

# **Frontiers in Population Sciences**

Report to the Andrew W. Mellon Foundation

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## **Background and Scope**

The purpose of this report is to identify promising areas for research in the population sciences in the next decade, with a focus on topics and mechanisms that might be especially appropriate for foundation funding.

After an introduction dealing with the motivation for the report, we identify compelling problems in fertility, marriage and the family, mortality, and migration. The research agenda in Population Sciences, we argue, goes well beyond a “mop up” operation at the end of the Demographic Transition. There are serious and interesting problems, with profound implications for human welfare, close enough to these traditional concerns of demographers to make them good candidates for the Population Sciences agenda, yet likely to benefit from fresh approaches. In the second main section, we propose ways in which those who fund research and training can foster progress, crossing disciplinary boundaries and the artificial divide between developing and developed countries. Before getting to the main arguments, we discuss definitions, since “Population Sciences” is not a term with a precise and widely agreed meaning.

This report is a product of interviews with individuals and meetings with groups, as well as a review of the literature. The discussions began with a presentation to a meeting of the NRC Committee on Population, and were followed by interviews with demographers working at the University of Pennsylvania, the University of Chicago, Georgetown University, and the University of Maryland, as well as colleagues at the Population Reference Bureau. A small group of researchers at various stages of their careers met before the 2001 annual meeting of the PAA to discuss training needs with us. Our literature review covered other efforts to assess progress and research needs (such as anniversary volumes of leading journals, various reviews surrounding the millennium, and reviews of projection methods) and broad reviews of fertility, migration, biodemography, and other topics

## **The Population Sciences**

The rate of growth of world population peaked around 1970. A few years later, the Nobel-Prize-winning biologist Peter Medawar wrote that “We are by no means out of danger yet, but I feel the period of acute panic is over and has been giving way to a more sober assessment of the practicality and effectiveness of the procedures that might help to

keep populations within reasonable bounds” (Medawar, 1981: 305). More recent writers have argued that even this “sober assessment” phase is coming to an end. The absolute size of the annual increments to world population peaked during the 1990s. To Nicholas Eberstadt (2001), this marks the end of “the ‘population explosion’, the only demographic era within living memory” and calls for a redeployment of intellectual resources. The population explosion was the only population problem of which many people were aware, which has led Philip Morgan to wonder if “we’re going to miss the Population Problem, just as the Pentagon misses the Soviet Union.” Most national populations are still growing, and there has been relatively little concern over population decline, even though some countries and many regions have been below replacement-level fertility for more than a generation. What is the rationale for the Population Sciences in an era without a distinctive, worldwide Population Problem?

One approach to defining the scope of the Population Sciences is to focus on the topics studied by the core subject, Demography: processes affecting the size, growth, composition, and distribution of populations; namely, fertility, mortality, and migration. Family and household demography are natural extensions of the study of populations of individuals. Samuel Preston points out that “the number of technical demographers on university faculties is tiny, and at best, stationary,” so progress has always depended on an ability to attract scholars from other disciplines to work on population problems (1995: 237). He used the analogy of a central city (core topics in demography) and its suburbs -- the overlap with other social and health sciences, constituting “population sciences.” We might further distinguish between the “inner suburbs” -- the collaborations with other disciplines to study the proximate determinants of fertility, marriage, migration, mortality -- and the “outer suburbs” -- where the list of topics, methods, and data is much broader, and includes the study of childrearing, movement into and out of poverty, demography of crime, effects of social welfare policies, retirement savings, and a whole host of other topics represented at the annual meetings of the Population Association of America. Daniel Lichter (2000) refers to “Population Sciences” defined in this way as what “demographers (self-defined) actually do,” as distinct from *a priori* definitions.

A second approach pays more attention to methods than to subject matter. In several of the articles commissioned for anniversary volumes of *Population Studies* and *Demography*, the characteristic contribution of Demography to the social sciences was seen as an approach to data analysis, a particular concern with measurement and estimation. “Few demographers,” in John Caldwell’s view, “will continue to pile up hypotheses or proceed from one speculative idea to another, unless they can gain some support from the analysis of quantitative data. This is certainly not the situation in much contemporary sociology and anthropology.” (Caldwell, 1996: 313). Statistical techniques and data sources are the common property of people trained at Population Research Centers: these include event history models, logistic regression models, nationally representative surveys, the increasingly popular longitudinal data sets, and census microsamples.

Population scientists bring these tools to a wide variety of tasks. For example, demographers modeling fecundity and fertility helped develop methods of looking at factors affecting transitions between states (like pregnancy, postpartum infecundity, and risk of pregnancy) and the resulting time spent in different reproductive states. Economic demographers began to use a similar approach to look at “spells” of welfare receipt, and transitions into and out of welfare receipt and poverty. They found that the great majority of welfare spells were of short duration, though most welfare recipients at any one time were in the midst of long-term spells. This finding challenged entrenched policy views and helped pave the way for welfare reform in the United States (Gottschalk, McLanahan, and Sandefur, 1994). Similarly, demographers had a unique awareness of social change as a process of cohort replacement, with the adult population turning over as younger cohorts enter and the oldest ones die or retire (Ryder, 1965). This has proved to be a very useful way to look at changes in family and worklife (Bianchi and Spain, 1986) and in civic participation (Putnam, 2000).

For this report, we chose to use elements of both approaches -- using an outline based on the traditional topical headings, but looking for areas in which methods and data sets could be expanded into new areas of concern for the social sciences. We did not attempt to cover in depth the boundaries between population sciences and public health, in part because a recent conference on the past and future of connections between Demography and Epidemiology covered that subject in a more comprehensive way (Weinstein and Hermalin, forthcoming).

What these examples suggest is the value not only of new methods and new data sources but also of moving some familiar ones into new disciplinary or policy territory -- the gains from intellectual trade. In this report, we argue that the Population Sciences could benefit from both imports and exports. There are vital issues near the heart of the old topical standbys -- fertility, mortality, and migration -- that need a fresh approach and new alliances with other disciplines. There are also issues important for human welfare, further away from the heartland of demography, for which the tools, data sources, and even attitudes toward data, common in the Population Sciences, could give a boost to research.

To push the analogy further, the particular contribution of foundations in funding research could be to lower the barriers to intellectual trade. Foundations are well placed to encourage work that crosses disciplinary boundaries. The foundations should also be able to foster work that crosses geographic and institutional boundaries in useful ways, funding training, collaborations, and creation of data sets.

### **Fertility -- How Low Can It Go? And How Fast?**

The speed of fertility decline in most parts of the world in the last few decades is both a challenge to our understanding of human populations and a trend of great practical significance in its own right.

The Population Reference Bureau estimates that 2.7 billion people (44 percent of the world's total) live in countries at or below total fertility rates of two children per woman (Haub and Cornelius, 2001). A much smaller number of people still live in countries with high fertility rates (total fertility rates of four or more children per woman) – just under 1 billion, or about 16 percent of the world's population. Just 20 years ago, those proportions were almost exactly reversed: Forty-five percent of the world's population lived in countries with total fertility rates of four or more children per woman, and 16 percent of the world's population lived in countries with fertility rates below replacement level (World Bank, 1984).

Of some 2.7 billion persons in low-fertility countries now, 97 percent are residents of East Asia, North America, and Europe. If we were to count the large Indian states as separate countries (their populations are bigger than all but a handful of the sovereign nations), we could add nearly 100 million Indians in two South Indian states to the population currently below replacement-level fertility. Several other large Indian states are nearing replacement-level fertility.

The pace of fertility decline in the remaining high-fertility regions will affect the likelihood of achieving health and economic improvements in many of the poorest and most politically turbulent countries. This conclusion does not depend on fixed-resource arguments or any calculation of “carrying capacity,” but on a plausible assertion about public investments. Even if the ultimate size of the stable population did not matter, one would want to know more about the pace of fertility decline in order to gauge the magnitude of the task of meeting targets for reproductive health programs, schooling, and others included in the ICPD Programme of Action and other social agendas. The pace and timing of fertility decline could also determine when, if ever, high-fertility countries would have a chance at a “demographic bonus” as a stimulus to economic growth, comparable to that which benefited Southeast and East Asian countries in the 1980s.

Almost all (93 percent) of the high-fertility populations are found in sub-Saharan Africa or Muslim countries of South and Southwest Asia. The report of the NRC Panel on Population Projections raised two important points that could affect the likelihood of rapid fertility decline in these regions in the next decades (Bongaarts and Bulatao, 2000, ch. 3). For Africa, the shock of the AIDS epidemic is a source of great uncertainty (though HIV prevalence is very high in many of the countries that were among the earliest to have significant fertility decline, like Botswana, Zimbabwe, and Kenya). For the Middle East, the report raised the possibility of “selectivity” -- something about Pakistan is different and has caused high fertility to persist even when its most populous neighbors are well along in the fertility decline, and whatever that something is, it may not change.

“Selectivity” at the macro level is like “unobserved heterogeneity” in microlevel studies, a signal that empirical work has to make do in the absence of a complete theory. These two issues -- the effect of the AIDS epidemic on fertility and the possibility of “Islamic exceptionalism” in fertility, are new manifestations of old and perhaps neglected issues in demography, namely, mortality-fertility interactions and culture and fertility. Progress

on these issues might be hastened by new attention to what John Casterline (2001) has recently called the “mediating cognitive factors,” the antecedents to the formation of fertility preferences.

For the countries at the low-fertility extreme, there is a symmetric concern with the age composition of the population, with attention focused on old-age dependency rather than youth dependency. The problem of adapting old-age pensions, health and long-term care programs to a drastically changed ratio of beneficiaries to workers and taxpayers has attracted much attention (see, for example, Bosworth and Burtless, 1998; and World Bank, 1994). Rapid demographic change is undoubtedly important; it makes fundamental policy reforms both necessary and more difficult than would be the case in a more stable demographic situation. But there are also many institutional factors mediating between the demographic basics and the economic outcome. For example, pension schemes can be designed so that each generation prepays for its own retirement, as in Chile and the United Kingdom. These two countries are among those aging most rapidly in the middle-income and OECD groups, respectively, yet they have among the smallest unfunded liabilities for public pensions.

For the countries in the middle, those with TFR between 2 and 4 children per woman currently, both sets of problems can be relevant at the same time, and the pace of fertility decline determines the length of this period of concern with both ends of the age-dependency spectrum (World Bank, 1994).

All realistic projections of the world population assume continued decline in fertility rates. The difference between high, medium, and low population projections is only one child per woman, which translates to a difference of 5 billion persons in 2050, well within the life expectancy of nearly everyone born this year. For populations growing rapidly, even if the rate of increase is slowing, 10- or 20-year delays in reaching replacement level fertility can mean very great differences in the size of ultimate stable populations. Casterline (2001) presents evidence that the pace of fertility decline may be slowing down, even though Bongaarts and Watkins (1996) had shown that fertility declines are beginning at lower levels of income now than in the past. If Africa and the Middle East follow the path of Latin American countries rather than East Asian countries -- getting stuck at levels above three children per woman for some decades before declining to replacement levels -- the implications could be vast. This point is illustrated by O’Neil et al. (1999) for North Africa: If the TFR for the region fell from its current rate of 4.2 to 2.0 in 20 years, the stable population would ultimately be 279 million. If the same decline took 50 years, the population of the region would stabilize at 432 million.

In these circumstances, the population sciences would be much more useful if they had produced models of fertility decline that could distinguish among these possibilities. But the theories and projection models we have do not perform anywhere near the requisite tolerance. As Ronald Lee (1995: 10) puts it, “when all is said and done, however, after these three decades of intense research ... we have little hard knowledge of the fundamental causes of fertility behavior.” Bongaarts (2001) argues that “whether desired

family size remains at or drops below 2 is the most crucial issue determining post-transitional fertility. Conventional fertility theories are essentially silent on this topic.” Dirk van de Kaa, arguing that knowledge is organized and shared even in quantitative sciences through the use of narrative, laments that “... there does not yet exist a single ‘good story’, accepted by all knowledgeable scholars, about the setting and conditions necessary and sufficient to generate the central action of fertility decline” (1998: 390).

Where would the script for the new story come from? Much of the broad-gauge theory about the Demographic Transition in the middle decades of the 20th century explained the microlevel supports for high fertility and the reasons for initial changes (e.g., Caldwell, 1982). It did not address directly the questions that should now dominate the discussion -- how low can fertility go, and how fast will it decline in the countries still above replacement level? Models derived from the New Household Economics, beginning in the 1960s, were intended to have general validity -- at least, their relevance was not explicitly limited to high- or low-fertility regimes. But as Cleland and Wilson (1987) pointed out, such couple-centered “demand theories” did not succeed in explaining much of the cross-country variation in the timing and pace of fertility decline, in Europe or in contemporary developing countries. Neither in the earlier experience of Europe nor in more recent experience in Asia and Latin America did there appear a threshold of economic development, nor of mortality decline, after which fertility decline set in. Nor have recent studies found a threshold of women’s education, nor a prerequisite of prior adoption of the ideal of a nuclear family. Changes in desired family size have been nearly simultaneous with the changes in fertility.

Much of the older empirical literature on the fertility decline attempted to partition causation between what is induced by deliberate efforts to promote family planning and what is induced by development more generally (see review in Tsui, 2001). The purpose, or at any rate, one outcome, of this literature was to generate support for family planning programs as both a humane and an effective way to stabilize the human population. But this literature is not especially designed for the later stages of the Demographic Transition; it says very little about the pace or endpoints of fertility decline in populations with near-universal access to effective contraception and abortion. The interesting variables are for the most part lumped together as “development.” This literature also now seems less relevant to its original purpose of program evaluation. The emphasis on reproductive rights of individuals that has dominated policy discussion since the 1994 ICPD makes the discussion of aggregate fertility rates as a policy target sound jarring.

The theory on which fertility projections are based is underdeveloped. People in advanced countries are able to act on their preferences, but there is little apart from borderline tautology to explain where the preferences come from. The NRC Panel on Population projections argued that fertility is unlikely to rise much above replacement level because “only massive societal transfer could conceivably compensate [prospective parents] for the trouble and lost opportunities of raising a large family” (Bongaarts and Bulatao, 2000: 106). But this assumes what needs to be explained, why so many people regard the trouble and lost opportunities as not worthwhile, and something they would have to be compensated for. Raising large numbers of children has always been costly,

as we now know, even for hunter-gatherers living in very parlous economic circumstances (Kaplan et al., 2000). People in many European cities, as Wolfgang Lutz (1999) points out, have more dogs than children. But they didn't require societal transfers to induce them to buy the dogs; they enjoy having them. Middle-class people in rich countries could afford three children and one car or one child and two cars and regular vacations -- so all these commodities (the children, the cars, the travel) have opportunity costs. The problem is that the rewards from having multiple children have fairly suddenly become lower than their opportunity cost (which was there all along, and may have been higher relative to incomes in the past), and that is what theories have to explain. Lutz quotes Nathan Keyfitz as saying that the task for countries contemplating pronatalist policy is to make having children *fun* -- "no one talks about the opportunity cost of having sex."

So we are left with a situation in which it would be both illuminating and immensely useful to predict fertility preferences in populations that control, even if imperfectly, their fertility, but our only candidate theories either explain the initial change that already happened, or take preferences as given, as in most economic demography. Where will new theories of the formation of fertility preferences come from?

It is always difficult to predict whence the next breakthroughs are coming, but two recent approaches do seem promising: 1) the study of interdependent preferences and 2) evolutionary psychology and comparative biology more generally. A few words about early returns from each of these approaches will help indicate the research, data, and training that they will require.

Partha Das Gupta (2000) has written a broad-ranging essay that illustrates what a model of interdependent preferences might explain. It is not necessary to assume that a great ideational change precedes rapid fertility decline. Under some circumstances when my decisions depend on what everyone else is doing, there can be "tipping points," such that a small change in the environment can cause a great shift in an equilibrium point (say, from one where everyone has high fertility and educates children very little) to another (low fertility and high education) without a change in values. Clothes fashions may provide a useful analogy. If everyone all along has a strong preference not to look too different from the crowd, and weak and heterogenous preferences for their favorite colors, it only takes a few people with a strong preference for black to get everyone wearing black. No one changed their worldview, but they all changed their wardrobe. Das Gupta's model does seem to fit with the tone of interviews from different parts of the world. Interviews with Thai villagers (Knodel, Chamrathirong, and Debalvaya, 1987) and with rural Kenyans (Watkins, 2000) reveal similar reactions to the environmental stimulus of formal schooling. No one wants their own children to be left behind when the rules of the game look like they are shifting and much more schooling will be required to get ahead in the future. People plan and care for their children's welfare, and they watch what other parents (the parents of their future children's prospective spouses and competitors) are doing. Thus fertility behavior can change much faster than objective indicators of the social and economic environment.

This approach gets around the devastating critique of demand theories mounted by Cleland and Wilson by shifting the focus from the couple (or the patriarch) as a unitary decisionmaker to the social environment made up of other couples. Economic models can also handle this kind of feedback (for example, computable general equilibrium models), but not easily. In a way, the emphasis on the social environment returns the population sciences to the classic concerns of sociology -- turning away from the four-decade dalliance with new household economics.

A second approach would take advantages of the rapidly developing field of evolutionary psychology, which now has its own literature, both experimental and nonexperimental, its own journals, even its own society, all dealing with topics that should be central to the concerns of demographers.

Evolutionary psychology is an attempt to understand patterns of human behavior and mental abilities as adaptations to particular problems posed by the environment during a long stretch of evolution. The core messages of evolutionary psychology are that evolution acts on mental processes, and that most of human evolution took place in a very different environment from that in which we now live (Pinker, 1997). One misunderstanding about evolutionary explanations of human behavior is the notion that they are concerned with differences among human populations, and lead toward Social Darwinism. On the contrary, they are mostly about uncovering human, and even primate or mammalian, universals.

The paradox of people feeling they cannot afford more children when (objectively) they can, may be resolved by finding stable, widely shared "if ... then" rules linking social status to preferences to behavior. If these exist, they could be studied with the methods of evolutionary biology (comparison with closely related species, "reverse engineering" to detect their purposes) and the experimental methods of psychology. The research program of evolutionary psychology is well-suited, in other words, for investigating the kind of preferences that Das Gupta posited on an *a priori* basis.

Evolutionary psychology might help guide the study of childrearing and marital stability as well as that of childbearing. Unifying theories are a time-honored way to make progress on heretofore-difficult problems. It may be that demographers have learned as much as they are going to on how to handle problems of selectivity and unobserved heterogeneity through proper specification of econometric models for survey data. Collaboration with evolutionary psychology may help triangulate on some of these problems, giving demographers new types of data, including experimental data, and new ideas about formerly "unobserved" variables to measure. It is not that demographers would have to abandon what they do best -- analyzing data with one eye on data quality and the other on specification error -- and dabble in evolutionary theory instead. Rather, the evolutionary theory could replace economic theory as a starting point for satisfying new empirical work.

It may be harder for demographers to borrow hypotheses and methods from evolutionary biologists and psychologists than it was for them to borrow hypotheses and methods in

the 1960s and early 1970s from neoclassical economics. In both cases, there is grumbling among the data-oriented, inductive demographers about overreaching, universalist claims and abstract model-building (see the Caldwell quote above!). But evolutionary theory has the added hurdle of overcoming the disreputable legacy of past linkages between population sciences and the eugenics movement.

We would not suggest that evolutionary psychology or any other branch of biology is going to solve all mysteries for population scientists, any more than the new household economics did. Some empirical tests of some of the straightforward propositions of evolutionary theorists fail, at least for the United States (see for example, Freese and Powell, 1999), just as some propositions derived from the new household economics did not work for every setting. Rather, the suggestion is that evolutionary psychology is an active and growing field in the social sciences, with interests in topics like assortative mating, marital stability, the timing and number of children, and childrearing practices. Demographers ought to engage with this field in the next decade. To do so will require some investment in learning a common vocabulary.

### **Marriage and the Family – Convergence?**

Trends in fertility rates for countries nearing replacement level need to be understood, not just because of the bearing they have on projections of the size of the stable population, but also for understanding the changing nature of the problem of caring for persons in dependent ages. It is easy to see the Demographic Transition as a story of progress -- the end state (low mortality, low fertility, low or zero growth) is preferable to the pre-transition state. But there is no such underlying agreement about Family Transition -- whether all societies are in fact on paths that converge, and if so, whether the end state is in some way preferable to what came before. Nor is there the same level of agreement about the efficacy of policy measures.

Families lost their economic function as producing units early in the Industrial Revolution, but kept their function as a consuming and income-redistribution unit for several generations longer. In post-industrial economies, Dirk van der Kaa (1987) and others have seen a “Second Demographic Transition” to a regime characterized by low proportions of adults currently married, unstable unions, high proportions of births outside marriage, and fertility rates well below replacement level. In the postmodern regime, adult men and women work for non-family-based organizations; women spend a portion of their adult lives raising a small number of children, with help from the biological father, the state, and a current male partner; and old people support themselves with pensions from the state and former employers. Adult men have relationships of varying levels of intensity with their own families of origin, the children they have fathered, and their current partners and co-resident small children. The “retreat from marriage” may not be universal, however. Divorce rates in Indonesia, traditionally among the highest in the world, have been declining steadily over the last 50 years. Modernization there has been accompanied by increased stability of marriages, along with rising ages at first marriage and increased levels of education (Heaton, Cammack, and Young, 2001).

Few analysts have made use of the household roster data from the DHS surveys, or from any of the other large-scale survey research programs like the LSMS, PAPCHILD, or CDC surveys, to examine trends and regional differences in household size and composition, to see if in fact there has been convergence toward a universal household model. John Bongaarts (2001) finds only ambiguous support for the convergence hypothesis in current data from developing countries.

The marked variations in “post-transition” life are particularly evident for children in the rich countries. The retreat from marriage and the increase in cohabitation have been general, but children’s experience of various family forms differ markedly even among low-fertility countries. The variation is not limited to whether or not children live with their two biological parents married to each other; there is also great variation in the incidence and duration of living in other family forms (with single parents, cohabiting parents, mother and unmarried partner, etc.) At the high end, Heuveline, Timberlake and Furstenburg (2000) estimate that well over half of all children in the United States and New Zealand spend some time before their fifteenth birthdays living with a single mother (not cohabiting with a partner), compared to one in 10 Italian children. Children born to married parents can expect to spend on average 2.7 years before age 15 not living with both biological parents, compared to 1.2 years in France. From the children’s perspective, there is apparently no single endpoint for family transitions in the postmodern world.

One hindrance to understanding the retreat from marriage has been the especially strong cultural and political anguish that this subject generates. Social scientists working on marriage and the family often complain that their work is taken out of context by one side or the other in the “culture wars” in the United States. Cross-country comparisons might help to de-politicize this field and encourage a renewed search for causes operating at a deeper level.

Examining the causal connections between family structures and vital rates is an old-fashioned idea. The fact that there is no clear, agreed set of family structure variables to measure in surveys and use in demographic analyses shows the stunted development of this field. Ryder (1988) did the classic work showing that fertility and mortality schedules put limits on the degree to which most families in a society can achieve the norms to which they aspire. It is unlikely that non-nuclear family types would persist unchanged during the transition from almost complete illiteracy to a situation in which a majority of both women and men are illiterate, any more than the nuclear family has remained stable either in ideal or practice.

The future of marriage and the family matters in part because of its relevance to the future of fertility decline. One of the unresolved issues in explaining the early stages of fertility decline is the debate about the significance of the ideal of the nuclear family. Caldwell argued that a precondition of fertility decline was the spread of the Western idea of the nuclear family, connected to religion, education, and consumerism. Cleland and Wilson (1987) used East and Southeast Asian examples to argue that fertility could fall to

quite low levels even while non-nuclear households remained common and the nuclear family is not the ideal. There may now be a similar problem about the role of the nuclear family in determining post-transition fertility. It has been suggested that very low fertility (at least period rates) in Italy and Japan are explained by the unpopularity of marriage for young women. The causation may run in both directions between marriage and fertility. Some have argued that marriage is less popular because one of the traditional reasons to get married, to have legitimate children, is less compelling (Willis and Haaga, 1997). Cousin marriages are common in many of the regions where fertility remains high. In a cross-national analysis, Berry (2000) found that prevailing family type (he used an eightfold typology developed by Emmanuel Todd) is significantly associated with fertility and mortality levels and trends, independent of per capita incomes. He argues that the diversity of family types could lead to a variety of post-transition fertility levels.

There are important policy and scientific reasons to be concerned about the future of marriage and the family, beyond what it might mean for fertility rates and thus the size and age structure of populations. Any significant change in the role of the family could have a profound impact on the future role of government in the economy.

Norman Ryder (1988) provided a simple enumeration of ways to solve the fundamental problem of transferring resources from those who produce a surplus to those who consume more than they produce; namely, the young and the old. One mechanism is intergenerational, within the family. A second he called "intercohort," a sharing of resources between young and old organized by the government. The third is intertemporal, whereby an individual either borrows against future earnings or lives on savings accumulated at an earlier stage of life. This third mechanism depends on mediation through financial institutions. Mortality decline "changed the age distribution in ways that made previous normative designs untenable" (Ryder, 111) by delaying the age at which young adults could expect to inherit land and increasing the number of siblings among whom it has to be divided. During the transition, according to Ryder, there will be a proliferation of three-generation households; after the transition, two-generation or even one-generation households dominate.

One of the central problems facing developing countries, even those still in the process of the demographic transition, is to find the new balance among the mechanisms for transferring resources from those in "working" to those in "dependent" stages of life (Samuel Preston, personal communication). What combination of family, state, and market will provide adequate care and money for the dependent young and the dependent old? The United States may represent one extreme, with large proportions of children living in one-parent families and large proportions of older people living by themselves. Other societies may be able to afford this degree of fragmentation in the future, but one could understand them wanting to define their own options. Even in the United States, as Ronald Lee (1997) has pointed out, the volume of resources transferred among generations within a family, largely from older to younger members, greatly exceeds the volume of resources transferred to the old through the public-sector mechanisms.

One contribution of demographers to understanding the implications of change has been to project what current trends mean for the structure of kinship over the life cycle. The rapidity of demographic change in the 20th century has been unprecedented, so families have never before had to adapt within one lifespan to such a radically altered environment. Tommassini and Wolf (2000) have simulated the effect of persistent low fertility on the caregiving burden for those in middle age in Italy in the coming decades. They find a surprisingly small increase in the number of childless and sibling-less elders. Like Watkins, Menken, and Bongaarts (1987) for the United States, they do not find a dramatic increase in the proportion of women's adult lives spent as caregivers for both young children and elderly parents. The simulations of Italian families are intriguing, but relied on a survey with limited data on kin availability for starting values.

As Kenneth Wachter (1998) has pointed out, the regime of low fertility and high rates of marital instability does not necessarily produce a cohort of older people with no kinship ties. As longevity improves even at the oldest ages, many older people will have even older living parents. Due to smaller family sizes, older people will tend to have fewer living siblings, but again thanks to increased longevity, they will keep the ones they have to a later age. Older people will have fewer of their own children, but because of multiple relationships, may be connected to more members of younger generations as step-children. From the point of view of an individual member of the older cohorts, there may be a rich variety of younger people with whom one is connected through biological or fictive kinship relations. For society as a whole, though, these relationships are bound to be diluted, compared to current situation, since the ratio of working- (and caring-) age people to the old will be so much smaller. In a postmodern society, one could not be expected to keep up with, let alone care deeply for, all the ex-partners of one's parents, and one's parents.

These kinship simulation projections deal with issues that have clear importance to policy makers and generate strong public interest as well. How many AIDS orphans will there be? How many older people in rural areas of Africa will have to spend their old age caring for their orphaned grandchildren? Who will be available to care for the elderly? Yet they do not appear to have been very widely exploited, for developing countries in particular. Development of kinship simulation models in software (preferably in widely used statistical packages) adaptable to the kinds of data available for most developing countries could be a good investment that would stimulate policy-relevant studies in countries at various stages of transition.

### **The Future of Mortality Differentials**

Including mortality differentials in a list of "new frontiers" may seem paradoxical. At the most macro level, the differentials have narrowed. In the last five decades, the difference in life expectancy between the developed and the developing countries (considered as blocs) has fallen from 25 years to 12 years (Chesnais, 1999: 127). Mortality differentials are hardly an unfamiliar topic for the population sciences. But just as with fertility decline, the study of mortality differentials is a frontier again, partly because of a need to explain why differentials within populations failed to disappear.

Samuel Preston published in the mid-1970s graphs of the cross-national relationship between life expectancy and per capita income, along with an analysis of change over time. They showed a curvilinear relationship, flattening out at upper levels of income. Progress in reducing mortality at old ages seemed to have stalled in the United States in the 1960s, and demographers spoke of the rectangularization of the survival curve. These empirical relationships could be reconciled with Omran's idea of the Epidemiological Transition. It seemed reasonable to suppose that mortality differentials would be compressed, across countries as more of them reached the levels of per capita income at which the curve flattened out, and within countries as infectious diseases and infant deaths waned and the poorer classes benefited from access to medical care.

Three developments have made mortality differentials a subject of renewed interest. The most obvious is the AIDS epidemic. Before 1980, infectious diseases were obviously still the leading group of causes of death for much of the world's population. But the infectious disease causing the most deaths -- vaccine-preventable diseases of children, malaria, puerperal fever -- were well understood, and their control could be considered an organizational, economic, and political problem, rather than a demographic or epidemiological mystery to be solved.

The second development is the evidence from Eastern Europe that mortality improvements can be reversed. This is often discussed as a problem of Russia since the breakup of the Soviet Union, but the problem actually affects more countries of Eastern Europe and predates the breakup of the USSR. In the 1960s, for example, life expectancy for Czechs and Slovaks was higher than that of Austrians; now it is considerably lower, since the latter continued to improve while that of the Czechs and Slovaks worsened (Feachem, 1996).

Lastly, there has been renewed attention to mortality and health differentials within national populations, including a growing body of evidence that social inequality affects the risk of death even when the disadvantaged have access to what would seem to be adequate medical care.

These developments would seem to be challenges primarily for epidemiologists. What do the other population sciences have to contribute? This may be a field in which the second of the approaches to defining demography is the relevant one -- demography as a set of data sources and methods, with potential for a distinct contribution to epidemiology in the next decades.

Jack Caldwell and Susan Watkins have both argued that demographers were well-placed to help with the study of AIDS because of their comparative advantage in the study of sexual behavior at a population level. The AIDS epidemic is obviously a central concern for anyone working on African population issues. As an outgrowth of the study of fertility determinants, demographers and anthropological demographers have developed ways to elicit reports of sexual behavior. As a byproduct of past work on family planning programs, demographers have also proved useful in the design and evaluation of

prevention measures. Demographers have managed surveillance systems in low-income countries, often as a by-product of the need for sites where vaccines can be tested. In regions where mortality rates are still high and vital events registration still underdeveloped, the development of reasonably accurate indirect estimates of mortality is an important need.

Beyond their specific contribution to the study of the AIDS epidemic, demographic approaches may prove valuable in the health sciences more generally. Old distinctions between “behavioral” risk factors, properly the study of social epidemiology and health psychology, and biological or genetic risk factors, may be breaking down. The issue of *Science* announcing the publication of the “complete” map of the human genome had a very useful short article on recent work in behavioral genetics and how the Human Genome Project is likely to make further advances possible (McGuffin, Riley, and Plomin, 2001). By now there are enough twin and adoption studies of heritability to justify their conclusion that “genetic variation makes a substantial contribution to phenotypic variation for all behavioral domains” that have been studied. In fact, the genetic component of variation for behaviors tends to be higher than that for neurological or mental diseases (see their table “Current Understanding of the Genetic Basis of Selected Behavioral Disorders and Traits”).

The key contribution of the last decade has been the ability to model Quantitative Trait Loci, which has allowed geneticists to move beyond Mendelian inheritance in population studies. They no longer search for “the gene for X” when X is an expression of multiple genes in a particular environment. Demographers are interested in processes that are fundamental to life histories, especially of social animals -- choice of mates, timing of fertility and the number of children, childrearing, age at death (Kaplan et al., 2000). These are so fundamental that no one expects to find “a” gene for anything important affecting them, and thus the ability to find (and handle statistically) data for multiple genes affecting them means that genetic knowledge is becoming useful for problems demographers actually care about. And increasing numbers of demographers are in fact collaborating productively with geneticists and evolutionary theorists on the study of mortality (see, for example, Wachter and Finch, 1997).

A key point was made by Richard Udry (1994) in his presidential address to the PAA: Removal of some of the artificial social and economic constraints on behavior can free people to act on their preferences. The genetic component in the determination of preferences could thus account for more, not less, of the variation in behavior in developed societies. In an earlier presidential address, to the Southern Sociological Association, Udry had asked the rhetorical question “How Much Biology Should a Sociologist Know?” With the extraordinary progress that has been made in behavioral genetics in the past two decades, the answer now should be “a lot.”

### **Urbanization and Environmental Consequences**

Some time this decade, for the first time, more people live in cities than in villages or hamlets. About 90 percent of world population growth in the next quarter-century,

according to United Nations projections, will occur in the urban areas of developing countries.

The urban population is growing, but the growth rate of most “megacities” has been lower than forecast, even as recently as the 1980s (Brockerhoff, 1999). For some regions, this reflects the fact that the growth of total populations was slower than expected. But for Africa, this is not the explanation, since the proportion of the population living in cities has increased more slowly than anticipated. The smaller share of primate cities in national populations over time suggests that there are feedback mechanisms, dynamic elements not fully accounted for in the first generation of theories of urbanization in developing countries. Brockerhoff (1999: 775) argues that “analysis of the causes of urban growth ... [w]ith few exceptions ... has been neglected for some time, despite its obvious demographic significance.”

The future of urbanization is linked to the future of fertility. As Brockerhoff has argued, overall fertility rates are affected by the speed with which new migrants from the countryside or small towns conform to urban behavior. The urban future is also connected with the future of the family in developing countries, discussed in the preceding section. One of the motivations for urban migration, according to Oded Stark (1991), is the spreading of economic risk among members of an extended family. The relevant economic decisionmaking unit, in these models, is a non-co-resident family, only intermittently a household as in conventional economic models. To the extent this is true, and the ties survive the migration experience, then transfers within the family should mitigate the problem of population aging in rural areas and link the fortunes of cities and rural areas in complex ways.

On a larger stage, urbanization and international migration are linked. The 2000 U.S. Census showed surprising growth, or less-than-expected population losses, in many of the central cities of the United States. The migration data are not available yet, but it seems likely from the race and ethnicity data that much of the explanation is that central cities in the United States were the destinations for large numbers of migrants from Mexico, other parts of Latin America, and Asia. It is important to understand the migration decisions of Mexicans, to understand both why Mexico City is growing slower than expected and why Chicago is growing at all.

There is another sense in which the study of urban and metropolitan growth in the United States could benefit from a more international perspective. The literature, and still more the political discussion, on metropolitan growth in the United States have always been dominated by a sense of American particularism. Suburban sprawl, in this perspective, is a necessary, perhaps unfortunate, outcome of the vast distances, the sense of room, the need of Americans for space, and their love of the automobile. Yet Canadian metropolitan areas are more compactly settled, despite all the same alleged causes being true of Canada as well. Americans live happily in many suburban communities at high densities, provided they have the suburban amenities of good schools and police protection and low taxes. It is likely that progress in understanding metropolitan growth,

and the possibilities for affecting it through policy, would be helped in both rich and poor countries by a North American or even a broader geographic perspective.

The consequences as well as the causes of urbanization deserve a new round of research attention. Much of the early work on megacities emphasized the overwhelming problems of clean water, waste disposal and air quality, and this theme also appears in reviews being prepared for the World Habitat Conference. Geographers working on environmental issues have introduced the idea of the “footprint” of large cities in the rich countries, the impact that the activities of one city can have on land use all over the globe. More optimistically, environmentalists have also developed a stylized model of “environmental Kuznets curves,” showing that the air and water quality indicators at first worsen and then improve as per capita incomes improve.

There is a disconnect, though, between the policy discussions and the demographic work on urbanization. Barbara Torrey puts it this way: Which is worse for the environment, a megacity of 20 million or five mid-size cities of 4 million each? Mexico’s urban population is going to grow by 5 million in the next decade, more or less. Would it be worse for Mexico and for the world to have Mexico City reach the size once projected for it, or to have another Guadalajara? This may not just be a thought experiment, since at the margin policy might influence location choices of businesses and households.

The locally harmful consequences of urbanization -- overload of sanitation facilities, crowding leading to increased disease transmission, breakdown of transportation systems, problems with basic governance, overloading of the ecosystem restoration processes -- are basically problems of density, which could reach their peak effects even in small or mid-size cities, affecting just as many people in the aggregate as a single much larger metropolis. The globally harmful consequences, such as greenhouse gas emissions or deforestation for rangeland to feed them, are a function of how many people there are and how they are living, not so much where they are living. It could be useful to guide both urban policy and environmental policy to have a much more nuanced literature on what makes urban growth harmful.

For environmental consequences of local or global population growth, there are many mediating institutional factors involving incentives to pollute and the ownership of common resources. Populations could be growing slowly or even declining and still create severe and worsening environmental threats, as was seen from East European factories, the burning of dirty coal in China, or the per capita emissions of greenhouse gases from rich countries. In the terms of John Holdren’s “I=PAT” formulation (Environmental impacts are a function of the population, affluence, and technology), the interesting, complex, and policy-relevant issues tended to concern the links between affluence and technology and environmental impacts. The “P” term was needed as a multiplier for per capita impacts, to add urgency to the problems. What environmentalists wanted from population scientists were only the projections, and these can be produced by (surprisingly small and lightly funded) units at the UN and the U.S. Census Bureau.

In the future, however, the connections between population sciences and environmental studies could be much richer, if more were known about how changing age composition, household structure, and settlement patterns affected environmental outcomes, as well as just population size.

### **International Migration**

Some 120 million people are estimated to be living outside the country in which they were born. (That doesn't necessarily mean they moved; sometimes, as in the breakup of the Soviet Union and Yugoslavia, it was the international borders that moved.) In international law and in the organization of relief, the distinction between refugee movements and internal displacement matters, and the distinction between voluntary and involuntary movements is crucial. For research on causes, duration, and consequences of movements, though, sharp dichotomies are probably less appropriate than a continuum of "push" and "pull" factors causing people to move different distances, permanently or for short times.

Several of the opportunities for progress in migration studies that have been discerned by those working in the field involve the dissolution of old boundaries -- between refugee studies and the rest of migration; between migration and the rest of demography; and for developing countries, between migration and economic development policy. (In the section that follows, I rely much on personal communication with Charles Keely, Susan Martin, and Andrew Schoenholtz, but will not repeat this citation after each idea borrowed from them.)

First, there is the divide between the people and institutions working on refugee issues and the larger world of migration studies. Black (2001) points out that the academic field of refugee studies has maintained a strong link to the practical problems of relief, return, or resettlement. Much effort has also gone into typology, which has a normative impulse. "How should internally displaced persons, environmental refugees, or those displaced by development projects, be classified?" is linked to the question "How, and how much, should the international system come to their aid?" The situation Black describes seems analogous to what some observers feared would happen with the study of fertility, if it had been entirely subsumed by the operational problems of the family planning programs. If family planning programs were as complex as refugee relief and if they inherently involved multiple governments and international agencies with different mandates, straddling borders, and the constant threat of violence and local jealousies, perhaps fertility theory and the data collection to support comparative studies would never have progressed. It may be that the simplicity of the programs (relative to refugee operations, that is) has allowed the science of fertility to flourish.

Douglas Massey (1999) has argued that one of the weaknesses of (unforced) migration theory until recently has been a failure to take into account the roles of governments. Perhaps this is the contribution that the world of refugee studies can make to the larger study of migration, since the role of the state, or the absence of states, are prominent in refugee case studies and normative analysis. One could picture useful work, if not on a

grand unifying theory, at least on typologies and case studies, in which migration as an outcome is set along a continuum with varying degrees of “forcedness,” rather than a dichotomy.

The links between migration studies and the rest of demography could become more important, especially for the low-fertility countries like the United States in which migrants and their children are projected to account for a steadily growing proportion of the increase of the population. Projections of the future fertility of the United States, for example, contain implicit assumptions about the pace at which fertility of immigrant groups will converge to that of the native-born. “Migrant paradoxes” may be important clues to factors affecting fertility and mortality more broadly. Mexican migrants to the United States have higher fertility rates now than do Mexicans in Mexico, and lower infant mortality rates by far than native-born Americans living at comparable incomes.

The development of kinship and social networks crossing international borders could likewise add an interesting new comparative element to family demography. At one extreme are what Charles Keely calls “interconnecting diasporas,” the phenomenon of national groups with long-standing connections in multiple states (such as Palestinians, Kurds, Afghans). The old paradigm of migrants leaving one country, willingly or not, settling in another, possibly returning to their native land, may not describe very well the life of these ethnic groups in the decades to come.

Comparative policy studies are likely to grown in importance (Cornelius, Martin, and Hollifield, 1994). The next decade could also be an opportune time for innovation in U.S. immigration policy (again, assuming the attacks of September 2001 do not elicit a long-lasting change of plans.) One sign is the discussions between the United States and Mexico, which seem to be heading toward a new development of legal but nonpermanent statuses (Martin and Teitelbaum, 2001). If these programs expand and proliferate, they could mean that immigration policy in the United States will look more like the guest worker policies of Europe in the 1970s and 1980s. Alternative proposals are for changing the system of preferences for permanent immigrants, away from family reunification and toward economic criteria, which would push the United States in the direction marked by Canada and Australia. In either case, the result should be to generate more interest in cross-national comparisons, and a breaking down of the old assumptions of American exceptionalism in immigration matters. Massey (1999) has described a tension between the trends promoting migration from developing to developed countries – growing wage differentials, cheaper travel and communication, the development of transborder family and social networks – and what he sees as strong political and economic pressures on governments in developed countries to restrict immigrant numbers. He was writing before the recent terrorist attacks on the United States and the current economic recession, but these events reinforce his forecast.

Immigrant assimilation is a topic of enduring and probably growing interest in receiving countries. In the United States, the bipartisan Commission on Immigration Reform (1996) brought renewed attention to the need for public policy toward immigrants after

they are in a country, not just policies about how many and which immigrants to let into the country.

Some countries still commonly categorized as “developing” are themselves receiving countries for international migration, or simultaneously sending and receiving countries. The comparative study of immigration policies could usefully involve more than just the traditional receiving countries and Europe.

Comparative policy studies among sending countries would also be useful. The long-standing concerns with the “Brain drain” are still real. The 1992 Human Development Report estimated, for example, that over a million professional and technical workers had left developing countries for the United States, the United Kingdom, and Canada alone in the preceding three decades. Of the approximately 110 million persons now alive who were born in Mexico, nearly a tenth live in the United States. But many developing countries encourage emigration, or at least organize it to encourage remittances. Studies linking the microlevel study of motives for migration and patterns of return and circular migration, transborder kinship networks, and remittances, with the macrolevel study of development policy, would have great policy significance.

Innovation in data collection, attention to data accuracy, and clever indirect estimation of rates that cannot be directly measured are all among the traditional contributions of demographers. Few substantive fields need these gifts now more than the study of illegal international migration and forced migration. Learning from the 2000 U.S. Census that nearly 7 million more persons resided in the country than official estimates had indicated was a reminder that even a rich country can be surprised. Large bands of uncertainty have to surround estimates from countries with poorer statistical systems, even in the best of times. Forced migration, whether of internationally recognized refugees or of “internally displaced persons,” adds a challenge, since the same conditions that create the need to move make censuses and surveys impossible. The UN Population Division has not issued estimates of the number of international migrants since its 1990 estimates, and budget cuts mean that few staff resources can be devoted to migration. Charles Keely sees a need for greater international exchange on “how to count the uncountable,” one that could stimulate the thinking of demographers on eminently practical problems.

### **Preliminary Evidence About Where Population Scientists Are Heading**

So far the analysis for this report is purely normative, based on the opinions of those whom I interviewed or whose works I read about what *should* happen. But in making decisions about research funding, realism is needed. Well-established researchers do not deflect far from the paths on which they are traveling simply because a research funder wants to see something done. Less well-established researchers do not lightly stray too far from the paths marked out by those who are older and better known. Academic research is always in a real sense curiosity-driven. What evidence is there that population sciences are in fact interested in these topics, and in cross-disciplinary and comparative approaches to them?

Content analyses of articles in *Demography* (Teachman, Paasche and Carver, 1993) and *Population Studies* show some broadly similar trends, despite the different styles and editorial preferences. The proportion of articles on fertility and contraception peaked in the early 1970s. In *Demography*, marriage and family had emerged as the largest single category in the schema used by Teachman, Paasche and Carver by the early 1990s. Migration, the stepchild of demography, had held steady for years at about 10 percent of the articles published. Of course, family and migration studies are not proper subsets of demography, and they have their own specialized journals, in which many PAA members publish.

In a survey of IUSSP members (with 637 respondents, the majority from Europe), population aging was ranked first among “phenomena that have most influenced demography,” ahead even of “rapid population growth in less developed countries.” This certainly shows a lively concern with the issue (“Demography on the Threshold of the Year 2000,” accessed online at <http://census.ined.fr/Demograpahy2000>, on Dec. 5, 2000).

The future content of the journals might be surmised from the programs of recent meetings of the Population Association of America (PAA). With the help of Suzanne Bianchi and Arland Thornton, I have been able to supplement the record with a look at the submissions that did not get accepted, as a measure of new ideas on which members are working. Some of these, of course, will never take off, but they may be indicative of evolving interests. For the PAA annual meeting in 2000, 48 regular sessions on migration and urbanization were proposed by members, more than for either fertility and contraception or for health and mortality. Evolution and behavioral genetics were suggested as topics by three people. The future of the family was directly the subject of only one suggestion for a session, but several dozen others were concerned with policies affecting families, fatherhood, the effect of family structure on child outcomes, and other such topics related to current trends in family life.

From this we might conclude that it is likely that an initiative calling for more work on family, household, and kinship demography would elicit a good reaction, with many institutions already poised to organize projects and supervise trainees. What a new initiative would add would be to internationalize what currently seems a geographically fragmented field. Initiatives on migration and urbanization would likely generate a response from within well-established research communities working on these topics, but new directions would probably have to involve much network-building among disparate disciplines. Initiatives seeking new thinking on fertility preferences, especially through links to evolutionary psychology, would be addressed to a smaller audience, but it does already exist. The study of mortality differentials has a long pedigree in population sciences, and a natural audience in that PAA members and sometimes whole PRCs are housed in Schools of Public Health. The task for this topic would be effective linkage with the new work made possible by the new genomics and the linkage of social status to somatic processes. The barriers to trade in this case are low, since Epidemiologists serve as a linking discipline between the Social and Life Sciences for health and mortality (see

the papers presented at a conference on demography and epidemiology held at Georgetown University, in Weinstein and Hermalin, 2001).

Particularly important for any estimate of where the field is headed are the motivations of students. Why are people taking population courses? I asked that question of several people who teach at leading universities, and received no single answer, which is probably a sign of good health. Graduate students, not surprisingly, tend to be interested in what the professor is best known for (e.g., AIDS in Africa; marriage and the family); that's why they're there. Undergraduates often take courses with the word "population" in the title because they are primarily concerned about the environment (Patrick Heuveline, personal communication). At the University of California, in the first large American state in which Non-Hispanic whites are a minority, undergraduates push for more about international immigration in the population courses, according to Ronald Lee. Both Mark Hayward and Myron Guttmann report that there is strong demand from students in Human Development and Family Studies programs for demography courses and joint degrees.

These statements reinforce an impression that there is interest in all the topics discussed here, and that population sciences are seen as relevant. They could be disturbing, though, for anyone concerned about maintaining a disciplinary core. One could picture population studies developing centrifugally as this generation of students moves into teaching and research.

### **Fostering Problem-Oriented Research and Training**

The preceding sections tried to make the case that there are major unsolved problems affecting human welfare and closely related to core concerns of the population sciences-- fertility, marriage and the family, mortality, and migration -- and further that there are reasons for thinking that new approaches will pay off in the not-too-distant future. But the approaches tentatively identified here all entail some crossing of traditional disciplinary boundaries and a "globalization" of research. The institutional setting for research and training, and the career paths for researchers, are not always conducive to this work. It is not only a problem of the well-known inertia of universities; the way research funders divide up the world has also contributed to current rigidities. Progress on these issues could also be helped along by the creation of a new data infrastructure for comparative research, one not so closely tied to programmatic needs, but optimized more for cross-national and interdisciplinary research.

In this section, we discuss ways in which research funders may be able to foster development of people, data, and institutions.

### **Developing the People**

The question "What do the Population Sciences have to contribute to the solution of these problems?" is distinct from the question "What will the current generation of demographers contribute?" Intelligent and creative social scientists with talent for

theorizing and research skills will always head toward important problems. The role of research funders might not be to steer the best people toward the problems they want studied, but to hasten progress by helping match good young researchers with good opportunities.

Dan Lichter sees a generational split between population scientists trained before 1980, when the global Demographic Transition dominated the field and gave it unity, and the current generation of students. The faculty of population research centers, in his view, are still concerned “largely with international population issues and the traditional demographic components of fertility, mortality, and migration and certain conventions of data and method” (Lichter, 2000: 3). For a newer generation, those trained in the 1990s, there is much less that is distinctive about demography. “Younger demographers have responded to the exigencies of the tenure process and have retreated from traditional demographic topics and formal demographic work. They have also retreated from time- and travel-intensive international research” (Lichter: 4).

The problem with international studies in particular may be part of a more general trend of disengagement with the outside world among young Americans, reflected, for example, in television and newspaper coverage (Lindsay, 2000). Early reports of reactions on university campuses to the terrorist attacks of Sept. 11, 2001, indicate a resurgence of interest in International Relations, and the Middle East in particular, though it is obviously much too early to tell if that is a quick reaction or a long-lasting change. The concern is that older cohorts of American academics who shared their contemporaries’ interest in the world outside the United States would gradually give way to younger cohorts for whom international concerns rank low.

Our concern is with the population sciences, broadly conceived, rather than Demography as a discipline. Few graduate degrees are awarded in Demography; even in universities that do have such a degree, many professors encourage Ph.D. students to get their degree in one of the larger disciplines with a minor in Demography, in part to broaden their job prospects. Hybridizing disciplines may be a sign of strength, if the population research centers (PRCs) are lively spots for intellectual exchange, whose affiliated faculty offer courses that students find useful, and if the research projects housed there make progress on worth problems. The cohort replacement that Lichter discerns does raise a problem for the kind of work we have called for in preceding sections, though, if it means that the PRCs become less involved in cross-national comparative research or less able to provide relevant training to scholars from developing countries.

A strategy for encouraging both a continued focus on international work and productive collaborations among disciplines should probably include measures aimed at several phases of the scholarly life course: predoctoral, postdoctoral, and mid-career. We should also think about movements in both directions across disciplinary and geographic boundaries.

A byproduct of the needs of the international family planning movement for research and evaluation was the availability of support for travel and study and consulting by U.S.-

based academics, giving them some first-hand knowledge of the problems of developing countries and also creating opportunities for collaboration. Though we have no direct evidence on this point, it is likely that the natural progression of the programs has probably lowered the demand for these consultancies. Family planning programs are mature, and there are specialist consultants and institutions, including many in developing countries, working on project design, implementation, evaluation, and survey research. Even when universities are doing the work, these are often specialist units within universities operating on “soft money” with tenuous links to the academic departments. Training young researchers, or giving them significant professional experience overseas, may be a less-common byproduct of program operations in the future than it was in the 1970s and 1980s.

The disciplinary organization of universities creates disincentives for ambitious researchers early in their careers who might otherwise consider straying across boundaries. Too much time spent on international projects, which are often subject to delays, could slow down the production of publications that count for academic tenure. Time spent acquiring skills in a new discipline, research method, or language would be less advantageous than the equivalent time spent generating articles in core journals of one’s original discipline.

It is probably not possible for any but the very biggest research funders to reshape the early career track for academics in the United States. But travel and short-term training awards with application procedures that are not too onerous might make it easier for those who are adventurous to test the waters without committing themselves to career-threatening changes of plans. Such awards might include short-term (2-3 months) travel to institutions in other countries. This would not be enough time to get a serious project conceived and completed, but it might be enough for a young scholar to get an article finished, to make or reinforce personal contacts, to find out about new data sets, and to give a seminar or perhaps lead a short course on a new method with which she is familiar. Reciprocal awards for travel both to and from PRCs in the US might keep contacts fresh and interests alive. Such short-term visits would not have the same payoff as a longer fellowship, either for the awardee or for the host institution. But if the goals are to maintain the international outlook of institutions in the United States, and to build ties for future development, this might be more cost-effective than relying solely on those willing to commit to a single long period away from the country where they intend to work.

The boundaries crossed may be disciplinary as well as geographic. One model that might well help facilitate future productive collaborations is the “summer institute,” like the institutes on Demography and Economics of Aging and on Health and Aging for Social Scientists that RAND organizes. These bring in young researchers as participants, for a week of “master classes” given by senior researchers, as well as practical help in getting started with new and complex data sets. It is unlikely that one such course would be sufficient to allow a young researcher to start making productive contributions in a new field, nor even to write convincing proposals. But well-designed short courses could help prepare demographers to be good partners in future collaborations with biologists, epidemiologists, physical anthropologists, and evolutionary psychologists. The reverse

would also be useful -- a short course in Demography and Population Biology for young and midcareer researchers in the Social and Life Sciences who want to know enough to work on issues central to the population sciences.

Those I interviewed for this project were often ambivalent about the utility of post-doctoral fellowships in general as a means of fostering interdisciplinary research. On the one hand, recent Ph.D.s are not yet settled into a research agenda, with others dependent on them for funding. Jane Menken suggested that people who have recently been awarded tenure may be at just the right juncture to consider new directions and take chances on work that might have a long-term payoff.

The exigencies of getting research funding in a competitive world discourages even established, senior researchers from moving outside their specialties. Even so, there have been several persistent and unusually curious scientists who have managed in mid-career to learn demography. They now bring an extraordinarily rich perspective to population issues, one that allows fruitful syntheses of work that is not usually considered together (see for example, Kaplan et al., 2000; or Carey and Judge, 2001). One would not expect many people to be able to do this when they are well along in their careers, nor would it necessarily be a good idea for most. (There are advantages to the division of labor.) But there seems to be a high payoff to encouraging those who do have a taste for it. Paul Krugman has noted that it would be a natural transition for more economists to pick up on evolutionary biology -- it does not require a massive investment in learning new types of mathematics, since the formal models look very familiar (Krugman, 1996). There is a basic logistical problem that people cost more to support when they are older than when they are younger. But they also have more of a track record on which to judge the likely outcome of an investment in their career development. Senior academics may be eligible for sabbatic leave, and willing to use that time for branching out intellectually. Research funders might consider asking PRCs to identify colleagues in other disciplines, or in institutions overseas, with whom they have a reasonable congruence of interests for future collaboration, for potential funding for a fellowship outside the field in which they have done their previous work.

### **Developing the Infrastructure of Data**

As discussed above, the population sciences are distinguished as much by a skeptical approach to wayward data as by subject matter. In some disciplines, such as Anthropology, it is customary for young scholars to be involved in primary data collection. In others, such as Economics, only a few specialists get involved in primary data collection early in their careers. But as Preston (1993) has argued, Demography is “perhaps the most inductive of the social sciences,” and without interesting data suitable for multivariate analysis there is not much for demographers to contribute, however fascinating or potentially relevant the topic.

During the 1960s and 1970s, an increasing proportion of demographic research consisted of analysis of microlevel data from cross-sectional surveys (Teachman, et al. 1993; Lee, 1995). A central problem was how to test causal models with such data, and one of the

solutions imported from econometrics was the estimation of multi-equation models using instrumental variable techniques. These never enjoyed the full allegiance of population scientists, perhaps because of their “fragility” -- analysts had too many choices in specification, replication was difficult, and measurement error, common enough especially in data from developing countries, produced bias in coefficient estimates of unknown direction. For testing of policy models, data on the supply side of public services were also needed, which required collection of data on schools, clinics, hospitals, etc., beyond the mere fact of whether a facility existed and how far away it was.

Three strategies in data collection have developed to get around the excessive reliance on clever modeling of cross-sectional survey data: 1) longitudinal designs; 2) linkage of survey data with in-depth interview material or other “qualitative” data; and 3) linkage of household and institutional data. Each of these three developments has allowed important advances in analytic methods. The linkage of ethnographic methods to survey research, for example, has helped the study of sexual practices and the risk of HIV transmission. But each of these refinements has threatened to make cutting-edge data collection more expensive, technically complicated, idiosyncratic, and more confined to the rich countries.

The major international survey research program, the DHS, is an outstanding achievement. Comparability (not just of questionnaire items, but also of missing values, file organization, etc.) makes cross-country comparisons easier. But the growing divergence between what demographers need to investigate (topics like those we have discussed), and what the cross-national survey programs sponsored by USAID, UNFPA and UNICEF produce, means that some new efforts need to be made to produce appropriate data infrastructure for a new generation of cross-national studies. Like earlier cross-national programs of data collection (the KAP surveys, the World Fertility Survey), the DHS and CDC survey research programs were aimed at producing fairly standard cross-sectional surveys, usually with a very limited range of household economic data or data on facilities. In some countries, with supplementary funding, the DHS program has produced more detailed data on consumer expenditures, or on facilities; some DHS data sets have been linked to in-depth data, for example, on unmet need for contraception; and some have even incorporated a modified longitudinal design. (For these reasons, the DHS program is now officially known as “MEASURE DHS Plus”). But the basic design is not meant to produce these refinements for research. Cost pressures have kept the focus on getting basic indicators measured as frequently as possible, for subnational areas when possible.

For program purposes, even before the ICPD, there was a declining need for data of the sort that are useful to test models of fertility decline. Since modern means of fertility regulation have proven acceptable in all parts of the world, and since much is known about how to design and implement programs, the international family planning movement has “solved” its intellectual problems, leaving behind only implementation problems. The major survey programs sponsored by USAID are increasingly designed to produce data for program indicators, not for testing alternative models of fertility decline against each other.

Like funding for data collection, much of the funding for research on fertility preferences in developing countries had a program focus. Questions that have been of particular interest include: Do couples communicate about, and negotiate, contraceptive use? Does son preference affect contraceptive behavior and fertility? Can preferences and behavior be affected by deliberate attempts to spread information, through the mass media or face-to-face communication? Almost by definition, this literature is looking at the margins. What is the extra impact of the program or the son preference or the spouse? What is needed for the questions about the future, though, is something on a grander scale: What determines rapid changes in preferences, fertility plateaus, and large gaps between stated preferences and achieved family sizes?

One difficulty in the way of comparative work on changes in the family is the lack of standard vocabulary and corresponding questionnaire items. There is no widely used typology for family structure or household types, nor a framework for classifying proximate determinants, such as Blake and Davis and Bongaarts provided for the study of the Fertility Transition. Besides needing more individual research projects on changes in marriage and the family, the field may need some synthetic and typological projects. It would be especially useful to develop model questionnaires for household rosters that could be widely adopted in surveys.

Comparative work using survey data sets is a well-established tradition, thanks to the effort put in by the WFS and the DHS staff to producing standard recode tapes and clear documentation. Now DHS data are available over the Internet, subject to restrictions imposed by the countries concerned, which should facilitate access by researchers in developing countries, researchers interested in exploratory work on a large number of data sets, and researchers in the future interested in historical comparisons. Qualitative data, by contrast, are typically not so accessible, not well-documented, and little-used for secondary analysis and comparative studies. The availability of the Internet and some software packages that can facilitate the analysis of textual materials should make it possible for researchers to be more ambitious in the future, if an investment is made now in Internet archiving of qualitative data. There is some precedent for cross-national comparisons, predating the Internet era. The Human Relations Area Files at Yale University served as an archive for ethnographic material, and also allowed some standard coding of ethnographic observation that allowed valuable comparative work on topics like infant feeding.

A similar opportunity exists for innovative data of other types, notably longitudinal data sets and data sets with extensive community-level and facility observations. To test hypotheses about fertility motivations, and intergenerational transmission of behaviors, there is a need for data that tracks people through different stages of the life course and families through multiple generations. Longitudinal data sets, notably the National Family and Household Surveys for the United States, have allowed the study of movement among a more complicated set of unions than the standard categorization used in traditional demographic surveys (never married, married, divorced or separated, widowed). This has made possible a new literature on cohabitation as practiced in the

United States, Canada, and Europe. The lack of similar longitudinal data sets with comparably rich information on family and household types for other countries hinders the study of the one of the most important implications of social and demographic change.

Truly longitudinal studies in developing countries are fiendishly expensive and require a long time to produce results, so there is a good case for secondary analysis that fully exploits the data that do exist. Data collection efforts that are adjuncts to foreign-aid funded programs have no particular reason to go back and find respondents. Several of the classic field studies in developing countries in the 1960s and 1970s produced data sets with a rich variety of variables on children and their families going well beyond those of standard demographic surveys; for example, the Children of Santa Maria Cauque, the INCAP village studies in Guatemala. It would be useful now if researchers could have access to those data sets to compare across time and space with the longitudinal data being produced now. How did families adapt to the demographic transition during the acute phases? It would also be useful for many purposes to have followup data on the families and children who were part of intervention studies or control groups. In at least one such case, the children of the INCAP studies, long-term followup not envisioned in the original design has proved both feasible and interesting. There are some families in the Third World about whom a great deal is known, at least potentially. Recent efforts to link researchers working at different demographic surveillance sites in developing countries are an excellent start for building an infrastructure that could support truly comparative, truly longitudinal studies of family change in multiple world regions.

An effort to make older data sets more widely available, or to link them to followup data, will run into both practical and ethical difficulties. The original research teams have typically dispersed, and the old data sets are often sketchily documented. Informed consent procedures would have to be worked out in a manner satisfactory in the countries where research was conducted, the countries of researchers, and the countries of research funders, all of which may have different criteria and concerns, and some of which have changed their rules over time. There are legitimate concerns when data collection is separated too much, in time or place, from data analysis -- those who created data sets may feel exploited, those who do the analysis may miss important elements because of their lack of context. But these difficulties must themselves be put in context. Informed consent issues are a problem for all cross-national social research, and will be more of a problem as collection of biological information is added to social research. Nor do the ethical concerns all point in one direction, against the research. In many research projects in developing countries, there was always more of an intention to train local scientists and address local concerns than ultimately proved possible. Making data sets lastingly accessible on the Internet might mean that host-country researchers get more out of these data than has been true in past projects. And the gap between data producers and analysts is a problem for all secondary data analysis, whether cross-national or not. There are real problems, but also real opportunities.

A good deal of work will be needed to document and make available on the Internet (with appropriate disclosure review) longitudinal data sets from many countries. Such work is

a public good, and gets crowded out of projects when data collection proves more expensive and takes longer than expected, which is often the case. The RAND Labor and Population program has done a creditable job in making the Family Life Surveys (from Indonesia, Malaysia, and Guatemala) accessible on the Internet, along with good documentation. This work is expensive, but the availability of the Internet makes it a cost-effective way to extend the useful life of data sets greatly.

Some have expressed regret at the possible loss of demographers' traditional, and distinctive, concern with errors of measurement. Ansley Coale and James Trussell (1996: 484) concluded from a review of the development and use of demographic models: "While the trend toward the use of sophisticated statistical models appropriate for the problem to be analysed and the data that are available is healthy, the trend toward accepting demographic survey data at face value is not." To preserve the valuable tradition, new ways have to be found to allow young researchers to be "close to their data." despite the increasing expense and complexity of data collection. Any efforts to build the new data infrastructure ought to include ample opportunities for younger researchers, preferably an international mix, to spend time working with those active in data collection and processing.

### **Developing Institutional Environments**

It has been pointed out that for all the intellectual advances of the twentieth century, the basic departmental structure of a modern university -- the divisions in the Arts and Sciences; the professional schools -- would be quite familiar to academics of the late 19th century. Derek Bok, former president of Harvard University, has likened changing a university curriculum to moving a cemetery. The stability of organizations should remind us that there is a real cost to interdisciplinary work, and it ought not to be recommended without good reason. This stability also poses a challenge to research funders: To what extent should they encourage new institutions to do new tasks?

We should ask whether it makes sense in the new century to fund research infrastructure and training organized around Population Research Centers, or would it make more sense to fund Family Research Centers, Social Epidemiology Centers, and Centers for Environmental Problems, Urban Problems, and Rural Problems? Demographers would be valued members of each of these, and they would still have their own meetings and journals. But they need not define the centers. One could imagine demographers having a status similar to that of geographers in North American universities -- a ubiquitous discipline, owners of some valued techniques, but typically working without top billing.

One enduring role for PRCs is as centers for design, implementation, documentation and archiving, and locus of "institutional memory," for complex data projects, which, we have argued, are increasingly important to the field. In different ways, RAND performs this function for the Family Life Surveys; the University of Wisconsin for the National Survey on Families and Households, and the University of North Carolina for the National Study of Adolescent Health. This role is a traditional strength of demographers.

PRCs are also likely to keep a crucial role in training. There is no good substitute yet for face-to-face communication in training, and it will always be important to have institutions with a critical mass of people at several stages of research careers working on related topics, where new researchers can serve apprenticeships.

But there are disadvantages to size and specialization, as well, which would argue for supporting a portfolio of centers of different sizes, and perhaps some research networks transcending single institutions. We have argued that new alliances across disciplines are likely to produce advances on the topics discussed in this report. Often people with a talent and interest in this sort of work teach at liberal arts colleges or universities without PRCs, where a lesser degree of specialization is encouraged. These people might be valuable members of research networks, even if their institutions are not likely candidates to be major PRCs.

There have always been successful research collaborations across institutional boundaries, and some of these are large and enduring enough to be prototypes for “virtual PRCs” in the Internet era. Conversely, there are existing centers where the web of collaborations is not particularly dense. Individual researchers may derive more benefit from their collaborations with like-minded colleagues in other buildings or other institutions than with those whose offices are down the hall; the “Center” for such researchers has an administrative but not necessarily much of an intellectual significance.

The Internet and cheap air travel should have some effect on the structure of costs and the optimal scale of organization of population research. They might make it easier and more effective to keep together networks defined by common interests, and use of the same data sets, rather than common places of employment.

This might be a particular advantage in overcoming the brain drain problem for developing countries. Researchers from developing countries have often found that they are isolated when they return from training at overseas institutions. The infrastructure for research -- libraries, skilled assistants, release from teaching duties -- is insufficient for staying on the cutting edge or making a contribution to new problems. The only way to maintain one’s skills and contacts, and a decent income, is to devote oneself to consulting for international agencies or to take a job in another country. Several of the foreign aid donor agencies have lost interest in funding long-term training. Some of the problems, though not all, could be alleviated if researchers based in developing countries could stay active as members of networks -- able to keep up with new methods through distance learning courses, able to interact regularly with colleagues at small meetings, able to take advantage of libraries and programmers physically far distant over the Internet. It would be useful to have a review and some experimentation to see how well such research networks can function in the coming decade -- what is the right size, frequency of face-to-face interaction, and breadth of specialization to keep them productive.

The Internet is already changing the division of labor in population studies, blurring the boundaries between data collection and analysis and research dissemination. One example is unfolding in the reporting of the 2000 Census in the United States. The

Census Bureau posted the so-called “redistricting data” on the Internet, state by state, as each was ready, during February and March 2001, along with news releases, summary tables, and some text explaining trends and procedures. In the past, the Bureau staff would produce detailed tables and publish them in multiple volumes for several years after the Census date. Analysts, mostly based in universities, produced articles and books, which went through academic quality-control procedures and emerged in print, again with a lapse of a few years. For Census 2000, the process seems more compressed. The Census Bureau posted data sets on the Internet, in successive waves, state by state, and analysis has been decentralized and rapid. For example, on the SUNY-Albany website ([www.albany.edu/mumford/census/](http://www.albany.edu/mumford/census/)), John Logan and colleagues have presented almost in real time the gradually unfolding story of how racial residential segregation had changed in different regions during the 1990s.

These developments hold some promise for virtual research communities -- advance on the older model of attachment to a discipline at the time of first training, and to one institution. They might facilitate the connectedness of researchers in developing countries, and also lower the barriers between disciplines that university architecture and organization perpetuate. It is not that the government funding agencies cannot foster such communities. The NICHD expressed interest in a recent request for applications in novel types of “virtual center,” the NIA supports some activities that transcend the individual centers; and NICHD staff have organized research networks. But the processes for reviewing applications may not be conducive to such ventures, at least not in their early stages. Virtual research centers may be an area of comparative advantage for foundations interested in fostering population research.

## **Conclusion**

The population sciences have managed to thrive despite ambiguity, with multiple sources of funding not clearly demarcated by topical interest. The grantees are ingenious, and can put together useful programs of teaching and research with overlapping grants from funding agencies with diverse missions. A certain degree of “crowding out” might be seen – but foundations should exploit their freedom and leave the government agencies to worry about the marginal impact of their funding.

The great advantage of foundations is that they can be less risk-averse; there are fewer forces acting on them to favor safe science and big science. A good portfolio ought to have a few risky ventures in it. If evaluation later shows that everything achieved was planned, and this happens year in and year out, then the foundation is probably being too cautious. This argues for foundations to lead the way in experimenting with new organizational forms for research.

The foundations may have a comparative advantage over the government research funders in their ability to deal with problems or projects that cross international boundaries. It is not that the National Institutes of Health or the National Science Foundation cannot do this; they have funded many productive international research projects and participate in consortia of various sorts, and the Fogarty Center at NIH exists

to foster exchanges and collaborations. But again, the review system set up to ensure fairness and high quality may have the unintended effect of discouraging innovation, thus creating an opportunity for foundations that fund population research.

### **Acknowledgments**

This project may seem an unlikely one for someone who has not been active in research for some years and who has never been a foundation program officer. My excuse for this temerity is that I have been working for the last decade or so on the border between

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