the question to which an answer is being sought.

A simpler approach that would avoid the errors to which a survey is subject (of which sampling error is the least) is to compare the number of employees ahead of a representative individual—let us call him Ego—in a fast-growing and a slow-growing organization, as if promotion depended only on age. Superimposed on individual ability, assiduity, influence, luck, and all the other elements that determine promotion in the real world, is the pure effect of growth on individual careers, and that is what we want to ascertain.

First suppose a given schedule of survival—knowing that the deaths of his contemporaries help Ego's promotion, we do not want differences of mortality to cloud the result of our analysis. Then suppose an age distribution that is a function only of this survival function and rate of increase, so that the stable model described earlier is applicable. Finally, take as the arbitrary benchmark for measurement the age at which individuals arrive at a position where one-half of their fellow-workers are below them and one-half above, say a junior supervisory position.

After that a simple piece of mathematics shows that, for given rates of death or resignation, the age x at which Ego reaches such a position is shortened by two-thirds of a product of two factors:

1. The time from age x to retirement, discounted at the rate of population increase; and
2. The difference between the mean age of the group senior to the point of promotion considered, and the mean age of those junior to it. This difference cannot be far from half of the length of working life.

With an entry age of 20 and a retirement age of 65, the comparison of two populations whose increase differs by Δr percent gives for the difference in ages

$$\Delta x = -(2/3)(15)(22.5)\frac{\Delta r}{100} = -2.25\Delta r$$

Thus the time of promotion is delayed by 2.25 years for each 1 percent by which population growth is lower. That demographic factor is overlaid on all individual differences of ability, influence, and luck. While the model is based on pure seniority, some such effect will apply if any element of seniority is present. Only if length of service in the organization is wholly disregarded in promotion will the model be irrelevant.

Effect of Development on Population Increase

This brief article is not the place to take up intricate issues of population economics, which is an entire academic discipline having scores of specialists, a literature running to many hundreds of articles and books, and its own lines of cleavage and of controversy. It is worth saying enough only to show that both theoretical and empirical methods are applied in this field, and that, notwithstanding their extensive and skilled use, much remains to be done in disentangling the lines of causation. The literature speaks of "development" as the socioeconomic transformation into the modern condition, and of "income" as sufficiently correlated with development to be used as a proxy.

First the effect of development on population: a quick look at cases suggests a familiar negative relation, with which theory conforms. Development seems sooner or later to have brought a reduction in population growth in all the instances where it has occurred. All of the rich countries have low birth rates today, and the very richest are not replacing themselves. For example, West Germany had fewer births than deaths in 1973, and in 1974 it had fewer births plus net immigrants than deaths, so that its population actually declined by 2 percent. But the countries of Eastern Europe are much less rich, and they also have low birth rates, while in Britain the birth rate first started to fall almost a century after development was underway. Thus the correlation is not perfect, but still history seems to be saying that with more or less lag, industrialization has led to reduced family size.

In theory this may be due to women finding jobs and sources of prestige outside the home, so they do not need to rely on childbearing for their standing, and to children being on the one hand more expensive and on the other less directly useful to their parents as income increases, both effects being related to the decline of the family as a productive unit with the growth of industry. With easy contraception, relatively weak motivation suffices to cut the birth rate. What we ought to believe in this matter, summed up in the concept of demographic transition,[4] is relatively unambiguous because the dominant theories and the most conspicuous anecdotal evidence all point the same way.

Yet even here, the more closely and systematically scholars have looked at the data, the less clear they have found the effect of development on family size. Taking income as a proxy for development, Adelman makes "an analysis of fertility and mortality patterns as they are affected by economic and social forces."[5] Her materials, mostly based on national statistics for 1953, show a decidedly *positive* relation between income and fertility. Friedlander and Silver partial out more variables, and find that for developed nations fertility and income are positively related, but for less developed nations negatively.[6] David Heer calculated correlations for 41 countries that suggested that the direct effect of economic development is to increase fertility, and the indirect effects (through education, and so on) are to reduce it.[7] But it makes a difference when the data for the 24 less developed countries are separated from those for the 17 more developed and more than one point of time is introduced, so that changes rather than levels are correlated. Ekanem used two points of time, the 1950 decade and the 1960 decade, but the effect of his greater care seems to be a less clear-cut result than Heer's.[8] Janowitz follows five European countries and finds that variables shift enough through time that the longitudinal relations, more likely to indicate causation, are decidedly different from the cross-sectional regressions.[9]

It would be too unkind to say that these efforts constitute raw empiricism. They are oriented by an economic theory: that increased affluence causes people to buy more of most things, the exceptions being labeled inferior goods. Since no one considers children inferior goods, many argue that children and income "really" are positively related, but the relation is concealed by the intervention of other factors. The better-off have access to contraceptives of which the poor are ignorant; the better-off have higher quality (that is, more expensive) children, and so can afford fewer of them.[10]

Effect of Population Growth on Development

The writers cited above were trying to find the impact of development on fertility where, despite some complications and contradictions, causation seems clearer than in the inverse problem: in which direction and to what extent does rapid population growth affect development? Among all the questions that demographers seek to answer, this is the one that is truly important for policy.

In the classic theory, rapid growth means many children—40 percent or more of the population under age 15 years. The children have to be fed, clothed, and educated, and however the cost is divided between parents and the state, it requires resources that compete with industrial and other investments. In addition, growth requires that provision be made for *increasing* numbers, in particular to equip a larger and larger labor force with capital goods. Thus a fast-growing population is doubly handicapped.

So much for the static aspect of the demographic-economic relationship. As to dynamics, when fertility falls from an initially high level, the dependency ratio begins to shift immediately in an economically favorable direction. Thus investment can be greater compared to what it was before. Lagging 15 or 20 years behind is a longer-run dynamic effect: a slackening of the growth of the population in the labor force ages. When relatively fewer children grow up to enter those ages, there is less competition for productive jobs and each entrant may have more capital to work with compared to the situation that would exist if the birth rate had not been cut.[11]

All this is based on the view that development is capital-limited rather than resource-limited. But if it is resource-limited, population is an even more serious drawback, although now the absolute level of population is the problem rather than the birth rate; the more people, the less resources at the disposal of each, on a theory running back to John Stuart Mill and ultimately to Malthus. In the most general statement, certain ratios of labor to the other factors of production—land and capital—are more favorable than others, and most developing countries are moving away from the optimum with present population sizes and birth rates.

But now try to see how matters would look if no theory had ever been presented. Let us try to wipe theory out of our minds, and look at the data with complete naiveté. Among developing countries, Pakistan is increasing at over 3 percent and India at less than 2.5 percent, yet Pakistan seems to be making more economic progress. Iran's rate of population increase is much greater than Nepal's, and so is its economic advance. Brazil and Venezuela are not increasing in population less rapidly than their economically stagnant neighbors; indeed, Argentina and Chile, with very low birth rates, may be becoming poorer absolutely. Mexico is advancing economically with an annual population increase of 3.5 percent per year, one of the highest in the world and higher than that in Paraguay or Bolivia, where economic dynamism is absent. On the other hand, sub-Saharan Africa has high rates of population increase and low income growth. Figure 1 depicts the broad array of relationships between population growth and increase in income for large countries in the contemporary world. As noted at the start of this article, the relation that theory predicts is not at all evident.

It makes a difference if we compare birth rates rather than natural increase, and for the theory, births less infant deaths might be the best indicator of the demographic impact. But whatever measure is used, the inverse correlation with economic dynamism simply does not appear.

Of course individual countries can be analyzed, and by making allowance for such nonpopulation aspects as leadership, political conditions, the educational system, religion, the dissolving of patrimonial social relations as expressed in landholding and other ways, along with resource endowment, we need not be at a loss to account for the observed national differences. This explanation a posteriori can be made to sustain the theory, but hardly answers the disturbing question: to what extent would naive examination of population and income data for the poor countries of the world have discovered any clear effect of population on development? Would the effect have been as blurred as the effect of population increase on age distribution?

It is just this incapacity of the raw data to speak for themselves that permits some to argue that population and its growth do not harm development and should be allowed to take care of themselves. One might expect the facts to silence anyone who could utter such opinions, but as presented either anecdotally as above or in simple correlations they do not. How can the facts be made to speak loudly and clearly to this issue?

How Nature Covers Her Tracks

The reason for bringing these very difficult matters into the present exposition is the hope that their investigation can be aided by going back to some simpler issues, like the relation between age distribution and the rate of increase of a population. There most would agree that theory gives the right answer: the rate of increase determines the proportion old (as well as middle-aged and young) in the population. Where the relation is obscured by migration or by changing birth and death rates, as it commonly is, these are seen as mere disturbances. Such noise could drown out the relation in the observed data without weakening our conviction that the relation is "really" as stable theory says it is. Up to this point stable theory has the immutability of the laws of logic: if over a sufficient period of time death rates are the same in two populations, then the one with the higher birth rate will have the lower proportion at ages 65 and over. Belief in this is unshaken by El Salvador being higher than Honduras both in rate of population increase and in percent over age 65, or by similar cases that might turn up. The supporter of the theory would convincingly argue that the official data must be wrong (perhaps registration of births is differentially incomplete), or there has been age-selective migration, or some other reason underlies the discrepancy between expected and observed relationships.

Although stable theory can never be disproved, it could be deprived of all interest if in the real world certain things that it assumes constant were in fact steadily changing. If death rates were always falling at a certain pace, then the proportion of old people would everywhere be different from that given by stable theory, and a different theory would be required for interpreting reality. Any steady change that was universal would make us want to replace stable theory with its fixed rates by some other, inevitably more complicated, theory that would have equal force of logic but be more applicable. In fact, change is not so uniform under different real conditions, but is rather erratic, a means by which nature covers up her mechanisms, rendering their interpretation not amenable to a universal theory.

But change, whether steady or erratic, is not the means by which the mechanisms of nature are most effectively covered. More deceiving is the clinging together of variables. Suppose all countries of rapid growth were countries of emigration, so that they lost their young people to countries of slower growth; then the conclusion derived from the application of stable theory would be downright misleading. We would want some other theory, perhaps one on which populations tend to spread out evenly in relation to resources. In fact, such a view is held on internal migration, where free movement occurs and people go to distant places unless they are attracted to intervening opportunities.[12]

As a further example of variables clinging together, we saw that if couples that marry older have more children per year of marriage than those that marry younger, the relation between marriage age and number of children expected by stable theory would be modified. In an extreme case, the number of children per couple could be absolutely fixed without reference to marriage age, say up to 30. This would invalidate the simple connection between age at marriage and the birth rate and tend to frustrate any policy of raising marriage age in the hope of reducing population growth. In practice, this effect is probably very partial if it exists at all in poor countries, and on the other hand a higher age at marriage may well be conducive to women taking jobs and establishing a permanent connection with the world of work outside the household, and so have a twofold effect—both reducing the time available for childbearing and, by directly liberating women, reducing the incentive to have children per year of married life. Empirical investigation of the clinging together of variables can strengthen or weaken a theory.

The Oblique Use of Data to Challenge Theory

In short, challenges to theory have to take the form either of showing that some of the variables assumed fixed move in a systematic fashion, or more importantly, that some variables supposed to move independently in reality cling together; that some of the independent variables are not really independent, but are creatures of other hidden variables of quite different nature.

How then can the classical theory that rapid population growth checks development be challenged? The matter is important because a theory that there is no chance of proving wrong has little value for science.

One way is by declaring that there is a trend toward development everywhere in the world, as well as a trend toward smaller families, and that the latter makes no difference to the former. Suppose the trend to development occurs everywhere sooner or later and nothing can either stop it or hasten it. On this comfortable view of development as immanent in human history no detailed causal theory would be possible, and no policy measures would be sought or needed. Such a view is not entirely absent from contemporary discussion, although in its very nature little evidence can be summoned for or against it.

A more persuasive direction of attack is to adduce evidence that enterprising personalities are more often born into large families and to show quantitatively that this greater enterprise is sufficient to overcome the capital and land shortage due to large families. Or else that couples with more children will have a greater incentive to save and so increase investment funds. Or else that having many children increases consumption but fathers of large families work correspondingly harder and offset this. All of these are statements on the individual level that there is a sticking together of the variables concerned with development—population growth, motivation to work, motivation to save. Nothing in logic proves that the sticking together does not occur, but it is the obligation of anyone who challenges the theory to adduce evidence.

On the national level, the countries that are developing may be the ones in which the authorities are development-minded and persuade their people to make sacrifices that more than offset the disadvantages of population increase. Again, evidence bearing on this specific point would be required.

To take an example that, alas, may not be entirely unrealistic, if dictatorial technocratic regimes are effective in producing development, and if these happen to be lukewarm about population control, then the population effect might be dominated by the dictator effect. But one would only give up the classical theory if there were shown to be some necessary relation between technocratic dictators and development on the one hand and dictators and large families on the other. Otherwise one would still have to insist that the dictator was paying a price for population growth, and the price could be avoided.

Why, then, does the failure of a correlation-type approach to show that development follows on a slowing of population growth present no challenge to the theory? Certainly a purely cross-sectional analysis can at best be suggestive of propositions concerning longitudinal changes, and proves nothing concerning them. Overlooking this, the major difficulty is that many other factors affect the correlations. In principle, the disturbing factor of "motivation to work," or "making sacrifices" could be partialled out or held constant while the relation of population to development is examined. Yet even if one or two disturbing factors could be identified and measured, many others would remain. And to partial out a large number of variables simultaneously raises logical difficulties if any of them are correlated with the variable retained.

What part of the observed phenomena is a manifestation of the underlying causal mechanism and what part is the concealment? Even for the most straightforward matters this is not an easy question to answer. For national populations, one assumes, age distributions are really determined by the rate of increase, and migration or correlated death rates merely conceal this true relation. On the other hand, density-dependent growth is in evidence for many animal populations, so high birth rates might cause high death rates or out-migration. If the correlation of high births with out-migration is necessary, if it is an intrinsic part of the causal mechanism that the investigator is attempting to lay bare, then the stable theory of age distribution is downright wrong; if it is a provisional and temporary complication of the observed data, then the stable theory stands. If autocratic regimes produce development and the same autocratic regimes fail to initiate family planning, this may result in a positive correlation between population increase and rise in income per head, and the student who wants to know what is happening must penetrate to the intermediate variable, "autocratic regime."

After discovering the existence of this intermediate variable, the student would have to judge whether its operation is necessary or incidental. Notwithstanding Hume's proof that necessity is never inferable from finite observation, such judgments are as unavoidable a part of science as they are of common sense. (The difference may be that science makes them tentatively, common sense makes them dogmatically!)

To express the conclusion of this argument in its most radical form: no amount of data showing a gross positive correlation between the birth rate and economic growth can substantially weaken the belief that these two variables are causally negatively related under the economic-demographic conditions that characterize the contemporary world. After all, every country is a unique case; cross-sectional correlations do not carry over into longitudinal correlations; nature has many ways of concealing her mechanisms.

Nonetheless, empirical data have to be applied to check theory, and doing so is the heart of demography as of any other science, but data have to be brought to bear in an oblique fashion. If we are going to detect nature's hidden mechanisms, we need a subtlety that approaches hers. The attack by directly correlating the variables of immediate interest is less promising than a search for what other subsidiary variables stick to them. This applies equally to the analysis of age distribution, the effects of age at marriage on the birth rate, and population and development.

The Psychology of Research

A footnote on the mental conditions in which research occurs may help illuminate the way we get to know the facts of demography. Faced with a variety of data the investigator listlessly surveys them, in the hope of somehow tying them together. He is swamped by the multiplicity of observations and tries to fit them into a scheme, if only to economize his own limited memory. He becomes more animated when he sees that some general connections do subsist in the data, and that a model however crude helps him to keep their relations in mind. The model is much more than a mnemonic device, however; it is a machine with causal linkages. Insofar as it reflects the real world, it suggests how levers can be moved to alter direction in accord with policy requirements. The question is always how closely this constructed machine resembles the one operated by nature. As the investigator concentrates on its degree of realism, he more and more persuades himself that his model is a theory of how the world operates.

But now he is frustrated—he has just turned up an incontrovertible observation that is wholly inconsistent with his theory. Such an observation is truly a fact, an exception to the theory that cannot be avoided or disregarded. A struggle ensues as the investigator attempts to force the theory to embrace the exception. As his efforts prove vain he questions the theory, and looks back again at the raw data whose complexity he thought he had put behind him. The intensity of the struggle that ensues is one of the hallmarks of scientific activity, and distinguishes it from mindless collecting of data on the one side and from complacent theorizing on the other.

The problem and its possible solutions have now taken possession of the person. In this phase of his research his unconscious is enmeshed and is working on the question day and night. Sleep is difficult or impossible; eating and the daily round of life are petty diversions. He is irritable and distracted. Whatever he does, the contradiction he has turned up comes into his mind, and stands between him and any normal kind of life.

During the struggle the investigator is like a person with high fever. Then with luck he comes on the answer, or his unconscious does. He finds a model that fits, perhaps nearly perfectly, perhaps only tolerably, but well enough to provide a handle on the varied data. His tension relaxes, and he goes on with the normal and dull work of establishing the details of the fit and presenting his results. He must indeed revert to a calmer state before he can hope to communicate his finding to an audience that is perfectly normal. An immediate test of his result will be whether it makes sense to his contemporaries; an ultimate test is whether it can predict outcomes involving data not taken into account in the establishment of the model.

Only in exceptional cases will one period of feverish concern produce a final theory and permit immediate relaxation. More often a long series of false starts and disappointments will precede the resolution. Sometimes the problem turns out to be unsolvable in the existing state of knowledge, or beyond the capacity of the investigator, and then he has the unhappy task of winding himself down without the desired denouement.

None of the psychological accompaniment of scientific production is special to demography, but that field may show it in heightened form, at least compared with other social sciences. The abundant data of demography cause an inappropriate theory or an erroneous prediction to stand out more clearly than the corresponding failure in writing history or in the general analysis of society. Where that possibility of a sharp rejection by hard data is lacking, the game of research loses its seriousness—it is like playing solitaire with rules that are adjustable to the cards that have appeared.

Conclusion

The several examples of demographic knowledge and ignorance that form the main body of this paper show that resistance of data to generalization of which E. O. Wilson speaks.[13] In some instances, particularly those concerning age distribution, we have a simple model that fits a variety of circumstances well enough that no one would do empirical research; no one undertakes a research project to see how the fraction over age 65 varies with rate of increase among countries, partialling out size of population, income per capita, race, and other variables. Even to sugest this as a subject of research sounds eccentric, so accustomed are we to the model. Not so in the question of age at marriage, where two wholly opposed models are in people's minds. In one, parents once married bear children at a certain rate, fixed for their age—a passenger in the train passes a given point at the same speed irrespective of where he boarded. In the other, parents want a certain number of children, and proceed to have them once married, whether early or late. Neither is true, but it makes a great deal of difference whether the reality that they straddle is closer to the first or to the second. With breast cancer and the sex ratio of births, we have a mass of data and not even one model of what is happening.

Some apology is needed for introducing an epistemology of demography in an age hostile to metaphysics. After all, we do know how to assess demographic research objectively. The consistency of the theory it uses, the quality of its data, the likelihood that some alternative theory would fit its data better, such criteria are at hand to tell us how good is the workmanship in a particular investigation. Is there any point in attempting to go beyond these aspects of demographic method? I believe there is, even though no quick and simple answer to questions of theory versus data is to be expected. Greater awareness of the basis of our knowledge and judgments cannot but improve both.

Nonetheless the present essay will appear an inadequate and grossly incomplete attempt, even to those who see its objective as worthwhile. Each of us has his own sense of the contribution of theory and data to his knowledge and is enough of an individualist not to conform to the opinion of others. The success of the present article will be measured by the richness of alternative views that it arouses.

Notes

1. It is clear that if $r = 0$, the age distribution of the stable population equals the survival function. This special case is known as the stationary population.

2. This is one of the purposes that Ansley J. Coale and Paul Demeny had in mind when they constructed their tables in *Regional Model Life Tables and Stable Populations* (Princeton: Princeton University Press, 1966).

3. Data are from Nathan Keyfitz and Wilhelm Flieger, *World Population: An Analysis of Vital Data* (Chicago: University of Chicago Press, 1968).

4. For an explication of the demographic transition theory, see Frank W. Notestein, "Population—the long view," in *Food for the World*, ed. Theodore W. Schultz (Chicago: University of Chicago Pres, 1945), pp. 35–37.

5. Irma Adelman, "An econometric analysis of population growth," *American Economic Review* 53, no. 3 (June 1963): 314–339.

6. Stanley Friedlander and Morris Silver, "A quantitative study of the determinants of fertility behavior," *Demography* 4, no. 1 (1967): 30–70.

7. David M. Heer, "Economic development and fertility," *Demography* 3, no. 2 (1966): 423–444.

8. Ita Ekanem, "A further note on the relation between economic development and fertility," *Demography* 9, no. 3 (August 1972): 383–398.

9. Barbara S. Janowitz addresses the longitudinal aspect in "An empirical study of the effects of socioeconomic development on fertility rates," *Demography* 8, no. 3 (August 1971): 319–330. For other aspects of this issue, see Jean-Claude Chesnais and Alfred Sauvy, "Progrès économique et accroissement de la population: Une expérience commentée," *Population* 28, no. 4–5 (July–October 1973): 843; and Edward G. Stockwell, "Some observations on the relations between population growth and economic development during the 1960s," *Rural Sociology* 37, no. 4 (December 1972): 628.

10. Gary S. Becker, "An economic analysis of fertility," in *Demographic and Economic Change in Developed Countries* (Princeton: National Bureau of Economic Research, Princeton University Press, 1960); and Harvey Leibenstein, "An interpretation of the economic theory of fertility: Promising path or blind alley?" *Journal of Economic Literature* 12, no. 2 (June 1974): 467–479; and H. Leibenstein, "The economic theory of fertility decline," *Quarterly Journal of Economics* 89, no. 1 (February 1975): 1–31.

11. Ansley J. Coale and Edgar M. Hoover, *Population Growth and Economic Development in Low-Income Countries: A Case Study of India's Prospects* (Princeton: Princeton University Press, 1958).

12. Samuel A. Stouffer, "Intervening opportunities: A theory relating mobility and distance," *American Sociological Review* 5, no. 6 (December 1940): 845–867.

13. Edward O. Wilson, *Sociobiology: The New Synthesis* (Cambridge, Massachusetts: Harvard University Press, 1975).