THE HISTORICAL STUDY OF VITAL PROCESSES

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Over the last two centuries, almost all western countries have experienced large declines in fertility: the rate at which their female populations bear children. The decline has been more or less continuous; it has often seemed irreversible. Little of the world outside Europe and the areas settled mainly by Europeans has experienced the massive, continuous decline of fertility. During the same period, the same Europe-centered world has undergone industrialization and urbanization to a degree almost unparalleled elsewhere.

The fertility decline, the industrialization and the urbanization have accompanied each other closely enough to encourage the idea that industrialization and urbanization cause fertility to decline. We have plenty of ideas to make such a relationship plausible: the idea that urban-industrial families have less need and less desire for the labor of children than agrarian families do, the idea that contraceptive techniques and information improve as a consequence of advances in communication and in scientific knowledge resulting from industrialization, and so on. In fact, the problem is that we have too many explanations which are individually plausible in general terms, which contradict each other to some degree, and which fail to fit some significant part of the facts.

One recent look at the prospects for a worldwide fertility decline ran:

> Negotiating a transit from high fertility to low fertility levels could prove easier for today's underdeveloped countries, some of which required over half a century to move from fairly high to low levels . . . Today contraceptive methods are far more advanced, often have the active endorsement of the state,

> and are strongly opposed by cults and ideological groups only in some countries. Moreover, high fertility combines with low infant and child mortality to impose a heavier dependency burden on adults than was the case in the West in the nineteenth century, when children required less education and entered the labor force earlier. Urbanization also feeds the revolutionary change in man's aspirations now underway in much of the world (Spengler 1974: 17; the omitted material shows that fertility levels in the poor parts of today's world are higher than they were in most European countries when those countries began their nineteenth-century fertility decline).

Now, this statement deals with opportunities rather than established regularities. Nevertheless, its plausibility rests on a series of causal arguments: 1) that the efficiency of available contraceptive technology, the moral and political support for contraception and the extent of dependency all accelerate the decline of fertility, 2) that the worldwide "change in man's aspirations" reduces people's desire or willingness to have many children, 3) that these changes in goals and in the means available to the goals are the chief factors in the decline of fertility. In such an argument, urbanization and industrialization affect fertility through their impacts on general aspirations, attitudes toward children, burdens of dependency and contraceptive technology.

The connections are plausible. Yet in the present state of knowledge it is debatable whether the connections are strong or consistent. It is debatable whether they are the chief connections between urbanization and industrialization, on the one hand, and fertility change, on the other. In

a recent authoritative collection of essays on fertility and family planning, Simon Kuznets speaks of the "insensitivity of fertility levels to wide differences in economic and social factors despite the marked contrast between the [less developed] and [more developed] groups, each taken as a whole, with respect to both social factors and fertility", while Ansley Cole concludes:

> Perhaps we shall through a stroke of insight or good fortune discover a grand generalization that will provide a compact and widely valid explanation of the decline of marital fertility in Europe. But at the moment it appears that the process was more complex, subtle, and diverse than anticipated; only an optimist would still expect a simple account of why fertility fell (Behrman, Corsa and Freedman 1969: 159-160, 19).

Both in accounting for the contemporary distribution of high and low fertility in the world and in accounting for the pattern by which fertility fell in the West, then, we have a surfeit of interesting partial hypotheses and a dearth of successful general formulations.

In this book, we look hard at some portions of the western experience. By studying particular experiences closely, we hope to get a sense of the actual process by which fertility changed and to start ruling out a few of the available hypotheses. By turning to historical material, we hope to find rich, continuous documentation over the substantial blocks of time which major fertility changes require. By being explicit and punctilious about the models and methods we employ, we hope to link what we learn about particular experiences in Europe or North America both with comparable experiences elsewhere and with general arguments -- other people's and our

own. The book contains a series of general arguments, a number of historical illustrations of the arguments, a body of systematic evidence drawn from the historical experience of western Europe, and a smaller body of evidence from the United States. Each of the elements is incomplete. Nor do all of them fit together perfectly.

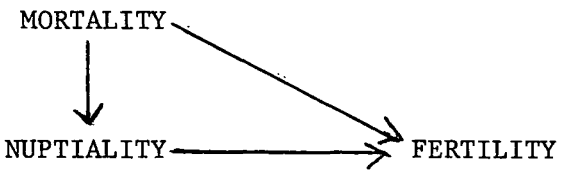
The purpose of this first essay is to provide a context for the other papers, to specify some of the ways in which they are incomplete, and to identify some points of contact among them. It is sweeping and speculative where they are prudent and precise. The purpose is still the same: to make connections between western historical experience and alternative general ideas about the determinants of changes in fertility.

Vital Processes and Collective Biography

The basic vital processes are fertility and mortality: the beginning and ending of life. Vital statisticians center their attention on fertility and mortality. They cannot, however, avoid dealing with three other fundamental demographic processes: migration, social mobility and nuptiality. Migration enters into analyses of fertility and mortality because the movement of people into or out of a locality affects the liability of the population to pregnancy and its liability to death. Social mobility -- that is, the movement of people from category to category rather than place to place -- similarly affects who is at risk to pregnancy or to death in any particular category; furthermore, since categories of people vary in their propensity to family planning, in their access to medical services, and in many other regards which affect their fertility and mortality, a large shift of people from one category to another sometimes alters the vital characteristics of the population as a whole. Nuptiality -- the

movement of people into and out of marriage--significantly affects fertility, since the great bulk of human conception, and an even greater share of human childbearing, takes place within marriages.

Mortality, nuptiality and fertility form a kind of hierarchy. The existing level and pattern of mortality set limits on who can marry, at what ages. They also affect the numbers and age distribution of women in the childbearing ages. The level and pattern of nuptiality set limits (not so stringent as in the case of mortality) on the portion of the female population likely to bear children. Over the long run the pattern of fertility helps determine who is there to marry or die. There may also be an effect of nuptiality on mortality because marriage somehow affects one's life expectancy. But in general nuptiality constrains fertility, while mortality constrains both nuptiality and fertility. Thus the main relationships which interest us are:



Other variables affect the core set. I have already mentioned migration and social mobility. Non-demographic variables also matter. For example, over the range of human history, the relative abundance of food has significantly affected the rates at which people have died, married and borne children. Despite the situation in the contemporary world (in which food-poor countries have high mortality, but also have high fertility and appear to have high nuptiality as well), the main historical tendency has probably been for food shortage to raise mortality, and to depress nuptiality and fertility. Again, the urbanization of a population seems to affect the

patterns of birth, death and marriage alike.

Births, deaths and marriages are events happening to individuals, while fertility, mortality and nuptiality are the resultants of those individual events at the level of a population. At the individual level we have the number of children ever born to a particular woman or the age at death of a particular man. At the level of the population we have a total fertility rate or an expectation of life at birth. The neat thing about the demographic analysis of vital phenomena, in fact, is that it permits us to deal with the individual and the group at the same time: first, by specifying the logic by which the one is aggregated into the other; second, by permitting us to compare the experience of any particular individual with that of the population to which the individual belongs.

Because of the explicitness and precision with which demographic analysis performs this aggregation and disaggregation of events, it provides an interesting model for a wide range of historical investigations. Historians who really want to talk about groups often find themselves surrounded by information about individuals. They have several standard ways of jumping the gap: by concentrating on spokesmen, leaders or elites within the population at hand, by pulling out "typical" individuals, by relying on the testimony of expert observers of the groups in question, by reporting and illustrating general impressions from long contact with individual records.

Recently, the effort called "collective biography" has offered a more systematic alternative. Collective biography consists of recording features of the life histories of considerable numbers of individuals in a uniform fashion, then aggregating the individual records into a collective portrait of the group and its structure. The collective portrait may consist mainly

of averages: mean income, moves per year, median size of a household. It may consist of measures for which there is no precise individual counterpart: income inequality, net reproduction rate, proportion of the labor force in agriculture. It may involve the sorting of the population into its major subdivisions: old vs. young, rich vs. poor, rural vs. urban. It may even reconstruct the relationships among individuals, for example by placing them within their distinct lineages. As the population under study becomes large, the portrait almost necessarily becomes quantitative. It also becomes more and more advantageous to let a computer do the collating and counting. In general, the payoff from collective biography--as compared with other ways of drawing general conclusions from multiple human experiences--rises as the number of persons increases, as the different records containing information about the same individual multiply, and as the general arguments being made are explicit and precise.

At first view, collective biography seems like a very inductive procedure: plug in the individuals and watch the patterns emerge. In fact, collective biography has proven most valuable to historians where someone has already developed an explicit and interesting model of the phenomenon under examination.

The historical study of social stratification and mobility is a good example. In that field, scholars disagree vehemently about the appropriate models, but they commonly work with explicit models of social hierarchies and of movement within the hierarchies. As a consequence, we have (among other things) a series of roughly comparable analyses of American cities which contradict the idea of a great expansion of opportunity in the twentieth century, yet establish both the abundance of minor movements up

and down the social scale and the rarity of movements from rags to riches or riches to rags. Other versions of collective biography have yielded interesting results in the historical study of elites, elections, legislative behavior and political conflict.

Historical Demography

The most resounding results, however, have come from historical demography. The discipline of demography began to take shape in the eighteenth century as a way of analyzing historical changes in population sizes and characteristics. Yet by the 1930s the discipline had become largely ahistorical in its concerns and its procedures. After World War II demography moved back toward its historical origins. The increasing desire to compare the current demographic experience of non-western countries with the past experience of the West promoted historical studies. The rising interest in identifying the demographic components or counterparts of the processes vaguely and optimistically called "development" augmented the possible return from studying long, well-documented historical population changes. And a series of technical innovations in demography and history made the demographic analysis of historical materials increasingly feasible and profitable. The innovations included the refinement of stable population models, the elaboration of procedures for making demographic estimates from incomplete data and the development of computer routines for the collation of large numbers of observations. They emphatically included the introduction of collective biography.

Two new practices brought collective biography to the center of historical demography. Both owed a good deal to the French demographer Louis Henry. The first was the use of genealogies to produce demographic

estimates for whole populations. The second was the application of essentially the same procedures as had been used to analyze genealogies in the construction of demographic estimates from historical records of births, marriages and deaths. In Henry's version of "family reconstitution," the investigator cumulated individual registrations of vital events into dossiers which related the members of a nuclear family to each other and grouped together the scattered references to the same individual. For families which remained in the population under observation for long periods, it was then possible to reconstruct such matters as the total childbearing experience of a given woman, the frequency of premarital conception of live births and the age at marriage of a family's children--even where the individual records of vital events lacked those items. Historical demographers began to produce long, fine, fascinating series of demographic indices for periods before the census, before governmental imposition of vital registration, indeed before the emergence of demography itself.

What is more, the results proved important to historians and demographers alike. The historians, for example, not only acquired demographic series to relate to the observations of wages, prices, production and political change they had long been accumulating, but also discovered that an agrarian world they had considered relatively immobile and isolated was swarming with geographic mobility and quickly responsive to changes in economic conditions. The demographers acquired strong evidence of controls over fertility and nuptiality in periods and places which should, by widely-held hypotheses, have displayed high, fairly stable, socially uncontrolled rates. Both historians and demographers gained access to a body of materials and procedures which permitted far stronger tests of their assertions concerning

long-run population changes than they had ever been able to manage before.

The important yields from parish registers of births, deaths and marriages (or, more precisely, of baptisms, burials and weddings) led historical demographers to search for other documents containing related information. They found them. Three main classes of documents contain information lending itself to systematic demographic analysis: 1) population enumerations, 2) registration of vital events, 3) by-products of private transactions.

The census is the contemporary quintessence of the population enumeration. There are few usable censuses anywhere prior to 1800. For the period before 1800, historians have uncovered an abundant supply of the census' ancestor: the enumeration carried on by a large organization for the purpose of identifying resources available to it. The organizations involved are mainly governments, but they include churches, estates and others. The resources in question are most often property of some kind, yet they sometimes include labor power and special characteristics of the population as literacy or military experience. Fiscal records--assessment rolls, records of payment, and the like--are the chief variety. Conscription registers, cadasters, rent books, enumerations of the poor all have their place. Sometimes these sources contain not only enumerations of the people to be taxed or drafted, but also descriptions of their households and summaries of their health, marital status, and so on. Used in conjunction with other documents, even those which only contain one characteristic of the individual will serve to establish the individual's presence or absence at different points in time.

The registration of vital events became a regular activity of western

governments during the nineteenth century. Before then some governments (for example, the Swedish state) registered births, marriages and deaths, but churches played the larger part in recording them. In particular, the Roman Catholic Church required the maintenance of registers, parish by parish, from the seventeenth century onward. The Catholic registers and their Protestant counterparts have survived in abundance; they have served historical demographers well.

The by-products of private transactions which serve demographic purposes include marriage contracts, testaments, deeds and the documents resulting from the settlement of an estate. This class of records is more heavily biased toward the rich and powerful than are the population enumerations and registrations of vital events. They have some compensating strengths: 1) they often identify a whole kin group, plus quasi-kin such as godparents, at the same time; 2) there are times and places in which most of the population seals a marriage with a contract or divides up an inheritance by written agreement; 3) where the population under study is, in fact, an elite, the by-products of private transactions supplement the standard demographic observations with rich evidence concerning wealth, personal connections and even quality of life.

The historical sources containing population enumerations, registrations of vital events and by-products of private transactions are rich. More are coming to light every year. But they are distributed quite unevenly. The existence of repeated demographic observations in more or less comparable form depends very much on the presence of large organizations which persist for substantial periods of time. Churches, governments and estates are the best historical examples. Business firms become

important producers of some kinds of continuous series in the nineteenth century.

Where such organizations are rare or weak, the historical record is correspondingly thin and heterogeneous. On the whole, that means the further back in time we go, the more ingenious and determined we have to be in locating the sources and reworking them into comparable form. It also means that with historical records it is often uncertain to what population the documents refer: Do these tax rolls cover the entire population, for example, or just the portion with enough wealth to pay taxes?

In fact, the population covered is characteristically the clientele of a large organization, rather than a population in which demographers would take an immediate interest. The problem is then to reconstruct the behavior of a demographically interesting population--a community, a class, a labor force, or something else--from observations of a large organization's clientele. A significant part of the historical demographer's expertise therefore consists of knowing the conditions under which the documents at hand were produced and reconstructing the operations of the organization involved. A significant part of the historical demographer's work consists of estimating or correcting gaps, errors or distortions in the record before drawing inferences from the observed population to the population of genuine interest.

The abundance and richness of historical records makes it possible to use many of them as one would use current vital registration: to detect class and regional differences in mortality, to sort out the relationship of women's childbearing patterns to their ages at marriage or to the mortality of their previously-born children, and so on. In this regard, the

records simply bring new and interesting populations to the attention of demographers. But there are some problems for which historical materials have definite advantages over contemporary censuses, special surveys and vital registration.

To the extent that the demographic processes in question take a long time to work themselves out, the relevant evidence necessarily reaches back into history. If we are to verify or modify theories of the "demographic transition" from high to low fertility and mortality, for example, we have no real alternative to assembling comparable series over periods spanning a great deal of urbanization, industrialization and demographic change; the comparison of different populations at the same point in time as if they marked successive points in a standard progression can never answer the question of whether such a progression actually exists.

Likewise, if the arguments at hand concern life histories rather than individual events, historical records provide fuller and more reliable evidence than can practically any contemporary source. If the great computerized data banks so many of us fear come into existence, they will compete seriously with the materials of historical demography. In the meantime, the demographer who confines himself to contemporary evidence must make do with retrospective reports which people give in interviews or offer when registering births, deaths, marriages and other crucial events. Or he can settle for the thin, incomplete life histories accumulating in the files of existing organizations. For my part, I have serious objections to digging the records of living people out of such files, and even more serious objections to linking the information concerning individuals in one file with the information in another file. (If the procedure for consultation of the records guarantees anonymity, shields the individuals involved from direct

consequences of the consultation and/or requires the prior consent of the individuals, my objections diminish.) The methods of collective biography, by contrast, permit the construction of rich, complete and demographically informative life histories from historical records without invading the privacy of living persons.

Finally, the secular trend is of interest. It is worth knowing when and where the world set off at its present dizzy pace of population growth, and how many times (if ever) the same sort of expansion has happened before. In the more immediate area of this book, it is important to know whether the nineteenth century fertility decline of western countries followed centuries of high, stable fertility, came as the largest of recurrent declines in fertility, or followed--as some evidence suggests--an extraordinary eighteenth-century rise in fertility. By the same token, it is useful to have historical time-lines of mortality, fertility, urbanization and migration with which to compare the recent experiences of the world's rapidly-urbanizing countries.

So there are some circumstances in which historical materials are not simply a supplementary source of demographic data, but the principal sources one wants to consult: where one's questions concern long-term processes, where the relevant evidence requires full life histories, where the secular trend is itself at issue. The essays later in this volume illustrate all three circumstances, especially the first.

As our essays also illustrate, historical materials do not serve all purposes equally well; they have some characteristic drawbacks. Where intentions, beliefs and knowledge figure prominently in the argument, historical records rarely contain the direct testimony on these matters which

a skilled interviewer elicits from living respondents. Many details of private life escape the written record. When it comes to contraceptive knowledge, desired family size, sexual practices or aspirations for children's careers, the historical evidence is almost always indirect. In these regards, our understanding of times before the twentieth century depends largely on literary treatments, testimonies of supposedly expert observers, penalties inflicted on transgressors, and inferences from such observable phenomena as child-spacing and intervals between marriages and first births.

Recent Work in Historical Demography

The discussion so far has another implication. Most of the last two decade's work in historical demography has been descriptive rather than analytical. It has consisted mainly of locating suitable sources, devising procedures for squeezing reliable demographic estimates from the sources, making the desired estimates, then using the resulting series and cross-sections to formulate or corroborate verbal arguments concerning the populations. It has not consisted of the formulation, estimation and testing of rigorous models.

The description is essential. Before national income analysis could become an effective tool of development theory, economists had to spend several decades conceptualizing national income, devising the necessary measurements, and accumulating reliable series for relevant populations. Historical demographers have been doing the equivalent of that work.

What is more, the descriptions have often proved valuable in themselves. For example, E.A. Wrigley's painstaking reconstruction of vital rates in the village of Colyton established that rural marriages and births responded

sensitively to changing economic opportunity for centuries before the age of chemical and mechanical contraception. The accumulating weight of descriptions for European and American populations before the nineteenth century has crushed the idea that a shift from "natural" or "uncontrolled" to "controlled" fertility came with mature industrialism. Again, the great rapidity with which European populations turn out to have recouped heavy losses to mortality in plague and famine has made explanations of major population shifts in terms of such catastrophes less plausible than they had been. The descriptive work has established that long before the industrial age a large region of southern and western Europe displayed what John Hajnal has called the "European marriage pattern": relatively later marriage for females, many people permanently unmarried, many households containing unmarried adults as well as a married couple; this arrangement sets early modern Europe off from the rest of the world, and may well have made the region's permanent shift to low fertility easier to manage. Finally, the descriptive version of historical demography has become a standard tool of historians who have no abiding interest in demographic issues as such; following the lead of Pierre Goubert's studies of Beauvais and its region, they have the means of incorporating information about the life experiences of ordinary people into accounts of the economic, social and political transformation of a village, city or region.

By analytical work I simply mean work which asserts a regular relationship among two or more variables, and attempts to test the asserted relationships by means of reliable evidence. One reason that little of the work in historical demography to date has been analytical in this sense is that the questions which first drew investigators into the effort were

largely descriptive: when and where did fertility begin its long-run decline? Did pre-industrial cities have high levels of mortality? Another reason is that the data for dealing with several variables simultaneously were slow to produce. And a third reason is that a major part of the empirical work was done by historians and others who were not accustomed to the formal modeling and testing of the relationships which interested them. The arrival of quantitatively-trained economists, economic historians and sociologists on the scene accelerated the analytic work in historical demography.

One simple illustration comes from the work of Dov Friedlander (1969, 1970). Friedlander adopts a version of Kingsley Davis' account of the demographic transition. He takes from it the idea that a rural population which experiences great population pressure (e.g. as a consequence of declining mortality) tends to respond either by lowering fertility or by accelerating out-migration. The greater the opportunities for out-migration, the longer rural fertility will remain high. Friedlander provides a set of hypothetical calculations showing how the two alternatives would work. Then, using already-published data, he argues that the British and Swedish experiences conform to the two alternatives: the British urbanizing early, absorbing plenty of rural population through out-migration, and experiencing relatively late declines in rural fertility, the Swedes urbanizing late, having relatively little rural-to-urban migration while mortality declined in the countryside, and experiencing substantial rural declines in fertility before large-scale urbanization.

Later we shall encounter two related difficulties in this approach. First, if we have the <u>agricultural</u> population in mind, the formulation neglects

an alternative which often occurred in western countries: the movement of agricultural workers into manufacturing within the countryside. Second, as a practical matter it is very hard to measure "population pressure" independently of the responses it is supposed to produce. Still the Friedlander argument immediately suggests extensions to other parts of Europe--including France, a relatively late urbanizer and a classic case of early fertility decline.

The Friedlander work lies halfway between description and rigorous analysis; although the central model is fairly precise, Friedlander neither estimates its parameters nor tests its fit to the available data. It is nonetheless a useful example; it shows the intersection of theories about the demographic transition, contemporary demographic models and procedures, and historical evidence.

The "Demographic Transition" Today

The problem of demographic transition dominates the historical study of vital processes today, as it has for thirty or forty years. In their baldest, non-technical form, the pressing questions are:

1. How and why did the populations of western countries move from high levels of fertility and mortality before 1750 to low levels of fertility and mortality after 1900, while almost none of the non-western world went through the same experience?
2. To what extent is the general process (or, failing that, particular relationships within it) generalizable to populations currently undergoing urbanization, industrialization and intensification of communications flows?

In his classic brief statement of 1925, A.M. Carr-Saunders did not regard either of these as greatly problematic. "There is no mystery about the

fall in the death-rate," he declared. "It was due to improved sanitary conditions and to advances in the study of medicine." (Carr-Saunders 1925: 40). The decline in the birth rate was, he thought, somewhat more complicated, but "While it is impossible to estimate the prevalence of contraceptive practices and of abstention from intercourse, it is probable that they account for the whole of the decline which the figures show." (Carr-Saunders 1925: 42).

In general, Carr-Saunders argued that increased economic efficiency enabled and encouraged a population to expand through increases in fertility, while signs of diminishing returns from technical innovation led the members of the population to restrict births by one means or another. Although he did not formulate the problem of future population growth in terms of what would happen as new areas of the world industrialized or urbanized, he held out the hope that the same semi-conscious process of adjustment would occur elsewhere.

Over the next two decades, western students of population paid rather more attention to the steadily declining growth rates of their own countries than to the accelerating growth rates of the non-western world. "Inadequately explored in a still-Kiplingesque West," writes Joseph Spengler, "were implications of the fact that while the rate of population growth in a politically fissured Western world was falling, that of underdeveloped lands, containing about two-thirds of the world's people, was incipiently high and potentially rising" (Spengler 1972: 339).

The idea of a regular and general demographic transition crystallized: "modernization" quickly and decisively depressed mortality, mainly through the development and diffusion of a) new aspirations which were incompatible

with the bearing of large numbers of children, b) contraceptive knowledge. The debate remained open about which features of "modernization"--urban life, for example, or the opening of individual careers requiring an investment in education--really mattered. However it worked, the consequences were clear: accelerating natural increase during the period in which mortality declined faster than fertility, then declining natural increase as "modernization" proceeded still farther. Presumably the same cycle would occur in the poor parts of the world if they could properly begin the process of modernization.

Since World War II, the terms of the discussion have altered--but not fundamentally. The idea of an early, general mortality decline resulting from technical change has persisted, despite some doubt about the life-saving effects of medical improvements before quite recent years, despite increasing emphasis on a reliable food supply as a life-saver, and despite the realization that in the contemporary world governments were introducing controls over disease in areas which showed no signs of "modernization" in most other regards; the theory made these areas dubious candidates for the next steps of the demographic transition.

Another of the original ideas has persisted: that the transition occurs--if it occurs--through the widespread conversion of married couples to the deliberate, efficient control of births. The major alterations in the discussion since World War II have been: 1) the recognition that levels of nuptiality and fertility are much higher in important parts of the contemporary non-western world than they were in most of the West before the nineteenth century's massive fertility decline began, 2) the growth of the idea that high rates of natural increase (notably those resulting from

the deliberate control of mortality in poor, high-fertility populations) in themselves block the economic path to the situation in which increases in production, in the long run, actually depress fertility, 3) an increasing insistence on government policy as the means to population control. W.D. Borrie made a characteristic recent statement:

> With regard to the basic requirement of food, the race between Malthus' hare of population growth and tortoise of food production still goes on, with the latter showing some signs at least of catching up a little. The next step, which is now being recognized in the forward planning of high growth regions is to sustain the balance between food production, social investment and industrial investment. Thwarted by their failure to reach this desideratum in face of ever expanding population growth rates, many of the 'developing' countries have now, as we have seen, turned to a new line--in Malthusian terms how to persuade the hare to go to sleep for a while. The limited success so far should not be interpreted as inability to bring about curbs to growth. The experiments now being tried are at most a decade old. The demographic transition of today's 'developed' countries of western and northern Europe and Europe overseas took from fifty to seventy years to accomplish. The trends this century in eastern Europe and Japan are reminders that events can move faster in the twentieth century. The new element in the present situation of the 'developing' areas is the widespread determination of governments to act and to lead their people toward the goal of population control...(Borrie 1970: 294-295).

Thus the question is still whether and how the poor countries of the world can recapitulate the demographic experience of the rich countries. But now the pressing questions for research appear to be first, under what conditions do married couples actively restrict births? Second, does "modernization" produce those conditions in a reliable, regular way? Third, to what extent (and how) is the production of those conditions a feasible object of government policy?

Historical demography is unlikely to produce firm answers to the third question. But it has some capacity to answer the first two. In fact, the two questions have dominated the analytical agenda of recent historical demography. The historical study of fertility has overshadowed the study of mortality, migration, social mobility and even nuptiality. The recurrent hope of historical demographers has been to develop an account of fertility change which would simultaneously a) explain fluctuations before the nineteenth century, b) clear up uncertainties about the western demographic transition and c) shed light on the populations of the contemporary world.

The Historical Agenda

While historical demographers have concentrated on the demographic transition in general and fertility in particular, other investigators have applied demographic procedures to a much wider range of historical problems. In so far as this work concentrates on determining what happened in particular times and places rather than describing or analyzing some general demographic phenomenon, we can conveniently call it "demographic history" instead of historical demography. The distinction is not precise, but it is useful.

I shall make no attempt to summarize the history of demographic history. Nor shall I try to prepare a comprehensive outline of its subject matter. Instead, I want to mention some large clusters of historical problems which have already attracted demographic attention. They are likely to attract a good deal more. I concentrate on European experience, with side glances at North America. That concentration ignores excellent work being done in Asia and Latin America, but it allows me to show connections and to work with the material I know best.

At the base of the whole heap of questions comprising contemporary demographic history lies one huge question: how did fundamentally agrarian populations turn into essentially urban-industrial populations? The question applies throughout the world. If we want to deal with large populations and nearly-completed transformations, however, we must confine our attention to Europe, its extensions, and Japan.

In Europe of the last five centuries or so, the question is more precisely how a predominantly peasant population turned into an urban-industrial one. The narrowing to peasants matters. Let us employ the term in a strict sense: members of households whose major activity is farming, which produce a major part of the goods and services they consume, which exercise substantial control over the land they farm, and which supply the major part of their labor requirements from their own energies. Nomads, hunters, fishermen, plantation laborers and many other rural workers drop out.

In that narrow sense, the world's major areas of peasant agriculture have been China, Japan, India and Europe. Occasionally someone makes a supplementary case for Central America, Indonesia or parts of

Africa. Peasant agriculture in this sense rarely (or never) appears in the absence of cities, extensive markets and large-scale structures of political control. The narrowing matters here for two reasons: 1) We have reasons to believe that the demographic characteristics of peasants differ significantly from those of other rural populations. 2) In the case of Europe and its extensions, the distinction of peasants from other members of the rural population helps identify profound alterations in rural social life and in the composition of the rural population which occurred while the population as a whole remained mainly rural.

Both points will receive plenty of attention later in this book. We shall, for example, repeatedly consider the possibility that the European peasant household (or the peasant community, or both) operated as an effective population-control mechanism, closely matching the opportunities for marriage and procreation to the number of persons the land could support. We shall also encounter evidence that rural wage-labor expanded considerably before any substantial urbanization of Europe and its extensions, and that the expansion of rural wage-labor tended to weaken the peasant system of population control. In fact, the arguments of this book suggest an unexpected, paradoxical extrapolation to the contemporary world: that the poor, economically dependent populations of the world are repeating the demographic experiences of the <u>proletarian</u> segments of western rural populations under conditions of more complete proletarianization and more thorough penetration of capitalism than occurred in the rural west. More on that later.

The transformation of peasant into urban-industrial populations is an old, old preoccupation of western historians. Demographic historians did not discover it. There are, however, new advantages to stating the problem demographically. First, we are now beginning to accumulate the demographic evidence which can make the analysis of the transformation more than a vague metaphor. Second, the demographic statement of the problem helps specify what there is to explain. For example, it is a long leap from the observation of a net loss of peasants accompanied by a net gain of urban workers to the conclusion that peasants moved off the land into urban factories. If peasants themselves made the move, it is at least possible that they experienced the shock of uprooting, unfamiliar surroundings and unpleasant work routines and responded to the shock with despair, disorganization or rebellion. But the net shift is also compatible with a chain of moves: peasants into rural wage-labor, rural workers into urban services, city-born children of rural migrants into factories, and so on.

Many historians--and even more sociologists, political scientists and economists seeking to build historical support for their analyses of development--have offered the first interpretation. I consider the second more likely. Whether either interpretation is correct, however, does not matter much here. What matters is that demographers almost intuitively ask the essential intermediate questions: What part did differential fertility and mortality play in the population shifts under discussion? What was the size and composition of the various flows between industrial sectors and between rural and urban areas? Did the

patterns of fertility, nuptiality and mortality themselves change as a consequence of the flows? The demographer brings to such historical questions an accounting framework which helps specify the when, where and how.

The demographic side of the inquiry into the creation of urban-industrial out of peasant populations breaks into three kinds of questions: 1) connections of population growth and economic change, 2) components of growth and compositional changes, 3) small-scale processes. Let me take up and illustrate each one in turn.

Population Growth and Economic Change

Any attempt to generalize about population growth and economic change immediately confronts contradictions. Over the long run, population growth and economic expansion generally accompany each other. Likewise, economic decline and demographic contraction tend to occur together. In the short run, fertility and nuptiality tend to respond positively, mortality to respond negatively, to upswings in economic well-being. Yet the demographic transition associates declining fertility and mortality--and, eventually, decelerating natural increase--with economic growth. In order to make consistent statements, we have to disaggregate: different rules for different time-scales, no doubt; different generalizations for different vital processes, certainly; perhaps different arguments for different populations and eras as well. Then it may be possible to see that all the regularities result from the operation of the same elementary principles in varying circumstances. At present that is a hope, not a promise.

One common way of disaggregating the problem has been to concentrate on shorter-run fluctuations in vital events: seasonal, annual or cyclical. Far more of the short-run studies have dealt with fertility and nuptiality than with mortality. Available studies divide into those concentrating on the relationships among strictly demographic variables and those treating demographic fluctuations as possible responses to economic fluctuations. The first is the particular province of demographers. It includes a high proportion of sophisticated theoretical work and another high proportion of painstaking measurement, but not much testing of models. A characteristic essay in this vein is Roland Pressat's decomposition of births in any particular year into three factors: the age composition of the female population, the lifetime fertility of the various female birth cohorts at risk to have children in that year, and the fertility level attributable to that year as such. Starting from there and using well-known data for white American females, he makes a plausible case that from 1920 to 1930 the annual fluctuations in births included a significant tendency for lifetime fertility to decline from one birth cohort to the next, while during the 1930s the best estimate of the annual change in lifetime fertility is 0 (Pressat 1969).

Again, Gösta Carlsson's analysis of variations in Swedish marital fertility from 1830 to 1879 (Carlsson 1970) deals directly with the impact of nuptiality on fertility fluctuations, then goes on to propose arguments linking short-run rises in marital fertility to economic well-being. The main statistical results are a demonstration that short-run

fluctuations in births occur in partial independence of the marriage rate, and a strong suggestion of birth control within Swedish marriages of the nineteenth century. Carlsson does not take the next logical step: the direct modeling and measurement of the relationship of fertility to economic fluctuations.

Others do. In one of the most sophisticated and comprehensive demographic treatments of the subject, Henri Léridon (1973) concentrates on month-to-month variation in fertility. Once he clears away various statistical obstacles with exquisite precision, he arrives at findings which are mainly negative or uncertain. Definite seasonal patterns appear in series from France and elsewhere, it is true. They are remarkably constant from one year to the next. But the differences among months are small compared to those reported for old-regime Europe or for poor agrarian countries of the contemporary world. Differences in seasonal patterns among countries and among social classes are declining. Once corrected for seasonal effects, month-to-month variation in fertility from 1950 to 1969 shows no significant relationship either to earlier fluctuations in marriage or to economic variables such as industrial production, employment and savings.

The last set of findings may surprise people who have been reading analyses of annual and cyclical variations in fertility. Although there is some dissent (e.g. Sweezy 1971), the bulk of the available theorizing and the mass of the available statistical results attribute a significant positive effect on fertility to economic well-being. K.G. Basavarajappa's concise summary of his analysis of Australia says, "An analysis of age-

specific marriage and age-duration of marriage-specific confinement rates showed that, during the interwar years (1920-21 to 1937-38), the movements in these rates were very closely associated with the movements in economic conditions" (Basavarajappa 1971:50). In the case of Italy from 1863 to 1964, "in the first seventy-five years...the conformity among marriages, births and business cycle is rather high and without trend either in its intensity or in its direction...the recent two decades bring an attenuation, rather than a consolidation, of the concordance between business cycle and demographic phenomena..." (Santini 1971: 581).

Likewise, Morris Silver follows up his similar analysis of the United States with reports for the United Kingdom 1855-1959 and Japan 1878-1959: "Births and marriages in the United Kingdom conform positively to ordinary business cycles. The cyclical response of births is not simply a reflection of cyclical fluctuations in marriages; it is, at least in part, a direct response. These conclusions also seem to hold for births in Japan and possibly for marriages. In addition, births in both countries seem to conform positively to Kuznets cycles in national income" (Silver 1966: 315). He finds no evidence that the strength of the cyclic response changed over time.

Like most of the work which boldly relates short-term fertility fluctuations to changing economic conditions, Silver's analysis rests on a rudimentary model: a stream of births moves in response to changing national income, which presumably represents the opportunities and costs impinging on couples capable of having children. He uses relatively simple detrended regressions to estimate the basic relationships. To find work

which attempts to specify the entire process connecting fertility to changing economic opportunity, we must turn to theoretical syntheses such as the one Richard Easterlin provides in this volume. Easterlin's own empirical investigations of American fertility fluctuations (e.g. Easterlin 1973), for example, work with incomplete models and only estimate a few of the relationships involved. We shall encounter other efforts to model the economic conditions affecting fertility when we come to the study of the small-scale processes.

We have a longer-run version of the same problem: if growth promotes population increase, how does that happen? Karlheinz Blaschke's massive study of the Saxon population from 1100 to 1843 brings out a contrast which is now standard in European demographic history: purely agricultural zones with a limited holding capacity, exporting their irregular natural increase to cities and to industrial regions; rural industrial zones of almost unlimited absorptive capacity:

> In these areas social differentiation developed early and to an extreme; the especially fast growing segment of their population, moreover, was the people whose basic economic activity was in handicrafts. This segment of the population and these areas in general provided the starting-point for a genuine industrial development in the nineteenth century; the industrial revolution could attach itself without a break to the existing structure (Blaschke 1967: 231).

Blaschke's study has the characteristic strengths and weaknesses of the historical literature: a fine sense of time and place, plus coverage of a great span of change; little specification of the exact

demographic mechanisms by which changes occurred. The obvious sequel is a closer study of the interplay of mortality, fertility, nuptiality and migration in at least some portions of Saxony's 750-year transformation.

Much of the existing work in demographic history works in the other direction, seeking the consequences of population growth instead of its causes. In speaking of eighteenth-century England, H.J. Habakkuk enumerates five ways in which the substantial population increase may have stimulated economic growth: by producing economies of scale, by making cheap labor abundant, by inciting a search for new methods to substitute labor for capital and natural resources, by promoting investment and by inducing extra effort from cultivators (Habakkuk 1971: 47-48). Not all of these strike me as plausible, or consistent with the others. In any case, they cry out for explicit modeling--including the representation of effects in the other direction, from economic growth to population increase. The successful modeling and testing of these relationships will be of the greatest interest to students of today's poor but fast-growing countries.

The same is true of the more rigorous (but no less controversial) argument Ester Boserup has applied to agriculture. Noting the association of high population density and highly-productive agriculture, Boserup argues against the basic Malthusian assumptions of an inelastic supply of land with diminishing returns from intensification. More exactly, she argues that under population pressure the inputs of labor (as exemplified by clearing of wastes or introduction of irrigation) tend to increase

sufficiently to override the diminishing returns due to the quality of land brought under cultivation. Therefore population increase stimulates agricultural productivity.

Coupled with the recent arguments (for instance, those of E.L. Jones) treating agricultural improvement as a stimulus to manufacturing, Boserup's analysis leads to an anti-Malthusian conception of the whole process of economic growth. Indirectly, it therefore raises questions about the supposed swamping of today's poor nations by excessive population growth. The answer could be, of course, that the relationship is curvilinear: some middling rate of population increase is most favorable to economic growth, while higher and lower rates are deletrious. Or it could be that Boserup is wrong. However the Boserup thesis comes out, the modeling and measurement of these relationships clearly belong on the agenda of demographic history, and her work has helped place them there.

One important exception to the simple correlation between population growth and agricultural productivity is the case of rural industry. In Europe and the Americas, there was a strong association between the expansion of rural industry and rapid population growth, on the one hand, between rural industrial concentrations and high rural densities, on the other. The causal connections are just as hard to specify in the case of rural industry as in the case of agriculture. In a recent close look at the phenomenon, Arnošt Klíma reports:

> In eighteenth-century Bohemia, population density varied in different parts of the country, being much higher in the mountainous and less-fertile regions. Statistics for 1764 give the average density of population for the whole country as 37.3 per square

> kilometre: 48 in the mountainous part of northern Bohemia but only 32.4 elsewhere; thus the less-fertile parts had a density almost 30 percent above the average for the country. Towards the end of the century, in 1789, the country average had risen to 54.8 per square kilometre, but that of the linen districts of northern Bohemia rose to 82, while the very fertile regions of Bohemia had no more than 56 per square kilometre (Klíma 1974: 50).

The observation recalls Blaschke's findings for Saxony. We shall encounter the same contrast in Braun's analysis of the Zürich region, later in this book.

Klíma, Blaschke and Braun are describing protoindustrialization: the expansion of manufacturing outside the factory system. It occurs by means of an increase in the number of producing units rather than a change in technology or a shift in the average scale of production. A great deal of European and American industrial expansion before 1850 happened through protoindustrialization; much of it took place in poor rural areas rather than in towns or cities.

Protoindustrialization has an important place in European and American demographic history. It is a major source of rapid population growth. It deserves special study because it provides a large series of partly independent natural experiments in which dissimiliar populations responded to changing economic opportunity by adjusting their patterns of nuptiality and fertility.

If the current drift of scholarly opinion is right, rural industry tended to grow up in regions combining 1) an underemployed land-poor

population; that is a possible consequence of immigration, partible inheritance, enclosures or rapidly declining mortality; in these circumstances, forms of agriculture with relatively inelastic labor requirements, such as dairy farming, favored rural industry over the intensification of agriculture; 2) access to urban markets for cheap finished goods. To be sure, merchants small and large played a crucial part in linking rural producers to raw materials and to urban markets; but the supply of merchants seems to have been highly elastic everywhere.

Protoindustrialization raises several different demographic problems. First, how regularly, and how, did rapid population growth precede the linking of cheap labor to urban markets via rural manufacturing? Second, is it true that the availability of employment in rural industry tended to lower the age of marriage, increase both legitimate and illegitimate fertility, and reduce the household to the nuclear family without servants? Third, is it true that the process was asymmetrical--expanding employment produced rapid population growth, but contracting employment simply produced misery--and that the resulting industrial population was more vulnerable to the wage-price scissors than the agrarian population was? Part of the problem is to what extent these tendencies are peculiarly true of rural manufacturing, rather than landless labor in general. During the eighteenth and nineteenth centuries, the landless increased in both agriculture and industry; whatever the cause and effect, before 1900 the bulk of the rapid population increase resulting from the western demographic transition occurred among the rural landless. Working out the demographic role of protoindustrialization would therefore aid our understanding of the western demographic transition.

Both protoindustrialization and the growth of landless labor in agriculture homogenized the economic experience of the rural population: larger and larger groups of people responded more or less simultaneously to the same fluctuations in prices, wages and employment. The transformation shows up in the changing pattern of migration. In Europe and America, there is no real evidence that large-scale industrialization greatly increased the frequency with which people changed residence. But the distances they moved increased tremendously.

The reason scholars have thought otherwise is that they have seriously underestimated the mobility of pre-industrial rural populations. Demographic historians who look at the subject directly almost invariably come out with high rates of turnover. In an agricultural community of Sweden, in 1881-1885, for example, Eriksson and Rogers arrive at mobility rates in the range of 0.5 moves per person-year of residence (Ågren et al. 1973: 72). In the town of Eskilstuna and its vicinity, Öhngren computes annual rates of gross migration (in-migration + out-migration) in the range of 200 per thousand population in the 1850s and 1860s. In the period of rapid industrialization which followed, the rates rose to 300 or so. Even then they were often higher than that in the nearby agricultural parishes (Ohngren 1974: 374-375). "In Hallines and Longuenesse (Pas-de-Calais) for the periods of 1761-1773 and 1778-1790," Poussou reports, "we find 51.3 and 36.3 percent of the population leaving, 45.2 and 45.1 percent of the population arriving, in twelve years" (Poussou 1971: 20). In these and other places, the average distance covered by migrants greatly increased as essentially local exchanges of labor gave way to large-scale movements among rural areas and, especially between country and city.

Giovanni Levi (1971) reviewed a number of recent works in French demographic history dealing directly or indirectly with population mobility. He proposed a three-phase summary of migration from the seventeenth century to the early nineteenth:

1. extensive movement but small net flows, dominated by a) circular movements of specialized non-agricultural workers between town and country as well as among towns, b) movements--especially seasonal--of agricultural laborers within the countryside, c) flows of beggars and unemployed workers in all directions, depending on the current geography of hardship;

2. rising long-term migration, increasing net movements from rural to urban areas and to industrializing rural areas, associated with and resulting from the increase of rural landless labor, the formation of large-scale labor markets and the rise of periodic unemployment;

3. large, permanent flows from rural to urban areas resulting from the deindustrialization of the countryside, the growth of large urban industries and the declining demand for labor in agriculture.

Levi assumes that the third type of migration moved many people from farming directly into manufacturing. That is probably incorrect. Some of that impression is due to the movement of rural industrial workers into urban industry. The main flow out of agriculture probably went into services. Furthermore, Levi's scheme neglects the large backflows behind the net movements in his second and third phases.

A more adequate model would replace the phases with statements about three sets of variables: a) the rising scale of labor markets, b) the conditions under which workers move among labor markets (including markets defined by different industries, whether geographically separated or not), c) the changing geography of job opportunities. Nevertheless, Levi's summary catches the distinctions among circular, chain and career migration (C. Tilly 1974: 288-296). It also gives a sense of the process by which small-scale but fairly regular movements of workers gave way to large-scale, irregular movements. In the process, large segments of the rural population fell into the rhythm of national and international fluctuations in economic activity.

At the extreme, whole regions became the economic dependencies of distant capitals. Their demographic experiences came to depend on the rise and fall of demand for their products in faroff places. For Java, Clifford Geertz (1963) has described the process of "agricultural involution": villages adjacent to foreign-owned plantations sold their labor to the plantations, retreated (often under pressure) from market production to subsistence agriculture, grew rapidly, eventually became dependent and vulnerable. So long as the world market for Indonesian sugar, rubber or tobacco expanded, the villagers multiplied and survived. When the plantations collapsed, the villages sank into misery.

In a broadly similar manner, the grain-growing regions of eastern Europe were becoming dependencies of Amsterdam and the other commercial-industrial centers of northwestern Europe during the sixteenth and seventeenth centuries. The growth of the "second serfdom" in eastern Europe consisted mainly of large landlords' assuming direct management of

their properties instead of continuing to live on rents, using the help of the political authorities to coerce labor from their peasants and to fix them in place, and raising wheat on a large scale for export via such commercial centers as Gdańsk (Danzig) or Riga.

A direct chain of credit attached the manors of Poland and Pomerania to the bankers of Antwerp. In his recent synthesis, Immanuel Wallerstein put it this way:

> This system of international debt peonage enabled a cadre of international merchants to bypass (and thus eventually destroy) the indigenous merchant classes of eastern Europe (and to some extent those of southern Europe) and enter into direct links with landlord-entrepreneurs (nobility included) who were essentially capitalist farmers, producing the goods and keeping control of them until they reached the first major port area, after which they were taken in hand by some merchants of west European (or north Italian) nationality who in turns worked through and with a burgeoning financial class centered in a few cities (Wallerstein 1974: 122).

There is a demographic side to all this: the argument requires a substantial labor shortage at the beginning of the process. It suggests that the considerable growth of the east European population during the sixteenth century resulted from migration--from deliberate colonization of thinly-occupied frontier lands. On the other hand, the scattered accounts now available indicate that the sections of eastern Europe devastated by the Thirty Years' War in the following century recovered

their losses quickly through natural increase. Perhaps natural increase also played a significant part in the sixteenth century.

We encounter the possibility that the same sort of saturation process that Geertz attributes to Java under the plantation system occurred in eastern Europe under the hegemony of the great wheat-growing estates. Faint in the background flickers a fascinating possibility: that the high rates of population growth in today's Third World countries will turn out to be less consequences of their own peculiar internal organizations than effects of their economic relationships with the rich countries of the West. The first scraps of information favoring such an interpretation would be discrepancies between results of cross-sectional and over-time analyses of fertility, evidence of a relationship between fertility and economic dependency (as indexed, for example, by the share of raw-material exports in national income) and signs of strong responsiveness of fertility in Third World countries to fluctuations in the world prices of their primary exports. Several years ago, Nathan Keyfitz (1965) suggested in passing that some such mechanisms were at work in the Third World. So far as I know, neither he nor any other demographer has followed up the suggestion seriously.

Work on population growth and economic change bears on the fundamental problems of western economic history. Assumptions concerning population processes underlie the alternative explanations of the industrial revolution which are now available. Our present state of uncertainty and ignorance concerning those population processes lays down a double challenge to demographic historians: to explicate and test the

alternative models now in use, to specify the demographic mechanisms whereby the transformation to an urban-industrial population occurred.

Components of Growth and Compositional Changes

In its simplest terms, the problem is to allocate the changes in size of the major populations under study among three factors: fertility, mortality and migration. British scholars, for example, are still debating to what extent the substantial eighteenth-century growth of population was due to a rise in fertility or a fall in mortality; a complete account would also allow for in-migration (e.g. from Ireland) and out-migration (e.g. to North America). How the three components changed makes a considerable difference to our interpretation of the period's social and economic history. If declining mortality is the chief contributor, we can imagine the rapid growth as starting without much prior change in the structure of everyday life: people were already receptive to life-saving innovations, and medical or sanitary improvements can begin without substantial prior changes in the average person's daily routines. (It is more difficult, but not impossible, to make the same sort of argument for life-saving improvements in nutrition or food supply).

If rising fertility or accelerating in-migration make major contributions to growth, on the other hand, almost any model of the change we can fashion will imply large prior changes in the local structure of opportunities. Thus the elementary analysis of population growth into its components sets important constraints on the possible explanations of the growth, and thereby on general interpretations of the period's social and economic history.

Components-of-growth analysis also helps with the details of economic and social history. For example, historians of Europe and America have

sometimes explained the widespread rise of illegitimate births around the beginning of the nineteenth century and the widespread decline in illegitimacy around the end of the century as a consequence of general changes in attitudes toward sexuality or the disruption caused by rapid industrialization. (For surveys, see Shorter 1971, 1972, 1973, Shorter, Knodel and van de Walle 1971, Smith 1973.) Illegitimacy has also been offered as evidence of changing attitudes or of disruption, but that form of argument assumes what must be proven. Now, female employment in domestic service increased, then decreased, in something like the same rhythm. Domestic servants were always one of the main sources of illegitimate children. So the rise and fall of domestic service may account for a major part of the trajectory of illegitimacy, without any general change in attitude or any general disruption of family life. In many such instances, it would be prudent to check out the compositional explanation before turning to the more complicated attitudinal or structural one.

Obviously, we can apply a components-of-growth approach to any categorization of the population for which data are available. Here I only want to sketch the significance of two overlapping processes: 1) the proletarianization of the population in general, 2) the changing composition of the rural population.

Proletarianization is a decline in the proportion of the labor force who have effective control over their own means of production, an increase in the proportion who are essentially dependent for survival on the sale of their own labor power. The definition contains several traps; "effective control" is often hard to judge, for example, in the cases of miners, weavers

or tenant farmers. Yet by almost any standard the proletarian share of the labor force increased enormously throughout the West some time after the fifteenth century. Between 1500 and 1800, the European population increased from roughly 55 million to about 190 million. My own guess at a partitioning of the increase runs as follows:

category	population in 1500 ($x10^6$)	population in 1800 ($x10^6$)	increase ($x10^6$)
landlords, owners & managers of producing units + their households	0.5-1.5	2-3	1-2
peasants + their households	25-35	70-90	40-60
wage workers in cities of 100,000+ & their households	0.5-0.75	4-5	4-5
other wage workers & their households	20-30	90-110	60-90
total population	50-60	180-190	125-135

These are, evidently, only guesses at numbers for which we have but shards of the necessary documentation. An early item on the demographic agenda is to refine and correct them. Yet the guesses are not fantastic. In the case of England, Gregory King guessed in 1688 that there were 1.4 million families, of whom 1.2 drew their principal income from agriculture. Of the 1.2 million, according to King, 350 thousand lived from their own land (Pollard and Crossley 1968: 154). In 1831, the census of Great Britain showed 1.8 million persons in agriculture, forestry and fishing; only 20 to 25 percent of them were full-fledged farmers (Deane and Cole 1967: 143).

Before the late nineteenth century, most of the increase of landless and land-poor labor occurred outside the factory-based proletariat so dear to twentieth-century Marxists. As Marx himself well knew, the growth of

landless labor in agriculture and rural industry created the mass of the European proletariat up to his own time; urban services and small-scale manufacturing accounted for most of the remainder. Factory employment grew later.

The analysis of proletarianization presents a standard, if difficult, components-of-growth problem: to what extent the swelling class of proletarians grew through its own natural increase and to what extent through movement of people from other categories. Each of these questions breaks down further: what were the contributions of changes in fertility? Mortality? Among the transfers, how many were a) changes of position experienced by individuals within their own working careers, b) movements into the proletariat by the children of non-proletarians, c) movements into the proletariat from outside the population under consideration, for example, through the enslavement of Africans? (This last category has its own historical interest. Fogel and Engerman show that the natural increase of enslaved Blacks on the North American mainland was high enough to produce sustained population growth, while life expectancy of their counterparts in the Caribbean was so low that only steady importation of slaves from Africa maintained the population. One probable consequence is a much more continuous flow of African culture into the Caribbean. See Fogel and Engerman 1974.)

The usual assumption is that the bulk of the proletariat moved into the class from outside through such processes as enclosure and the absorption of independent craftsmen into the factory system. Those were important processes, no doubt. The studies of the natural increase of landless labor

I mentioned earlier, however, raise the possibility that the proletariat multiplied itself to a large degree. If a careful compositional analysis showed that to be true, it would have profound implications for the political, economic and social history of the western working class. It would, for example, weaken Luciano Pellicani's argument that the "internal proletariat of capitalism" came into being via a process of "total uprooting" (Pellicani 1973: 68). It would diminish the probable role of the "loss of status" as a source of working-class protest. It would increase the plausibility of a distinct, continuous proletarian culture. It would open up the possibility that the change in size and characteristics of the proletariat was the most dynamic element in the western demographic transition.

Another version of that unsettling possibility appears in the changing composition of the rural population. I have already insisted that the rural population of western countries included many non-peasants. It even included many non-agricultural workers. In the case of France--that quintessentially peasant country--at the beginning of the nineteenth century about a third of the labor force in rural places was living from services, manufacturing, commerce and other non-agricultural pursuits. ("Rural" places included all communes with fewer than 2,000 persons in the central settlement.) At that point, rural textile production was the largest category, but woodcarving, smelting, basketry and even watchmaking all supported important clusters of rural people. Most miners also lived in, if not of, the countryside.

Miners are an informative extreme case. Friedlander has recently presented indirect evidence for the hypothesis that

> ...in coal-mining areas women had little opportunity for employment and could, therefore, contribute only little to the family's income and that men's earnings probably tended to shrink at a relatively early stage of life due to the nature of the special kind of work. This, and the unbalanced age-sex distribution resulting from heavy immigration, may explain...the pattern of early marriages and high marital fertility in coal-mining areas (Friedlander 1973: 49).

In Friedlander's analysis, extensive employment opportunities for young unmarried persons and lack of employment opportunities outside the home for adult women both promote high nuptiality and high fertility.

With some modifications, Friedlander's argument may apply throughout the rural population. Let us return to the idea of the peasantry as a self-regulating population. The regulation of numbers occurs through the tying of marriage and the opportunity to procreate to the inheritance of places on the land. When mortality is high, all other things being equal, new places on the land open up more frequently, nuptiality rises and fertility rises as well. (One of the more important questions about this hypothetical system is whether fluctuations in fertility depend mainly on changes in nuptiality, or whether both respond independently to shifts in opportunity--for example, through the estimates of prospective parents concerning future opportunities for their children.)

Let us assume that the basic decision rule of couples in the system runs something like this: marry as soon as you can acquire a permanent livelihood, and adjust your number of children to their chances of survival

and the probable return to the nuclear family of different levels of investment per child (cf. C. Tilly 1973). Then under a long-run decline in mortality people embedded in the peasant system are likely to shift from relatively high fertility to low fertility. Opportunities for out-migration will presumably slow this response. On the other hand, if attractive but expensive career opportunities for children arise, they should accelerate the process; there will be a movement, in Gary Becker's sardonic terminology, toward producing children of higher quality.

Something like this shift probably did occur widely among Europe's peasant populations as mortality fell from the eighteenth century onward. It did not show up as a dramatic and general decline in rural fertility, I suggest, for two main reasons: 1) because the opportunity for out-migration and the opportunity for local employment outside of peasant life both provided alternatives to restrictions on fertility, 2) because the non-peasant population did not behave in the same way. The first is plausible, but far from proven, in the light of what we know so far. The second is intriguing because the high-fertility behavior of the non-peasant population could result from following the peasant decision rule under changed circumstances. The rule is still to marry as soon as you can acquire a permanent livelihood, and adjust your number of children to their chances of survival and the probable return to the nuclear family of different levels of investment per child. But for agricultural laborers and rural industrial workers, a permanent (if not a sumptuous) livelihood is available young, some remunerative labor can be squeezed from almost any member of the household, and a heavy investment in one or two children would be risky. The result is high nuptiality and high fertility.

The hypothesis of a fundamental difference in the fertility behaviors of peasant and non-peasant rural populations is intriguing for another reason. It could help account for the gross regional differences in European fertility before the declines of the nineteenth century. The relatively low pre-modern fertility levels of Italy, France, Spain and Portugal could result from relatively high proportions of peasants in the total. The great block of high fertility in eastern Europe could be a consequence of the early proletarianization of the rural population on great estates. We would thereby circle back to the hypothesis linking high fertility to economic dependency. Let me insist that this is a chain of reasoning, not a chain of evidence. Part of the task of this book is to confront that reasoning, and its alternative, with evidence.

One more question raised by this line of reasoning is how fertility could ever have declined in the countryside. The answer is that the opportunities for rural wage-labor declined. It happened earlier in rural manufacturing than in agriculture, but it happened in both. On the whole, the European "rural exodus" followed the appropriate sequence: rural industrial workers relatively early, agricultural wage-laborers somewhat later, peasants (or, at least, agricultural workers who controlled their own land) the last to go in large numbers (Merlin 1971). If the peasants had been gradually restricting their fertility as mortality declined, but the non-peasants had been responding asymmetrically to employment opportunities, the net effect of this pattern of departure would be to produce a massive, rapid decline in rural fertility followed by a long, low plateau. Although the opportunities for out-migration and for

social mobility complicate both the argument and the evidence, I think something like this pattern occurred widely in Europe. Again, part of this book's task is to set limits on that sort of reasoning.

If my summary is correct, however, a new problem becomes salient: in the days of rural exodus, what happened to the fertility of the rural wage-laborers and their urban descendants? To produce the large, continuous declines observable in European fertility, we need some combination of escape from the Malthusian trap in the countryside and transformation of behavior coincident with migration to the cities. I suspect the change was slow in the country and fast in the city--and that the crucial difference was the availability in the city, at a high price, of opportunities to help one's children move up in the world. In the short run, the decision rule remains the same, but the difference in available opportunities transforms the behavior. In the longer run, however, the situation alters so much as to produce a new decision rule.

Small-Scale Processes

The arguments I have just been sketching rely on assumptions about the behavior of individual households. Moreover, they contradict a good deal of common sense and a great many portrayals of pre-modern fertility by treating procreation as the outcome of a more or less rational weighing of alternatives. So risky a notion deserves direct attention. It requires the study of processes at a smaller scale than we have been considering so far: at the level of the individual, the household and the kin group.

In the last decade, economists have been developing models of household behavior--including what they like to call "the production of children"--which operate at the small scale and incorporate assumptions of rational

choice. Marc Nerlove (1974) has summed up the major features of the theory most commonly employed in recent work as: "...(1) a utility function with arguments which are not physical commodities but home-produced bundles of attributes; (2) a household production technology; (3) an external labor-market environment providing the means for transforming household resources into market commodities; and (4) a set of household resource constraints..." (Nerlove 1974: S210). Most of the work done within this framework, as Nerlove observes, has been static in character; it has given little attention to such problems as the effects of changing household composition, the investment of one generation in the welfare of succeeding generations, or the causes and consequences of long-run shifts in vital rates. Nevertheless, a number of arguments elaborated in the recent literature converge on the hypothesis that "a rise in the cost of mother's time for the family will cause a substitution away from time-intensive goods such as children and toward those requiring more inputs of market-purchasable commodities" (Nerlove 1974: S210).

Nerlove makes three suggestions which could connect this line of argument with the general pattern of the demographic transition: that the effect of declining child mortality is to generate a greater demand for children (since the cost of achieving a given family size declines while the discounted sum of satisfactions per child increases); that declining child mortality produces an offsetting decline in the cost of child quality relative to the cost of numbers of children; that over the course of economic development the value of a unit of human time tends to rise as a consequence of increasing investment in human capital, with the consequence of "reinforcing the tendency to fewer children of ever-higher quality" (Nerlove 1974: S217).

The second and third suggestions dovetail with the arguments and findings presented elsewhere in this book. The first--that declining child mortality increases the demand for children--contradicts a major theme of our papers. Although Easterlin builds direct satisfaction from the presence of children into his analysis (and although none of our authors denies that the sum of such satisfactions per child tends to rise as child mortality declines), our discussions stress the importance of desires to transmit household wealth to successive generations without fragmenting it. The household itself is the major unit of production among peasants, artisans and many varieties of merchants, manufacturers and service workers. Where it is, the double desire to maintain and to transmit household wealth is likely to be strong. To the extent that this is the dominant incentive to procreation, the effect of declining child mortality on completed family size will be negative, not positive.

I have already suggested, however, that proletarianization dissolved the nexus among employment, household position, marriage, procreation, inheritance and the maintenance of household continuity. As the nexus weakened, so probably did the pressure to conserve and transmit household wealth, hence the resulting constraint on fertility. As the opportunities for employment of children outside the household expanded, the possibilities of enjoying them both for themselves and for the wages they brought to the household increased. The diminished pressure for household continuity probably also allows more room for what Philip Neher calls the "pension motive": "Parents invest in their children by bearing their rearing costs in anticipation of retirement when their children, in turn, will support them" (Neher 1971: 380). It may be, then, that Nerlove's formulation applies

to today's essentially proletarian populations, but lacks a significant variable when applied to populations in which the household is the fundamental unit of production as well as consumption, or to households which exercise collective control over substantial capital. The missing variable is the pressure to conserve family property.

Speaking of missing variables, a reader from outside of economics is likely to be amazed that these arguments attach so little importance to sexual desire and satisfaction. As Richard Easterlin remarks later in this volume, the economics of fertility is a "notably sexless subject." As a reaction to the crude Malthusianism which has underlain so much previous writing on fertility, it is useful to have an approach which stresses the non-sexual calculations behind fertility. Yet people do enjoy sexual intercourse; they sometimes pursue it in apparent disregard of costs and risks. Unwanted children are born both in and out of wedlock. Abortion and infanticide occur frequently enough to make us think that the decisions leading to sexual activity and to childbearing are at least partly separable.

The diagnosis suggests the remedy. We need an analysis of decisions to engage in intercourse, a separate analysis of decisions to have children, and an analysis of the constraints one sets for the other. The constraints will include the whole series of contingencies between intercourse and childbirth: the extent and effectiveness of contraception, the fecundity of the sexual partners, the likelihood of fetal loss, and so on. In seeking to synthesize the economics and sociology of fertility, Easterlin is making exactly that effort to relate arguments concerning sexual behavior to arguments concerning fertility.

Historians and sociologists have commonly finessed the problem in one of two ways. Sometimes they have treated the one set of decisions as dominant, the other as derivative: the essential decisions govern the frequency of sexual intercourse, while the probabilities of conception and birth are basically technical matters; or the essential decisions govern marriage and childbearing, while within the limits set by those decisions sexual activity varies too little to matter. The second finesse is to postulate a massive change from one system to the other: from "natural" to "controlled" fertility. Under natural fertility, in this way of thinking, the essential variable governing fertility is the age structure of marriages. Who can marry when is a function of economic opportunities, the supply of potential spouses and social pressure. Thus fertility responds strongly but indirectly to changing social conditions. (In the baldest Malthusian arguments, however, even that response is weak or non-existent; natural fertility simply means fertility approaching the human capacity.) Controlled fertility, in such a formulation, appears when couples acquire the individual freedom and the technical means to detach fertility decisions from sexual ones. Modernization provides the freedom and the means.

A variant of this argument appears in several of this book's articles. It postulates a shift from socially-controlled to individually-controlled (or, better, couple-controlled) fertility. In E.A. Wrigley's essay, for instance, we find the distinction between an "unconscious rationality exercised by individuals following the norms set for them by the society in which they live" and a "conscious rationality characteristic of couples in

industrial societies where family limitation is widespread" and the hypothesis of a general transition from one to the other. Wrigley points out that declining mortality destroys the "unconscious rationality" of the sorts of fertility strategies which prevailed in pre-industrial Europe. He suggest that declining mortality has helped promote the fertility decline wherever it occurred. Elsewhere (e.g. Wrigley 1972) he makes a rough equation between modernization and the spread of conscious, economically maximizing rationality, and hints that it occurred largely as a consequence of the diffusion of new ideologies.

An ironic result follows. We go from a society in which well-defined collective needs explain group-to-group variations in fertility while individual differences are matters of chance, impulse and inclination to a society in which collective needs set few constraints on fertility but individual calculation governs it very closely (cf. Ariès 1971). It seems to follow that at the level of the individual or the couple the importance of decisions concerning sexual behavior as determinants of fertility declines greatly as modernization proceeds. If that is the case, the further we go back in time, the less well the available economic models of fertility should work. And the more sex should matter.

That extrapolation of Wrigley's argument differs significantly from Edward Shorter's recent analyses of illegitimacy and sexual behavior in the modern West. Shorter inserts another stage between the eras of socially-controlled and individually-controlled fertility. The middle stage has working class women, liberated from family control by new opportunities for employment outside the home, leading a general move toward

individual gratification, including the search for sexual pleasure. At the same time, middle class women lead the trend toward restriction of births. As the two waves wash in opposite directions, they dissolve the old ties among marriage, birth and procreation. The middle stage therefore begins with rising fertility both inside and outside of marriage, as increasingly desirable and permissible sexual activity rises without a corresponding increase in contraceptive effectiveness; it ends with a decline in legitimate and illegitimate fertility, as effective contraception diffuses.

Louise Tilly, Joan Scott and Miriam Cohen have attacked Shorter's argument both for lack of evidence concerning the hypothetical changes in attitudes and for inconsistency with what is known about the actual patterns of female employment in western countries since 1800 (Tilly, Scott and Cohen 1974, Scott and Tilly 1974). They have pointed out that large numbers of European and American women worked in the company of their parents and siblings, committed their wages to the welfare of parents and siblings, and ceased their wage labor at marriage, and that the bulk of the nineteenth century increase in employment of women outside the home occurred through the expansion of non-factory occupations which had long employed female workers. They concede the concomitant rise and fall of both legitimate and illegitimate fertility and agree with Shorter in stressing the contribution of contraception to the decline. But they deny Shorter's calendar of attitudinal change and attribute the earlier rise in fertility to short-run effects of proletarianization and of declining mortality. Reviewing the American evidence, Daniel Scott Smith

(1973) rejects Shorter's calendar even more emphatically, minimizes the attitudinal changes involved in the last century's alterations in sexual behavior, and suggests that the most recent shifts continue a long series of swings up and down in both legitimate and illegitimate fertility.

Three elements of Shorter's analysis do not, I think, stand up well to criticism: the hypothesis of a new, massive, irreversible diffusion of desires for individual gratification starting toward the end of the eighteenth century, the idea of a consequent general alteration of sexual behavior as traditional constraints crumbled, and the explanation of fertility changes as a result of the new self-indulgence. But the debate is not closed. The concomitance of changes in legitimate and illegitimate fertility, as Shorter points out, challenges explanations which focus exclusively on changing family strategies. Likewise, the apparent generality and rough simultaneity of both the rise and the fall in fertility throughout western Europe make it difficult to invoke the immediate effects of urbanization or industrialization, which proceeded at very different paces in different regions. My earlier discussion of proletarianization gives some reasons for seeking a major part of the explanation in the expansion and then the contraction of rural landless labor. Whether that is a false lead or not, checking it clearly belongs on the agenda of demographic history.

The agenda includes the specification and localization of the vital changes to be explained. It includes combing and collating the scattered eighteenth- and nineteenth-century descriptions of sexual behavior and family life. It includes close study of differential patterns of change

by occupation, industry, age, family status, wealth and locality. And it involves modeling the relationships to be expected if the hypothesis of a massive, effective ideological change is correct or if the major alternatives to that hypothesis are correct.

Both this particular line of inquiry and the general problems in household economics discussed earlier lead to another two agenda items we have not yet discussed directly: the determinants of household composition and the causes and effects of labor force participation by different members of the household.

Household composition is problematic in more ways than one. First, the work in household economics generally depends on the assumption of collective decision-making by a household in terms of a single utility function. At a minimum, the presence or absence of aged parents, collateral relatives, numerous children, boarders, servants or multiple nuclear families within the same household should affect the shape of that utility function; with complex households, the assumption of a single collective decision-maker may work badly. What is more, household composition is a consequence of household decision-making: decisions to marry, to migrate, to have another child, to take on a hired hand, and so on. Thus decisions at one point in time will reshape the utility function for the next round of decisions.

In addition, households often make deliberate changes in composition as an alternative to altering their fertility patterns or changing their patterns of consumption of goods and services . Some homely examples are sending babies out to nurse (and therefore, quite likely, to die), bringing

in a hired hand when the farm family has a short supply of male workers, hiring out a youngster as a servant or an apprentice, doubling up with a sibling's family in times of hardship. All of these were common and crucial in some phases of European history. It may be possible to generalize the economic analysis of fertility into an analysis of decisions concerning household composition. Otherwise, we shall have to graft a new set of arguments about the causes and effects of household composition onto the existing tree.

Peter Laslett (1972) has recently held out hope of avoiding that complexity. He notes the statistical predominance in western countries of households consisting of no more than one nuclear family and no non-family members. Ansley Coale and others had already shown (e.g. in Coale et al. 1965) that for compelling demographic and structural reasons the large "extended family" consisting of a couple, their children and their children's children was likely to be rare even where people held it up as an ideal. Their arguments did not rule out the possibility of compounding through the co-residence of married siblings, the employment of servants, the taking in of lodgers, and so on. Working mainly from nominal census lists, Laslett and collaborators laid out long runs of evidence for the rarity of these arrangements in England, France, Italy, the Balkans, the Low Countries and the United States.

In an as-yet unpublished critique, however, Lutz Berkner shows that the evidence is not overwhelming: it is dubious whether the enumerations analyzed do distinguish households in a uniform and theoretically meaningful way, the statistical predominance of nuclear households at any one point in

time is quite compatible with arrangements in which households normally have a compound phase, and in any case the ethnographic accounts provided by Laslett's collaborators document the widespread existence of compound households. As Berkner sums up:

> Despite their focus on the small nuclear family, what do these studies actually indicate about family structure in the past? First, that a large proportion of the households in many regions included an extended family phase. This is true in southern France, Tuscany, Corsica, and of course Serbia and Japan. Second, that there is a great deal of regional variation which can be explained by social and economic differences. In Tuscany, households were more complex in the rural villages than in the cities, in the Netherlands they were more complex on arable than livestock farms, and in Japan complexity and size reflected commercial isolation. Third, that the complexity and size of peasant households is directly related to their wealth. This is the case in rural Lancashire, Corsica, and Tuscany. Fourth, that inheritance and succession rules are crucial variables. They explain the high incidence of household complexity in Japan (through adoption) and Serbia, and might explain the difference between southern and northern France or between Holland and Overijssel (Berkner 1974: n.p.).

It looks as though students of small-scale demographic processes will not be able to avoid dealing with household composition.

The same goes for the causes and effects of labor force participation by different members of the household. The problem is already on the agenda in the form of discussions of tradeoffs or conflicts between female employment and fertility. The general version of the problem concerns the disposition of the household's entire supply of labor. That includes the labor of children and old people. Following Chayanov, a number of students of the European peasantry have looked closely at the labor requirements of different types of farms, and have seen peasant households as carrying on a continual negotiation between their own age-sex composition and the work to be done (see Thorner 1964, Wolf 1966). The demand for labor on most peasant holdings is inelastic. Over the longer run, goes the hypothesis I mentioned earlier, peasants adjust their fertility to that demand for labor. Peasants respond to short-run discrepancies between the supply and the demand on their own holdings by farming out their own youngsters or taking in youngsters from other farms, by renting additional land or renting out land they cannot handle themselves, by hiring land-poor laborers, and so on. (In this volume, Berkner and Mendels, Braun and Wrigley all discuss different features of these adjustment processes.) The availability of piece-work and wage-work in rural industry and agriculture provide an alternative to the tuning of household composition to the labor requirements of the individual holding; however, it also provides means and incentives for the departure of wage-earners from the household.

Permanent employment outside the household and long-distance migration often begin as simple extensions of these local adjustment processes: a

region of Switzerland comes to specialize in the supplying of mercenaries to European armies, and their remittances keep the family economy going; what was once a few years of domestic service before a girl married becomes a lifetime as a maid, and so on. In another variant of the process, whole households come to be engaged in rural industry--first carding, spinning, weaving and so on within their own dwellings, then transferring the same division of labor into the early factories. The earliest promoters of "child labor," as Neil Smelser insisted some time ago, were the parents of the child laborers. They brought the children with them into the shop, received remuneration for the household as a team, and had to hire someone to fill the children's roles if they had no offspring of their own to do the job.

Historically speaking, the problem of labor force participation links directly to the problems of household composition and of proletarianization. In rural households, the connections between employment opportunities for children and fertility seem at least as important as the connections between employment opportunities for married women and fertility.

The current theoretical challenge in the study of small-scale vital processes is to see whether economic models such as those proposed by Nerlove and Easterlin can accomodate these new contingencies, or whether we shall require new models incorporating multiple utility schedules, changing household composition, partially independent determination of sexual activity and fertility, varying loci of control over fertility, and multiple opportunities for employment of the household's labor supply.

The Agenda

The historical study of vital processes, it turns out, has an agenda which is rich, distinctive and significant. Parts of the agenda belong to demography as a whole; historical materials are simply a convenient source of data for them. That is true, I would say, of the decomposition of year-to-year vital changes into cohort, compositional and annual effects. Other parts of the agenda are of great interest to historians, but matter little to demography itself. Most of the components-of-growth analyses I discussed earlier fall into that category. Yet there is an important remainder: fundamental problems which are at once historical and demographic. The damaged theory of demographic transition will not be repaired without close analysis of long series of changes in fertility and mortality. The extent to which peasant populations are self-equilibrating--and, if the extent is large, how the equilibrating processes work--matters to western economic history as well as to contemporary analyses of population control. The related question of whether proletarianization has a strong, consistent tendency to promote high rates of natural increase (and if so, how) applies to a wide range of situations both historical and contemporary. The tangled ties of population growth, labor supply, consumer demand and economic growth require sorting. How much, how effectively, and how out-migration, employment in wage-labor, restrictions on marriage and control of fertility itself acted as alternatives to each other in the western historical experience deserves the closest demographic attention. Finally, it will take a great combination of historical and demographic expertise to determine where, when and how the durable nineteenth-century decline of fertility occurred in

Europe and America: did something crucial happen in the cities? Did the proletarians' acquisition of property and of opportunities for mobility help them escape the Malthusian trap? Did some sort of attitudinal revolution rapidly revise people's approaches to sexuality and child-bearing?

These strings of provocative questions braid into two main strands:

1. In the parts of the world which are new predominantly urban and industrial, by what demographic process did the transformation of an agrarian into an urban-industrial population occur?

2. What caused the long-run changes in the fertility level and in the determinants of fluctuations in fertility within those same areas?

Other questions concerning mortality, nuptiality, migration and social mobility are also worth asking. Some of them come up inevitably on the way to answering the two master questions. But the general inquiries into the creation of urban-industrial populations and the determinants of fertility changes will surely dominate the agenda for some time to come.

In each case, we have two groups of theories to choose from. The first postulates a sharp discontinuity between the old system and the new one, and thereby requires us to formulate three subtheories: one concerning the dynamics of the "traditional" or "pre-industrial" demographic system, another concerning the dynamics of the "modern" or "industrial" system, and a third concerning the transition between them. In the case of the creation of an urban-industrial population,

the typical components are a model of the labor requirements of an agrarian economy, a model of the labor requirements of large-scale industrial production, and a model of a modernization process: diffusion, capital accumulation, entrepreneurship, organizational innovation, technological change in some combination or other. In the case of fertility change, the typical components are a model of "natural," "socially controlled" or "traditional" fertility behavior, a model of "controlled," "individually controlled" or "modern" fertility behavior and a model of the process by which one replaces the other: a primarily ideological process, a primarily technical process, or something else.

The second group of theories treat both long-run and short-run dynamics as outcomes of the same fundamental regularities, and thereby stress the continuities between past and present. In the case of the creation of an urban-industrial population, we have technical innovation, capital accumulation, investment, organizational transformation and changes in population composition accelerating or decelerating together as a consequence either of their own internal dynamics or as a function of exogenous changes in mortality, in communications or in institutions of property and political control. In the case of fertility change, we have individuals or households maximizing in accordance with a set of utilities which change very little and which are relatively uniform from one group to another, but under constraints which vary importantly from population to population and which shift significantly both in the short run and the long.

The choice between discontinuity theories and continuity theories is familiar. Every problem of "modernization" or "development" poses the same choice. It is not merely a matter of emphasis; the compromise "Some things change, while others remain the same," will not resolve it. What is at issue is not whether the values of crucial variables remain the same, but whether the relationships among variables change so radically from one domain to another that we need a new theory for each domain. Advocates of continuity theories tend to treat this as partly an empirical question (how well does a model which operates effectively in one domain work in the next?) and partly a question of convenience (at what level of generality is it currently easiest and/or most effective to argue?); they hope to subsume the best stage formulations into their own general models. Advocates of discontinuity theories tend to consider proposed general models as much more bound to their times and places or origin than their advocates admit, and to attack the fit of their assumptions, their categories and their empirical implications in the new domain; Karl Polanyi, for example, argued long and hard that the market was a historically specific development, that economic theories built around market mechanisms could not and did not fit most agrarian economies.

As is probably obvious in the earlier discussion, my own sympathies lie with the attempt to build general models. Nevertheless, I would like to see models which take time itself seriously. In general, I mean models in which what has happened before constrains what happens next. Developmental models which portray essentially

the same set of changes as recurring in essentially the same manner within population after population violate the prescription by treating each case as more or less autonomous. The formation of national states (to take an example outside the present discussion) was an historically specific process which began in earnest in western Europe some time around 1500, which lead to the creation of a state-system encompassing almost the entire world by the middle of the twentieth century, in which the states in existence at any given point in the process strongly affected the statemaking activities and outcomes of the newcomers at that point in time, and in which the states and protostates involved continuously shaped each other through war, diplomacy and economic activity. Yet we have abundant theories of political development which propose a recurrent process happening (or failing to happen) in country after country more or less autonomously. The same confusion prevails about capitalism: an historically-specific system of property relations which likewise originated in Europe and likewise came to dominate the entire world. It makes relatively little theoretical sense to label some countries of the contemporary world as capitalist and others as non-capitalist when all are embedded in an international system in which the market sets the price for all the factors of production. It makes almost no sense at all to analyze the development of capitalism country by country as if it were a standard, recurrent, autonomous process. Instead of developmental theories, in this sense of the word, we need historical theories: theories which relate the experience of any particular

population to historically specific processes involving a number of different populations at the same time.

The inclusion or exclusion of time matters because it affects the possibility of our generalizing from historical studies. Where a well-defined and self-contained developmental process actually does exist, on the analogy to the life-cycle of an organism, we can conveniently neglect time, and predict or even promote the recurrence of that same process in a new setting. That has been a sustaining hope of development theorists in economics, sociology and politics. To the extent that a process is larger than any particular population we may care to analyze and/or is changing significantly over time, the analogy from past to present will be faulty. That does not mean there is no way to generalize from the past; it means the generalization will have to include an adjustment for the time of its application, and may have to include specific allowances for the relations between the population in question and the rest of the world.

How does my polemic apply to historical studies of changing fertility? Mainly by warning against the effort to derive a standard sequence for the demographic transition from the experiences of single western countries and to apply it directly to the poor countries of today's world, by drawing attention to such possibilities as that proletarianization is playing a larger and larger part in the whole world's population processes as the economic interdependence of different parts of the world increases, and by suggesting that the form and extent of a population's dependence on others should become

major variables in our models of urban-industrial transformation as well as our models of fertility change. In short, by giving preference to continuity theories, but with time built in.

The authors in this volume have no obligation to honor my preference. In fact, they vary considerably in their theoretical preferences. Consider the contrast between a continuity theorist such as Ronald Lee, who aspires to capture the entire evolution of the English population in a single set of equations, and a discontinuity theorist such as Rudolf Braun, who insists on the cultural distinctions of a Swiss world of pre-industrial times whose regularities the expansion of industry simply swept away. Compare Easterlin's portrayal of households maximizing under changing constraints with van de Walle's stress on the diffusion of new ideas and information. The purpose of my long commentary on the existing literature has been to pace out the space and help the reader see where my collaborators stand within it, not to herd them all into the same corner of the space.

We have arranged our papers in a rough descending order of generality. We begin with Richard Easterlin's synthesis of economic and sociological ideas about fertility. The treatment is abstract, the scope the entire world. E.A. Wrigley's "Fertility Strategy for the Individual and the Group" discusses the impact of different mortality schedules on the survival of households or communities exhibiting various patterns of fertility. He concerns himself mainly with pre-industrial European populations, but explores in general terms in what sense such populations could be, and were, self-regulating. Lutz Berkner and Franklin Mendels undertake the systematic

analysis of a problem which has produced a good deal of folklore, but few clear results: the relationship among the system of inheritance, the composition of households and the patterns of nuptiality and fertility in western Europe before the twentieth century. In particular, they try to determine whether the inheritance system--especially the distinction between partibility and impartibility--has an independent effect on demographic patterns. Ronald Lee fashions a series of economic models of the determinants of temporal fluctuations in vital rates. He estimates the models by means of long series from England before the nineteenth century, using techniques ranging from simple regression to spectral analysis. The paper by Etienne van de Walle reports some of the findings of a massive region-by-region study of fertility changes in nineteenth-century France. More so than in other reports of the study, van de Walle examines (and makes preliminary tests of) arguments concerning the diffusion of contraceptive practice in France. In his "Multivariate Regression Analysis of Fertility Differentials among Massachusetts Towns and Regions in 1860," Maris Vinovskis actually presents a substantial discussion of vital trends in New England during the first half of the nineteenth century as well as the large cross-sectional analysis promised by the title. Using the fertility ratio as the primary dependent variable, Vinovskis alternates between establishing the strength of regional variations and measuring the relationships between his fertility indexes and a number of characteristics of the local population. Finally, Rudolf Braun draws on his long historical studies of the transformation of Zurich's hinterland as cottage industry rose and fell. In this essay, he emphasizes the contrasting demographic

behavior of rural households in agriculture and in industry, and sketches the demographic mechanisms by which the industrial population increased.

I take up the contents and implications of the seven papers in this book's conclusion. Here I want simply to forecast some of their common themes.

As compared with the existing literature and as compared with the agenda this group set for itself at the beginning of the inquiry, the papers attribute relatively little importance to industrialization as such. That is partly because of their concentration on "pre-industrial" populations. (The word is misleading because of the extensive small-scale manufacturing which went on in rural Europe before the nineteenth century.) It is partly because much of their work goes into inserting other variables--especially demographic variables--in between industrialization and fertility change. But it also reflects a growing doubt that exposure to large-scale manufacturing and its concomitants reliably transforms the patterns of nuptiality and fertility in the populations involved.

On the demographic side, our inquiries increased our appreciation of the effects of changing mortality. The theoretical discussions (for example, in the papers by Wrigley or Berkner and Mendels) stress the importance both of the turnover in adult positions due to mortality and of the highly variable life expectancy of children. The empirical analyses (e.g. in Lee and van de Walle) consistently reveal strong associations between levels of fertility and mortality. In compensation, several of the studies (notably Lee's) question whether variations in opportunities to marry acted as quite the regulator of fertility that Malthus and many after him have thought. In

pre-industrial and industrial populations alike, fertility regulation within marriage comes out as the primary adjustment mechanism.

That line of inquiry leads a number of the papers back to the hypothesis of an unconscious collective rationality which roughly matched the procreative tendencies of pre-industrial populations to the carrying capacities of their environments. Not that the system was gentle: in all our portraits, it depended on life expectancies at birth of less than forty years. In several of the analyses, it was compatible with long periods of declining real wages. And our general arguments make the system vulnerable to the increasing dependency of the local population on employment governed by demand in distant markets. Nevertheless, a picture of self-regulation short of utter misery emerges from our varied explorations of the agrarian West. By implication, our findings give grounds for both optimism and pessimism about the population problems of the contemporary world. Cautious optimism: we end up with some confidence in the capacity of human populations to regulate themselves. Pessimism: we end up doubting that the high fertility of the Third World results from the fact that its populations have not yet begun to restrict births--but will somehow begin to do so automatically as modernization proceeds.

NOTE ON BIBLIOGRAPHY

Since the bibliography of this paper is fifty pages long, I have decided to omit it from this version and to circulate it separately.