
POPULATION

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REVIEW

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Testing Evolutionary Hypotheses with Demographic Data

ALICE L. CLARKE

BOBBI S. LOW

INTEREST IS GROWING in the application of evolutionary and behavioral ecological theory to problems of human lifetimes—age-specific fertility and mortality, population growth, and emerging population–environment interactions (e.g., Cronk 1991a; Borgerhoff Mulder 1991, 1992, 1998; Hrdy 1999; Low 2000a,b). An understanding of human evolution, particularly the effects of environmental constraints on age-specific fertility and mortality (e.g., Stearns 1992; Roff 1992; Kaplan 1997; Low 1998), offers insights not only into our past, but into modern problems that are both large scale and urgent. Past theories have helped little to illuminate such issues as the transition to small family size (on this see, e.g., Schofield and Coleman 1986).

Low, Clarke, and Lockridge (1992) and Low (1993) highlighted the theoretical connections between evolutionary studies of human demography and modern problems; later Wilson (1999) suggested the utility of an evolutionary approach to demographic data, especially historical data—but was apparently unaware of a wealth of existing studies. Here we review evolutionary demographic studies of traditional, historical, and modern societies. For reasons that will become clear, we emphasize historical studies.

An outstanding tradition of evolutionary approaches to the demography of traditional societies began with the work of Chagnon (1979, 1982, 1988, 1997, 2000) and Irons (1979a,b, 1980, 1983a,b, 1997) (see Table 1 for some of the more prominent examples). Empirical studies of the demography of traditional people (e.g., Howell 1979; Lee 1979; and the exemplary Hill and Hurtado 1996) are valuable in constructing hypotheses about past selective pressures: they help us understand the vital theoretical relationships between, for example, resource richness and predictability, on one hand, and age at first reproduction, fecundability, and total lifetime fertility, on the other. Such studies are always small scale and particular in

TABLE 1 Demographic research on traditional societies using an evolutionary framework; most of these have sufficient demographic data to allow further analysis

Society	Research group	Research question
Ache	Hill and Hurtado, Kaplan	Demography; resources, fertility, and mortality; status and reproductive success; infanticide
Ayoreo	Bugos and McCarthy	Social support and infanticide
Datoga	Sellen, Borgerhoff Mulder, Sieff	Fertility and offspring quality; manipulation of rules for individual and familial gain
Dogon	Strassmann	Child mortality and marriage system; sexual conflict over reproductive rules
Gabbara	Mace	Optimal fertility and inheritance for lineage
Hadza	Hawkes, O'Connell, and Blurton Jones	Optimal birth spacing; grandmothing; manipulation of rules for family benefit
Ifaluk Islanders	Betzig, Turke	Status and family success; effects of sex-specific birth order
Innuits	E. A. Smith	Optimal foraging; conflicts of interest
Kipsigis	Borgerhoff Mulder	Reproductive value and bride price; status and reproductive success
!Kung	Blurton Jones	Optimal birth spacing
Mukogodo	Cronk	Sex-biased investment; status and reproductive success
Toda	Hughes	Status and lineage success
Turkmen	Irons	Wealth, status, and reproductive success; social cues of success
Yanomamö	Chagnon	Status, aggression, and reproductive success; manipulation of rules for reproductive gain

focus, however. Optimization models of the ecological demography of traditional societies also can be informative (Mace 1996, 1998; Blurton Jones 1986, 1997; Sellen, Borgerhoff Mulder, and Sieff 2000; Luttbeg, Borgerhoff Mulder, and Mangel 2000). They help us understand our starting point, as it were, but can be difficult to relate to modern populations. In contrast, modern demographic and behavioral ecological studies, while they may have large-scale databases, focus on humans in environments that are both complex and novel;¹ further, many have only aggregate data, a shortcoming that hampers many analyses (Low 2000b). Some studies of modern populations (e.g., Daly and Wilson 1984, 1985, 1987, 1988, 1997; Kaplan and Lancaster 2000; Kaplan et al. 1995) link our evolutionary past and our ecological demographic present, but for many questions elucidating such links may be difficult.

Historical demographic data offer particular value (see especially Voland 2000). Many historical demographic datasets are large, and—importantly—based

on individual and lineage records rather than aggregate data, so that analyses can be done of variation within populations (see Low 2000b; also Voland 1995, 1998; Low 1991, 1993; Low, Clarke, and Lockridge 1992). The conditions of the population under analysis vary but are less “novel” than those of modern populations (see Table 2 for examples of historical studies). These data, as Low, Clarke, and Lockridge (1992) and Wilson (1999) suggested, act as a bridge between limited data on traditional societies living in often-remote conditions and modern populations living under novel and complex conditions. Historical data allow us to examine populations in complex and relatively modern conditions. They are typically more homogeneous than large modern datasets, and more accessible analytically; underlying relationships (still likely to function today) are more easily discerned. They are thus ideal for asking a variety of

TABLE 2 Historical demographic studies using an evolutionary framework; most of these have sufficient demographic data to allow further analysis (see Voland 2000 for a related table on family reconstruction studies)

Country/population	Time period	Research group and questions
England	1550–1900	Scott, Duncan, and Duncan, <i>Epidemiology; ecological influences on fertility</i>
	1754–72	Hughes, <i>Resources, status, and reproduction</i>
Finland		
Saami	1700–1900	Käär, Jokela, Merilä, Helle, Kojola, <i>Sex differences in remarriage patterns</i>
Finnish	1752–1850	Lummaa, Haukioja, Lemmetyinen, Pikkola, <i>Twinning; sex ratio variation</i>
France	1730s	Hrdy, <i>Infanticide</i>
Germany		
Ostfriesland	1720–1874	Voland, Siegelkow, Engel, Dunbar, Klindworth, Gabler, <i>Resources, status, and reproduction; elite status and differential investment</i>
Ireland	1800–1966	Strassmann, Clarke, <i>Resource constraints and emigration</i>
Norway	1700–1900	Røskoft, Wara, Viken, <i>Resources, status, and reproduction</i>
Portugal	1380–1580	Boone, <i>Resources, status, and reproduction; elite status and differential investment</i>
Rome	From 27 BC	Betzig, <i>Wealth, power, and polygyny</i>
Sweden	1700–1900	Clarke, Low, <i>Resources, status, landownership, and reproduction; resources, status, and emigration; evolutionary perspectives on demographic phenomena; life course analysis</i>
United States		
New England	1750–1850	Towner, <i>Resource constraints and emigration; inheritance and differential investment</i>
California	1890–1984	Judge, Hrdy, <i>Inheritance and differential investment</i>
Utah, Mormons	1800s	Josephson, <i>Resources, status, and reproduction</i>

questions of interest to demographers, anthropologists, sociologists, ecologists, and evolutionary biologists (e.g., Low, Clarke, and Lockridge 1992; Low 1993; Voland 1995, 2000). Considerable work has accumulated, and the results are instructive for inference. Although the analyses extend across traditional disciplines, they are difficult for scholars in other fields to find. A new review seems both timely and useful.

Different fields, different questions

Demographers and evolutionary scholars alike are interested in life histories, although the particular questions addressed and the methods of analysis often differ. For most demographic analyses, proximate influences on fertility, migration, and mortality are of primary interest: under what conditions (social or ecological) do these shift? Evolutionary anthropologists (Hill and Hurtado 1996; Hrdy 1999), behavioral ecologists (Alcock 1998; Low 2000a), and evolutionary psychologists (Daly and Wilson 1988; Barkow, Cosmides, and Tooby 1992) more often focus on ultimate causality: what were/are the impacts of particular behaviors on reproductive and lineage success, under particular social or ecological conditions? Thus, evolutionary scholars would never ask, as do Schoen et al. (1997), why Americans would want children; genetic and lineage success are basic currencies (see Turke 1989, 1990). Finally, demographers and evolutionary analysts aggregate and disaggregate information differently, which can make comparison difficult (Low 2000b).

There are differences in emphasis (see Smith 2000), even within fields focusing on ultimate causes: What are the fitness effects of different strategies in particular environments? One could ask about past history (or present remnants of that history in modern traditional societies), or about current reproductive utility. Both are informative about ultimate causation (Holekamp and Sherman 1989; Sherman and Reeve 1997). Finally, some scholars identify problems as within the domain of “evolutionary” demography (Kaplan et al. 1995); others as within the domain of “ecological” demography (Low, Clarke, and Lockridge 1992). The first emphasizes that all populations follow the same basic rules; the second reminds us that under the same rules we will see different outcomes for populations experiencing different conditions. Both inform us about why humans live their lives as they do, in traditional, transitional, and modern populations.

Evolutionary and ecological questions addressed with demographic data

From the pioneering work of Williams (1966, 1992), Wilson (1975, 1978), and Alexander (1979, 1987, 1988), the number of publications on evolu-

tionary ecological aspects of human lifetimes and behavior has grown exponentially. The range of topics below is striking.

Marriage and fertility

Demographers and evolutionary ecologists, working with traditional and historical societies, are well aware that marriage rates and fertility patterns tend to show correlations with resource fluctuations (e.g., Wrigley and Schofield 1981; Hill and Hurtado 1996; Low and Clarke 1993). However, evolutionary scholars also focus on within-population impacts of resource variations.

In most societies that have been studied, whether traditional or historical, men use resources—wealth or status—to gain reproductive advantage. In traditional societies, they do so typically through polygyny: additional wives. In polygynous societies men's ability to marry and to reproduce successfully varies, sometimes enormously, so great expenditure and great risk taking may be profitable (e.g., Josephson 1993). This is hardly a specifically human pattern: in all nonhuman species, striving over resources is widespread and occurs because better-endowed individuals survive better, and reproduce more, than less well-endowed individuals (e.g., Krebs and Davies 1997; Low 2000a: ch. 4). Demographic transitions are actually complex shifts in these basic behaviors.

In more than one hundred well-studied societies, clear formal reproductive rewards for men are associated with status: high-ranking men have the right to more wives. They have significantly more children than other men (e.g., Hill 1984; Low 2000a: ch. 4). Many other societies have no such formal rules (such as "men of status X may have 2 wives"), but wealthy men are nonetheless more likely to marry and to have more wives than poorer men. Among the Iranian Turkmen, richer men have more wives and more children than poorer men; among the African Kipsigis, richer men marry younger wives (of higher reproductive value) and produce more children than poorer men. On the Pacific island of Ifaluk, men who hold political power have more wives and more children than others. The status-reproductive success pattern holds not only in these societies, but in others as diverse as the Meru of Kenya, the east African pastoralist Mukogodo (Cronk 1991b), the agricultural Hausa, the Trinidadians, and the Micronesian islanders (for a review see Low 2000a: ch. 4).

Even in societies in which few physical resources are owned, such as the Yanomamö and Ache of South America and the !Kung of the Kalahari in southern Africa, male striving results in male status, effective in marital negotiations. Among the Yanomamö, coalitions of related men are important (Chagnon 1979, 1988). So male kin available for coalitions represent a resource, and men manipulate kinship terms to maximize their affiliations

with powerful men (this can be a rather general pattern: e.g., Hughes 1988). Further, men can only marry women in lineages that have a particular relationship to their own, so men try to “redefine” their standing in ways that make more women available for mates (e.g., Chagnon 2000). Among the Ache, good hunters have more children than other men (Hill and Hurtado 1996). In quite varied societies, wealth or status and reproductive success are positively correlated for men.

In historical societies, too, marked wealth or status differentials within a population are frequently accompanied by marked fertility differentials. Typically, elite men—landowners or wealthy men—out-reproduced others (e.g., in Krummhörn, Germany: Volland, Siegelkow, and Engel 1991; Volland et al. 1997; Klindworth and Volland 1995; Norway: Røskoft, Wara, and Viken 1992; and Sweden: Low 1989, 1990, 1994; Low and Clarke 1991, 1993). Landowners married at a far higher rate than other men. In the Swedish data, for example, over 90 percent of landowning men in Tuna parish married, compared with approximately 30 percent of non-landowners. Landowners and wealthy men did not themselves marry earlier; rather they married women about 2.5 years younger than the wives of other men. Within parishes, women experienced similar interbirth intervals and age at last birth. Daughters survived better than sons, but there were no class differences in survivorship. The net result was that wealthy men averaged about 1.5 more children than others.² Of course, local complexities were of great interest; in Sweden, for example, the parishes included agricultural, mining, and fishing occupations, as well as external ecological shifts that affected everyone (although they affected the poor more severely than the wealthy).

Age-specific fertility

For other species, the principal predictor of optimal age at first reproduction is extrinsic adult mortality: when adult life is risky, fertility is early (Stearns 1992; Roff 1992; Charnov 1991). There are some clues (e.g., Daly and Wilson 1997) that despite human complexity, we may find similar patterns when we look locally at age-specific life expectancy. A second obvious factor in optimal age at first reproduction is the tradeoff between investment in self versus investment in offspring: having children too early reduces a mother’s ability to care for her offspring and to maintain her own condition. This results in the widely observed adolescent subfecundity of traditional societies, in which women typically have their first child at about age 18 or 19 (Lancaster 1986; Hill and Hurtado 1996); in such societies, people are keenly aware that having children too young is harmful (e.g., Hill and Hurtado 1996). There is clear evidence in both traditional and historical societies that resource bottlenecks which produce poor nutrition reduce fertility and increase mortality (e.g., Hill and Hurtado 1996; Scott and Duncan 1999).

In historical populations, marriage conventions typically delayed fertility for at least some women. Access to resources mattered—both for the population as a whole and for an individual relative to others. Marriage rates and age-specific fertility rates in a number of historical populations responded to drought and famine and resulting swings in crop prices (e.g., Wrigley and Schofield 1981; Low 1989; Low and Clarke 1993). Within societies, it is common to find that daughters of wealthier men or landowners married and began having children earlier than daughters of poorer families, whether or not sons did (e.g., Low 1989, 1990; Low and Clarke 1991; 1992; Røskaft, Wara, and Viken 1992; see Low 2000a for a more complete review).

Modern populations exhibit greater complexity. First, certain tradeoffs may no longer exist: nutritional status in some modern developed countries is sufficient to circumvent the lifetime reproductive “penalty” for very early child-bearing. In other species and in traditional societies (Roff 1992; Stearns 1992; Low 1998; Hill and Hurtado 1996), plots of age at first reproduction against intrinsic rate of natural increase show a clear peak: reproducing for the first time too early or too late means one leaves fewer descendants than others. This pattern is not evident in the United States today, however. The other main factor, life expectancy for adults, still matters. Geronimus and colleagues (Geronimus 1996a,b; Geronimus et al. 1996; Geronimus, Bound, and Waidmann 1999) found early reproduction among poor urban black women in the United States to be positively correlated with low life expectancy. In a study of ten Chicago neighborhoods of similar socioeconomic status but differing life expectancy, Daly and Wilson (1997) found that age-specific fertility was earliest for women in the neighborhoods with the shortest life expectancies at birth, and latest for women in neighborhoods with the longest life expectancies. (For a recent review of issues related to age-specific fertility see Ellison 2001.)

Twinning

Producing twins would seem to be an obvious means to increase the number of children a woman has during her reproductive life. If so, why are twins so rare? Haukioja, Lemmetyinen, and Pikkola (1989) found, in pre-industrial Finland, that producing twins did not improve the lifetime reproductive output for women because of high twin and maternal mortality. Anderson (1990) suggested that twinning has not been selected for directly, but rather is the byproduct of the multiple ova that produce twins. He speculated that multiple ova compensate for early embryo losses and increase the probability of survival of at least one zygote. The observed benefit would be shorter inter-birth intervals among women prone to producing multiple ova. Survival of more than one ovum would be an error condition and lead to higher child and maternal mortality.

Gabler and Volland (1994) found that mothers of twins had shorter inter-birth intervals and suffered increased maternal, infant, and child mortality in the eighteenth- and nineteenth-century Krummhörn population of Germany. Nonetheless, mothers of twins showed a reproductive advantage resulting not from the birth of twins but from higher fertility, more than compensating for the costs of bearing twins. The advantage continued into the second generation.

Lummaa, Haukioja, and Lemmetyinen (1999), using data from eighteenth- and nineteenth-century Finland, found no evidence of reduced birth intervals for women bearing twins. Conflicting results regarding the effect of twinning may illustrate a facultative response to eco-demographic differences between populations (Gabler and Volland 1994). Lummaa et al. (1998), comparing two preindustrial Finnish populations, found that where food was abundant and predictable, producing twins resulted in enhanced lifetime reproductive success. Where crop failures and famines were common, bearing twins reduced reproductive success. Studies across time and socio-ecological settings are needed to clarify the reproductive consequences of twinning. Given the rarity of twinning, historical data could play a critical role in providing the sample sizes and time frames needed to assess reproductive patterns.

Child survivorship

A general pattern of improved survivorship of children with increasing family wealth and status existed in nineteenth-century preindustrial societies (Hughes 1986). We found this pattern even within the relatively egalitarian populations of nineteenth-century Sweden, where both sons and daughters showed better survivorship in wealthier families (Low and Clarke 1992). Survivorship of children was, however, sometimes modified by social complexities. Periods of improvement in survivorship did not always follow a clear progression from upper to lower classes, again because of complex social interactions (Sundin and Tedebrand 1981; Nilsson and Sundin 1991). Despite variation, Low and Clarke (1992) found that wealth influenced survivorship beyond childhood as well.

Klindworth and Volland (1995) found that mortality of sons of the elite in Krummhörn, Germany exceeded that of wealthy farmers. While this finding seems to contradict the expected pattern of improved survival with increasing wealth, Klindworth and Volland explain that this anomaly is best understood as a case of local resource competition in which limiting the number of male heirs was required to minimize division of land important to lineage success. This insight is unlikely to occur to researchers not employing evolutionary logic.

Even within modern populations such as the United States, where basic health care is expected to be readily available to all citizens, child sur-

vivorship varies with socioeconomic status and education levels (e.g., Luker 1996: 109–110).

Complications in parental investment strategies

Sometimes parents terminate their investment in a child (through abortion, infanticide, abandonment, adopting-out); sometimes non-parents raise a child (through step- and foster-parenting and adoption). Evolutionary theory is of interest regarding these patterns. Parental withdrawal of investment seems at first obviously counter-selective. In nonhuman species, it is typically not parents, but reproductive competitors who harm offspring; the overwhelming majority of infanticides, for example, are committed by immigrant males, or males who do not belong to the victim's social group (see Hausfater and Hrdy 1984; van Schaik and Janson 2000; see also the review in Low 2000a).

Parents, however, do sometimes kill or abandon children, and this is of interest both from an evolutionary perspective and in terms of policy formulation. Because each infant requires great investment in order to survive and thrive, parental investment tradeoffs can be reproductively profitable in some circumstances and can result in variable investment across children—even to the extent of infanticide. Across cultures, the factors influencing abortion and infanticide include the mother's ability to invest and her other options (Hill and Low 1991), her access to additional resources (family, mate; e.g., Bugos and McCarthy 1984), the child's expected ability to succeed (Berezkei 2001; Daly and Wilson 1988; Hill and Ball 1996), and the economic and reproductive value of a woman's existing children. In polygynous societies, conflicts of interest may put children at risk (Strassmann 1997, 2000). As women age and their reproductive value declines, termination of investment in their children is less likely (Hill and Low 1991). Thus, abortion, infanticide, and neglect are more likely when circumstances reduce a mother's chance of successful investment.

Child abandonment reflects similar selective pressures. An example in which evolutionary inference is clear from studies lacking any evolutionary perspective is the fact that child abandonment in historical France (Fuchs 1984), Spain (Sherwood 1988), and Russia (Ransel 1988) was related to economic factors and mothers' abilities. Boswell's (1990) overview reveals that, despite great variation in time, country, and other circumstances, 46 percent (29 out of 63 documented cases) of abandonments were related to maternal ability to invest; when resource allocation (16 cases) and offspring quality problems (4 cases) were also considered, selective reasons were apparent in 49 out of 63 cases, or 77 percent. These results are, however, even more striking than the 77 percent figure suggests, for in the remaining 23 percent of cases there were no data explaining the abandonment circumstances beyond that fact that the child was abandoned. Therefore, all cases for which data existed were consistent with the hypothesis of optimal allocation of reproductive effort.

We most often find differential infant mortality associated with ecological and economic fluctuations (Low and Clarke 1991, 1993; Scott, Duncan, and Duncan 1995), and sometimes with social class as well (Scott and Duncan 2000; Hrdy 1992; see Hrdy 1999 for an overview). Cultural patterns can also be relevant. In Sweden during the nineteenth century, women in one far-northern parish did not breastfeed, but used cow's milk, and infant mortality was high. In the mid-1840s the Swedish government sent a doctor along with civil servants to the parish; he instituted a breast-feeding campaign, mostly among the upper class (Brändström 1984); information then filtered down to workers employed in upper-class homes.

These issues might seem local at first, but demographers and other social scientists have become increasingly concerned with them, as they affect both the population at large and the welfare of abandoned children.

Migration

Behavioral ecologists and evolutionary anthropologists are interested in why migration has evolved and how migration correlates with variance in resource availability and individual ability to access resources. Our questions are closely related to the "push versus pull" theories of migration (Grigg 1977). We are also interested in how migration may influence access to marriage partners, degree of genetic inbreeding, lifetime reproductive patterns, and subsequent population growth.

Historical demographic data are particularly useful in this case because the movements of individuals either can be reconstructed from a variety of population sources (Towner 2001) or are explicitly recorded within original records (Low 1989; Clarke and Low 1992; Voland and Dunbar 1997). A small number of studies have focused on dispersal patterns from an evolutionary perspective. While individual differences exist across the populations studied, some useful generalities emerge.

In nineteenth-century Sweden and Ireland, people were most likely to leave areas where the quantity, quality, or dependability of resources was poor (Low 1989; Clarke and Low 1992; Strassmann and Clarke 1998). Migration also varied with individual ability to access resources, and here the pattern is more complex. The migrant stream was composed of two tiers in nineteenth-century Sweden (Clarke and Low 1992) and in New England (Towner 2001). The upper classes possessed resources, skills, and trades that were in themselves mobile, allowing these people to take advantage of opportunities elsewhere, especially in comparison to the land-based resources of farmers. Farm workers, with no right to land, represented the other extreme. They often had little choice but to move in search of better working conditions (Eriksson and Rogers 1978).

Although both men and women left their place of birth, a higher proportion of women migrated in both Sweden (Clarke and Low 1992) and

New England (Towner 1999, 2001). Men tended to dominate long-distance migration (Clarke 1993a). Whereas unmarried people dominated the migrant stream in nineteenth-century Sweden (Clarke and Low 1992), the opposite was true during the same period in New England (Towner 2001). Dispersal may best be understood as a facultative, demographic response to social, ecological, and reproductive options in the local area and beyond (Clarke, Sæther, and Røskaft 1997).

Fertility and proportion of surviving children decreased with an increase in the number of lifetime moves made in the Skellefteå region of nineteenth-century Sweden (Clarke 1993a). Women who moved delayed their first birth by more than three-quarters of a year. Similarly, women movers in eighteenth- and nineteenth-century Krummhörn, Germany married about one year later than nonmovers (Volland and Dunbar 1997).

Migration patterns and rates are a growing concern in the modern world, yet migration is extremely difficult to study in modern populations since the ability to track individuals seldom exists. In this regard, historical datasets offer the rare opportunity to study correlates and consequences of migration. They remain greatly underused.

Violence, aggression, and reproductive success

Lethal conflict—both of individuals and groups during warfare—looks, at first glance, as if it should decrease reproduction. In fact, lethal conflict resembles infanticide and delayed reproduction: in specific environments, for some individuals, potentially lethal conflict is a high-stakes gamble in which there is some probability of winning (greatly increased reproduction) and of losing (death or disability) (see review in Low 2000a: ch. 13). In traditional societies, competition between males can be fierce, and competitive status of all sorts (not only winning conflicts but, e.g., accumulating resources) contributes to men's ability to marry and raise families. Because traditional societies are more likely to be polygynous (e.g., Murdock 1967, 1981), aggressively successful and wealthy men may have many more children than others (e.g., Chagnon 1988; see review in Low 2000a: ch. 4, 13, 14).

There is good evidence that the evolutionary origins of warfare lie in reproductive competition (Low 2000a: ch. 13,14). Women were the cause of warfare (as a result of abductions or failure to deliver a bride) in 45 percent of societies in one major study of 75 traditional societies (Manson and Wrangham 1991). Material resources specified as useful in obtaining a bride were causal in another 39 percent, and in about a third of these, ethnographies specified that richer men obtained more wives than poorer men. Land (clearly useful in establishing a family), livestock for brideprice, adultery, and wife stealing are major sources of conflict cross-culturally (see Low 2000a: ch. 13). In the Standard Cross-Cultural Sample,³ women were at risk of being captured in 66 of 158 societies; in the vast majority of these

cases, women were married or kept as concubines by their captors. In traditional societies, male aggression was clearly linked to reproductive gain.

In historical societies, also, male aggressiveness apparently had family lineage payoffs; military historians characterized medieval warfare as “violent housekeeping” by which groups of related men protected land and resources (Hale 1985). In warfare involving hierarchies of power (i.e., rank and specialization; probably all but tribal ambush warfare), risk has been negatively correlated with prior status and rank. Since at least the Middle Ages in Europe, disenfranchised or low-status men have been at greatest risk in war (see review in Low 2000a: ch. 14). Sons of Portuguese nobles during the fifteenth and sixteenth centuries, for example, would take three-week crusades to nearby, relatively safe locations, while sons of poor families fought in Jerusalem, often dying there (Boone 1983, 1986, 1988). Boone (1986) noted that politically powerful men apparently were aware of the problem of “excess” young males and deliberately chose to send the young men to foreign military campaigns rather than face disruption on the home front.

Today, the combination of evolutionary profit from aggressiveness and current male-biased sex ratios at birth can predispose groups to war. This, too, makes ecological and evolutionary sense (Chagnon 1988; see evidence reviewed in Low 2000a, ch. 13). Young males are societies’ most violent members, and societies with many young adult males are likely to see strife (e.g., Mesquida and Weiner 1999). In many developing countries today the age structure is young, and there is a preponderance of young men with few opportunities. Given the link between aggression and men’s reproductive success in our evolutionary past, this is an enduring demographic concern.

Additional hypotheses of interest

The topics we discussed above should be of interest to demographers of many sorts. In addition, a number of hypotheses are relevant to family historians, anthropological demographers, and other more specialized groups.

Remarriage. In virtually all societies in which remarriage occurs, it is a male affair. Men remarry more often than women, and they more often have families in second and subsequent unions than women do (e.g., Kåår et al. 1998; Low 1991; Low and Clarke 1991). In part, this relates to the difference in each sex’s mate value to the other. In our evolutionary past, and under many conditions today, men’s mate value is resource value and women’s mate value is reproductive value (Fisher 1958: the number of daughters a woman is likely to have during the rest of her life given current age-specific fertility and mortality schedules). Thus the evolutionary background to this pattern seems clear. Few demographers, however, have connected empirical patterns to the functional reproductive consequences.

Differential investment in sons versus daughters is tied to the relative potential reproductive gain from that investment (Trivers and Willard 1973; Charnov 1982). This phenomenon has an interesting relationship to evolutionary theory that is now widely recognized: numerous species adjust the sex ratio of their offspring (e.g., see Bourke 1997). Sex ratio adjustment is not a peculiarly human phenomenon, nor even one requiring conscious choice. Investments can be in the form of physiological investments (Voland 1989, 1990; Cronk 1991b,d, 1993, 2000), general provisioning (Abernethy and Yip 1990; Gaulin and Robbins 1991; Irons 2000), or intergenerational wealth transfer (Smith, Kish, and Crawford 1986; Low 1990; Judge and Hrdy 1992; Hrdy and Judge 1993; Judge 1995; Gaulin, McBurney, and Brakeman-Wartell 1997). In stratified societies with patrilineal inheritance, male-biased investment may be extreme (e.g., Boone 1986, 1988; Betzig 1986; see Low 2000a: 71–74 and 274). Biased investment favoring daughters occurs among peoples with low status relative to their neighbors (Bereczkei and Dunbar 1997; Cronk 1991b, d, e, 1993, 2000) whose daughters can “marry up.” Importantly, in some cases, sex biases in investment correlate with patterns of population increase for land-based groups; in expanding populations, sons are valued while in stagnant conditions they represent a cost (Voland 1995; Voland et al. 1997).

These are issues of concern today: extremes of sex-biased investment in countries such as China, India, and Korea create biased adult sex ratios, and brides become rare. Parents in some circumstances appear to respond to the sex ratio of their living children, so the sex ratio of third- and fourth-born children can be quite biased (e.g., in China the sex ratio of third births to families with two daughters is 224.9, and of third births to families with two sons is 74.1; Zeng et al. 1993; Low 2000a: ch. 10).

Illegitimacy. In most societies, marriage is a social precursor of pregnancy, whether the marriage is arranged or simple mate choice; whether it manifests itself in sleeping together in camp or results from years of courtship. Illegitimate status carries both social and reproductive costs in many societies. In nineteenth-century Sweden, illegitimacy rates varied across parishes (Gaunt 1980; Low 1990; Low, Clarke, and Lockridge 1991). In all parishes, however, illegitimate births were concentrated among women of lower socioeconomic status who had fewer means to avoid the social costs. Most births were first children of women who later married. In the Swedish data, paternity was assigned for some illegitimate cases; in all cases, fathers were lower-status men. Nonetheless, illegitimate status did not affect infant and child survivorship or adult reproduction. Illegitimate children were, however, quite likely to leave the parish before maturity (Low 1990).

Care of offspring by persons other than parents falls into two distinct categories: “helpers at the nest” and other relatives, and nonrelated caregivers

or “allomothers.” In many societies, a sibling (often an unmarried older daughter) cares for children; such nepotism exists because it enhances net reproduction of the parents sufficiently to compensate for the child’s lost reproduction (Emlen 1995, 1997; Davis and Daly 1997). In some traditional societies, women whose first child is a daughter (who will help care for siblings) have more children than others (Turke 1988). In many historical societies, either sons (Volland, Siegelkow, and Engel 1991) or daughters (Low 1991; Clarke 1993b; Hrdy 1999; Scott and Duncan 2000; Clarke 1993a) might forgo marriage to help care for siblings. Especially when resources became constricted, children might face tradeoffs between emigrating (see below) or becoming a helper (Strassmann and Clarke 1998). Grandmothers represent another important relative in child care (Hawkes, O’Connell, and Rogers 1997; Hawkes, O’Connell, and Blurton Jones 1997; Hawkes et al. 1998, 2000). This phenomenon may be related to the evolution of menopause, although Hill and Hurtado (1991) were unable to confirm this in the Ache (see Low 2000a: 112–113).

“Allomothers” (and allofathers) are quite a different story from an evolutionary point of view. In other primates, “allomothers” are often females who capture other females’ offspring and injure or kill them—a clear reproductive competitive strategy (see van Schaik and Janson 2000). Because human parenting is expensive, individuals in many societies are reluctant to invest as much in stepchildren as in own children (Stephan 1993; Anderson, Kaplan, and Lancaster 1999; Anderson, Kaplan, Lam, and Lancaster 1999), although there is considerable variation (e.g., Hewlett 1992; Anderson 2000). Children in modern societies with a stepparent are at significantly greater risk of abuse and infanticide than children living with both parents (Daly and Wilson 1988). Stepparents’ interests may conflict with those of parents; stepparents are more likely than own parents to abuse or neglect children (e.g., Daly and Wilson 1984, 1985, 1987, 1988). Although Gelles and Harrop (1991), Malkin and Lamb (1994), and Temrin, Buchmayer, and Enquist (2000) have claimed that their studies fail to replicate such a “Cinderella effect,” Daly and Wilson (1991, 2001) have identified glaring methodological failures in each of these studies.

Nonetheless, in cultures such as the Ache, in which both men and women have multiple sexual partners and marriage is casual, all men who associate with a particular woman are counted as father and invest in the child (also see Hewlett 1992). Adoption occurs in some nonhuman species and cross-culturally, and represents a phenomenon for which evolutionary theory proposes a unique rationale (e.g., Silk 1980; Dublin 1983). If rearing children is costly, under what circumstances would individuals take on the rearing of children not their own? There should be four circumstances: (1) when a close relative’s child is at risk (Silk 1980); (2) when the adopter’s status can be raised by adopting the offspring of, for example, a high-status individual (Dublin 1983); (3) when adoption of children gives access to a

mate who is alone and encumbered because of divorce, death of a spouse, or nonmarriage (Anderson 2000; Lancaster and Kaplan 2000); and (4) when proximate cues drive behavior—adopting a child to experience parenthood when pregnancy is not possible. In the first three cases, the strategy is reproductively profitable; in the fourth, it is not.

Longevity and costs of reproduction. Natural selection, by placing priority on successful reproduction, can in theory create negative effects on other life-history processes, including longevity. Although this relationship requires additional study, Lycett, Dunbar, and Volland (2000) examined the relationship using historical data from nineteenth-century Krummhörn, Germany and found that when confounding factors were controlled for (duration of marriage, time spent in fecund marriage), there is a strong negative relationship between longevity and reproduction with increasing economic deprivation.

Opportunities for the future: Merging perspectives

The evolutionary approach to human reproductive behavior has proved highly fertile in its twenty-year history (Irons and Cronk 2000; Gray 2000). Hypotheses and testable predictions from several disciplines converge with those of evolutionary and behavioral ecology in useful ways; this is certainly true in demography (including economic and historical; e.g., Easterlin 1978; Easterlin and Crimmins 1985) and economics (e.g., Becker 1981; Becker and Barro 1988; Becker and Lewis 1974). Yet the approaches remain sufficiently distinct that each can enrich others through thoughtful application.

Of course, no approach is universally effective. Dunbar, Clark, and Hurst (1995), for example, have suggested that evolutionary analysis focused on reproductive costs and benefits may be difficult when applied to historical populations, where limited data render it impossible to assess the actual fitness benefits of alternative courses of action available to an individual in any given case. This difficulty can apply to any retrospective data, modern or historical. Experimental approaches with controls are obviously not possible. Comparative methods (e.g., Harvey, Martin, and Clutton-Brock 1986; Promislow and Harvey 1990; Mace and Pagel 1997) that analyze behaviors of groups of individuals within, or across, populations who face different circumstances have been extremely useful in the biological sciences, and we see increasing use of this approach for demographic problems. These methods also address the important issue of nonindependence of samples. Phylogenetically, of course, humans represent a single species and it can be argued, therefore, that human populations are not statistically independent. Evolutionary scholars seeking to compare patterns across human societies are still struggling to assess the validity and utility of comparative methods

in dealing with this problem (Borgerhoff Mulder 2001)—a problem shared with, though perhaps not widely recognized within, the broad field of demography. Large sample sizes are required for such analyses; historical data are often better able than, for example, data on traditional societies to satisfy this requirement.

Demographers bring much-needed sophistication of analysis (e.g., family reconstitution; see Willigan and Lynch 1982; Sharpe 1990) to complex problems of reproduction. Evolutionary biologists have typically treated these in simpler ways (e.g., game theory, basic statistical analysis). Small samples in traditional societies often preclude sophisticated analysis, but historical datasets are frequently large enough. We suggest that demography courses become part of the training of evolutionary anthropologists and psychologists, and that behavioral ecologists (who typically have courses in life-history theory, a relative of demography) also take human demography courses.

Evolutionary scholars bring critical thinking to the problems of levels of selection and levels of analysis in ways that can help clarify research. For example, it is relatively common for demographers, in interpreting correlations between economic indicators and marital fertility, to infer that people choose to marry or procreate in order to help “manage” population levels (e.g., Viazzo 1989; Wrigley 1978). There are two problems with this. First, parsimony argues for the simplest explanatory hypothesis (Williams 1966, 1992), and it is certainly true that for most couples, marrying and having children in hard economic times is harmful at the individual (as well as the group) level. Second is the related fact that even in hard times, some individuals can reproduce (personally profitable, bad for the group), and we can predict the characteristics of these individuals (e.g., see Hawkes and Charnov 1988). All existing data are inconsistent with a “group benefit” assumption that haunted early demographic work as well as theorizing within evolutionary biology; here is a case in which evolutionary perspectives sharpen the focus of hypotheses.⁴ And the fact that behavioral ecologists, for example, use the comparative method across species can sometimes help in both research design and inference for complex problems in which multiple levels of analysis become important.

These facts mean that additional opportunities exist for fruitful collaboration. Although we have discussed primarily publications that have explicitly used an evolutionary framework for demographic analysis, many studies contain data that could usefully be examined from an evolutionary perspective (e.g., McNinnis 1977; Freedman and Thornton 1982; Johnson and Lean 1985; Cain 1985; Knodel 1988; Das Gupta and Bhat 1997). For example, consider how modern changes in fertility, work, and education interact. Combining demographic and evolutionary expertise may yield new insights. In post-demographic transition societies, children require great investment in education and training to become successful, establish themselves, and marry. The biological homologue of these demographic “quan-

tity versus quality" arguments (e.g., Easterlin and Crimmins 1985; Becker 1981; Becker and Lewis 1974; Tilly 1978) is called r-selection and K-selection in reference to the parameters of the logistic equation (MacArthur and Wilson 1967); the predicted result is lower fertility and greater per capita investment in offspring. Thus, the long-standing positive correlation between wealth/status and fertility might disappear.

Modern data are variable. In some cases, the wealth–fertility correlation still holds (e.g., Simon 1974; Essock-Vitale 1984), especially in rural areas. In other cases (Pérusse 1993, 1994) men's sexual access varies with wealth, but contraception means that fertility need no longer result from that sexual access. With small family sizes, men's wealth in some conditions no longer matters to net fertility (though it clearly matters to per capita investment: Kaplan et al. 1995; Kaplan and Lancaster 2000). Similarly, women today may trade reproductive value for resource value, working and delaying fertility.

The investment level required to produce successful offspring varies with environment, specifically with the threshold level of investment required for an offspring's success. Required investment correlates with the competition offspring face, whether this is simply a matter of population density, as in many nonhuman cases (MacArthur and Wilson 1967), of education (Knodel and Wongsith 1991), or of labor market opportunities (Kaplan et al. 1995; see reviews by Low, Clarke, and Lockridge 1992; Low 1993, 2000a: ch. 15, 2000b). But making the investments required by such driving forces does not guarantee the success of the chosen reproductive strategy. Relatively wealthy women who have late and low fertility do not necessarily experience greater net lineage increase or persistence.

In the life histories of other species, we see delayed maturation and late fertility only when these yield a net lineage reproductive profit (e.g., Stearns 1992; Roff 1992; see Low, Simon, and Anderson in press a). The very late and low fertility of highly educated women in developed countries today is almost certainly a social phenomenon that results in down-selection of those women's lineages (Borgerhoff Mulder 1998; Low, Simon, and Anderson in press a, b). Fertility is so low, and so late, in modern Western populations that it is difficult to imagine this as increasing lineage representation or persistence, unless (a) something other than numbers of children (e.g., numbers plus resources controlled) is being maximized and (b) such a strategy could sufficiently reduce unpredictable extrinsic mortality to compensate for very low numbers and long generation time (Low, Simon, and Anderson in press a). That is, such lineages could only gain if they were so well protected from mortality that their persistence was longer than that of other lineages that produced more children, but whose children died at a higher rate. We cannot imagine gathering reliable empirical data sufficient to test this possibility fully, but some recent models may be instructive (Low, Simon, and Anderson in press a, b).

For humans, it looks increasingly as though the decline in fertility that has accompanied industrialization and shifts in education and labor markets has been driven by within-population competitiveness but has gone so far as to become suboptimal from a reproductive point of view (e.g., Pérusse 1993, 1994; Borgerhoff Mulder 1998; Low, Simon, and Anderson in press a).⁵ It is unclear whether the choices by a growing proportion of individuals to maximize physical resources, at the cost of delayed and often lowered fertility, are maladaptive in a biological sense. For the first time, perhaps, we can craft a core theoretical approach to allow us to analyze modern reproductive patterns.

Demographic and evolutionary approaches converge in many respects; they are complementary in offering a rationale for patterns at different levels, including proximate mechanisms (e.g., social forces, satisfaction) and ultimate, evolutionary “strategies” (effects of behavior on lineage success). The real utility of integrating the approaches is twofold. Some predictions do not converge or are not discussed at comparable levels (e.g., the relationship between wealth and fertility today, across versus within populations); and others are entirely unexpected (e.g., sex differences in remarriage, sex ratio selection).

Finally, evolutionary theory proposes broad hypotheses about the conditions that will prompt declines in family sizes—something clearly of interest to demographers, historians, and policymakers (Mace 2000; Low, Clarke, and Lockridge 1992; Low 1993, 2000a: ch. 13, 2000b; Kaplan and Lancaster 2000). Given the complexity of modern conditions, even when we are certain the hypothesis is well crafted, we expect different results under different ecological conditions (Bereczkei 1993; Mace 2000; Low 2000b). Teasing out the conditions and theoretical reasons for fertility shifts is clearly useful; and continued dialogue and exchange of theoretical constructs can help us deepen our understanding of today’s wide-ranging population issues.

Notes

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1 Evolutionary novelty can be a considerable problem in analyses, depending on the question. In our evolutionary past (as in those of all other species), “fitness” (roughly, genetic transmission across generations; see Dawkins 1982: ch. 10) was achieved through effective proximate mechanisms: e.g., individuals who found sex pleasurable, or, in most societies,

men who controlled more wealth or had higher status, were likely to leave more children than others, all else equal (see Low 2000a for a more thorough discussion). Modern environments are novel in evolutionary terms—in this case, because widely available effective contraception has broken the link between sexual activity and fertility. Thus Pérusse (1993, 1994) found that in a modern population of Canadian men, of those who sought multiple partners, wealthier men had more partners—but not more children, because of contraception.

2 In Sweden, the prospects faced by the sons and daughters of wealthy versus poor

men also differed. The strongest pattern was that sons of poor men were likely to leave the parish as children; if they stayed, they were unlikely to marry and were likely to die without acknowledged children. However, the very few sons of poor men who nonetheless became wealthy had the highest reproductive success of all men.

3 The Standard Cross-Cultural Sample (Murdock and White 1969) comprises 158 societies, stratified for geographic region and language group, for which ethnographies are available by qualified ethnographers resident with the society for a substantial period. This sample is used to represent the breadth of cultural diversity and to minimize potentially confounding effects (e.g., geographic location).

4 The notion that individuals will act in ways that are beneficial to the larger group can be traced to the biologist V. C. Wynne-Edwards (1962). He argued that when there was a conflict between what was good for the group and what was good for the individual, group interests would win out. From an evolutionary perspective, then, he envisioned that natural selection, operating at the individual level, would be swamped by group selection. His hypothesis has, however, gained neither logical nor empirical support. Instead, empirical

evidence suggests that any group of altruists will be quickly out-reproduced by genetically selfish individuals, cheaters in the system. Although no longer an accepted concept among evolutionary biologists, the simple group selection approach has gained wide acceptance in the social sciences. Complicating the matter further, a number of more sophisticated evolutionary arguments to explain population-level epiphenomena have emerged and these approaches have, by some authors, also been referred to by the general term "group selection." Low (2000a; ch. 9, including Table 9.1) distinguishes between interdemic selection (selection operating on isolated local populations) and its variants, culture-gene interactions resulting in the emergence of coalitions and coercion of individuals, and the Wynne-Edwards group selection and population regulation argument. To date, none of the proposals convincingly argues that group-level selection will occur commonly in nature or will exist in stable equilibrium conditions.

5 Note, however, that many studies use a variety of measures and proxies, and few recognize that we may not be defining "environments" and "populations" appropriately (see Low 2000b; Low and Clarke 1993; Low 1993; Low, Clarke, and Lockridge 1992).

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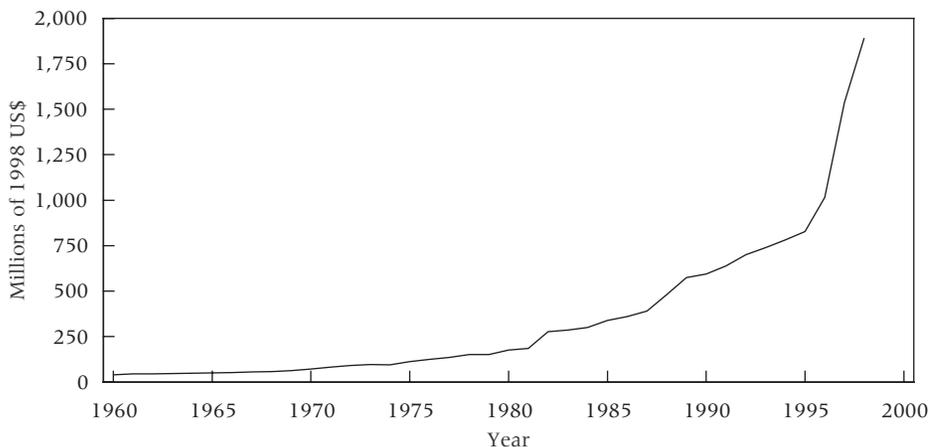
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Death at the Border: Efficacy and Unintended Consequences of US Immigration Control Policy

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WITH THE START of the 2001 fiscal year on 1 October 2000, the United States entered the eighth year of a major experiment using enhanced border enforcement to gain control over unauthorized immigration and to reduce it.¹ The experiment was made possible by two key policy shifts that began in 1993. The first was a decision by the newly installed Clinton administration to “get serious” for the first time about border enforcement. This determination took the form of a large, sustained increase in the budget of the Immigration and Naturalization Service (INS), especially the funds allocated to border enforcement (see Figure 1), which has continued under President George W. Bush. The total INS budget for the 2002 fiscal year is \$5.5 billion—more than triple what it was in 1993. The Border Patrol has more than doubled in size since 1993, with 9,212 agents on the payroll in fiscal year 2000. By the end of FY 2003, the authorized strength of the Border Patrol will be about 11,000. The INS is now the second-largest federal law enforcement agency, following the Federal Bureau of Investigation, which had 11,428 agents on duty in FY 2000.

The second key decision made by the Clinton administration was to concentrate the new resources it was providing for border control along a small number of relatively short segments of the border—the corridors that traditionally had been most heavily used by would-be illegal entrants. This strategy seems to have come into being partly because of strategic planning in the INS and other federal agencies and partly because of individual bureaucratic initiative at the local level. Early in 1993, President Clinton’s Of-

FIGURE 1 Enforcement budget of the INS, fiscal year 1960–99

SOURCE: Hanson, Scheve, Slaughter, and Spilimbergo (2002).

Office of National Drug Control Policy commissioned a study of new methods to increase border security from the Sandia National Laboratories, a federal government–supported facility devoted to research for the military. The study (Sandia National Laboratories 1993) recommended that the Border Patrol focus on preventing illegal entries, deterring them rather than trying to apprehend them at the border or in the interior of the country as clandestine “entrants without inspection.” This recommendation subsequently became the strategic underpinning of the “prevention-through-deterrence” policy that was embraced by the INS throughout the Clinton administration. The Sandia report recommended various measures to increase the difficulty of illegal entry, including the installation of multiple physical barriers, and the use of advanced electronic surveillance equipment.

At about the same time, the regional Border Patrol supervisor in El Paso, Texas, Silvestre Reyes (now a Democratic Congressman representing a district that includes El Paso) devised a new enforcement strategy for his sector. His idea was to station Border Patrol agents in closely spaced vehicles, right along the Rio Grande, and keep them there continuously, intimidating would-be illegal entrants. With only half-hearted approval from his INS superiors in Washington, Reyes implemented his strategy. It had dramatic short-term results, causing apprehensions to plummet by 76 percent in fiscal year 1994. An academic study of the El Paso initiative found that it was mainly deterring unauthorized “commuter migrants” living in the adjacent border city of Ciudad Juárez who had been commuting daily by foot to service jobs in El Paso, rather than long-distance migrants from the interior of Mexico, who continued to cross outside of the urbanized El

Paso area with the help of professional “guides” or who simply chose to enter at other, unfortified points along the border (Bean et al. 1994). Choosing not to wait for a serious evaluation or for INS apprehension statistics for subsequent years,² members of Congress, local officials, and much of the mass media touted the apparent success of the El Paso experiment, and the INS quickly found itself under great pressure to replicate it in San Diego and other “main gates” for illegal entrants.³ In sum, the El Paso experiment set off a chain of policy decisions, leading to the adoption of a strategy of “concentrated border enforcement” operations along the Mexico–US border. The strategy calls for:

—Thousands of additional Border Patrol agents, stationed in a few, narrow corridors.

—High-intensity, stadium-type lighting (both portable and stationary units).

—Ten-foot-high steel fencing, constructed by welding together Vietnam War–surplus corrugated steel landing mats, along 76 miles of the Southwest border as of May 2001.

—Permanently mounted and mobile infrared night scopes or “thermal imaging devices,” which detect migrants by their body heat and enable the Border Patrol to dispatch its agents and vehicles precisely to those places where illegal entries have been made.

—Large numbers of motion-detecting sensors, buried in the ground near the border.

—Remote video surveillance systems linked to in-ground sensors, so that as soon as a sensor is tripped a nearby video camera automatically pivots to survey the area.

—New road construction along the border, to give the Border Patrol greater access and mobility.

—A computerized system of biometric scanning, called “IDENT.” In this system, each illegal migrant who is apprehended is photographed, and his photo, fingerprints, biographical data, and the date and location of his apprehension are entered into a data base. The data base is supposed to enable the INS to detect repeat entrants, especially those who have criminal records.⁴

Some of the above-mentioned hardware and technology are old, dating back to the Vietnam War (e.g., the infrared night scopes); some parts are new (e.g., the “IDENT” system). The critical difference between the Clinton administration’s border enforcement strategy and that of previous administrations is that it concentrated the new resources being provided to the Border Patrol along just a few segments of the Mexico–US border.

This effort to fortify the “main gates” of illegal entry focused initially on the El Paso, Texas area. “Operation Hold-the-Line” (originally dubbed “Operation Blockade” until Mexican government protests prompted a re-

naming) was started there in September 1993. "Hold-the-Line" covers a 20-mile stretch of the border within the El Paso metropolitan area.

The second of the INS's concentrated border enforcement operations was "Operation Gatekeeper" in the San Diego sector. Launched in October 1994, "Gatekeeper" was implemented in three stages: In Phase I, most of the new resources were deployed on the westernmost, 14-mile segment of the border, running from the Pacific Ocean to the Otay Mesa port of entry. In Phase II, the operation was extended eastward to Tecate, into the mountains of East San Diego County. Phase III extended "Gatekeeper" across the San Diego County line into Imperial County, all the way to Yuma, Arizona. Next came "Operation Safeguard," intended to improve control along the 300 miles of international border in Arizona. Launched in 1994, "Safeguard" did not receive significant resources until 1999, when it became clear that the INS did not have enough assets in place in Arizona to stem the tide of illegal migrants who were no longer entering through California. Finally, "Operation Rio Grande" was launched in 1997 to secure the south Rio Grande Valley of Texas.

The efficacy of concentrated border enforcement

The most commonly used indicator of the efficacy of border controls in the United States is the number of apprehensions being made by the Border Patrol along the Southwest border. Throughout implementation of the concentrated border enforcement strategy, the INS has insisted that a drop in apprehensions will signify that the show of force at the border is deterring migrants from attempting to enter the United States.

Most immigration scholars regard Border Patrol apprehensions as a highly imperfect statistical proxy for the volume of illegal immigration but continue to use it, in combination with other indicators, because it provides a long time series (continuous data from 1951 to the present) and covers the entire Southwest border. Apprehensions statistics overstate the actual flow of persons because the data represent events (i.e., apprehensions) rather than persons, who may make multiple entry attempts on a single trip to the border, being apprehended several times in the process. We do not know what the inflation factor is, however, because the INS thus far has declined to release any of the data on recidivism (repeat entries by apprehended illegals) being collected through the IDENT system.⁵ On the other hand, apprehensions data understate the flow because of the large number of unauthorized migrants who manage to enter without detection. Some evidence suggests that the probability of apprehension has increased significantly since 1993 if illegal entry is attempted in one of the most heavily fortified areas. For example, in a sample of 345 unauthorized migrants interviewed in San Diego County in 1996, those who entered before January

1995 made an average of 1.42 crossing attempts before gaining entry into the United States, while those who entered during the first half of 1996 made 1.63 attempts (Cornelius 1998a: 130). By the late 1990s, however, the majority of would-be undocumented migrants and the people-smugglers who assist them had learned to avoid these heavily fortified areas. Thus, the borderwide “getaway ratio” (illegal migrants who avoid apprehension on a given trip to the border as a fraction of total migrants attempting illegal entry) may still be in the historical range of 70–80 percent, despite the post-1993 border enforcement buildup.

Apprehensions rose steadily along the Mexico–US border from FY 1994 through FY 2000 (see Table 1). The FY 2000 total surpassed the record of 1,615,844 apprehensions along the Southwest border set in fiscal year 1986, when Mexican migrants were rushing to the border in hopes of qualifying for one of the legalization programs included in the US Immigration Reform and Control Act of 1986. Altogether, apprehensions along the Southwest border rose by 68 percent during the seven-year period in which the Clinton administration’s concentrated border enforcement strategy was implemented. Some portion of this increase is attributable to the border

TABLE 1 US Border Patrol apprehensions of illegal migrants along the Southwest border, fiscal years 1994–2000

Border Patrol sector	FY 94	FY 95	FY 96	FY 97	FY 98	FY 99	FY 00	Percent change FY94–FY 00
California								
San Diego	450,152	524,231	483,815	283,889	246,092	182,248	151,681	–66
El Centro	27,654	37,317	66,873	146,210	226,580	225,290	238,126	761
Total	477,806	561,548	550,688	430,099	472,672	407,538	389,807	–18
Arizona								
Yuma	21,211	20,894	26,310	30,177	76,195	93,386	108,747	413
Tucson	139,473	227,529	305,348	272,397	387,406	470,449	616,346	342
Total	160,684	248,423	331,658	302,574	463,601	563,835	725,093	351
Texas								
El Paso	79,688	110,971	145,929	124,376	125,035	110,846	115,696	45
Marfa	13,494	11,552	13,214	12,692	14,509	14,953	13,689	1
Del Rio	50,036	76,490	121,137	113,280	131,058	156,656	157,178	214
Laredo	73,142	93,305	131,841	141,893	103,433	114,004	108,973	49
McAllen	124,251	169,101	210,553	243,793	204,257	169,115	133,243	7
Total	340,611	461,419	622,674	636,034	578,292	565,574	528,779	55
Southwest border								
Total	979,101	1,271,390	1,507,020	1,368,707	1,516,680	1,537,000	1,643,679	68

SOURCE: US Immigration and Naturalization Service.

enforcement buildup itself (each additional Border Patrol agent on the line will boost apprehension statistics). On the other hand, the increased use of professional people-smugglers in response to stiffer border enforcement has reduced the probability of apprehension, at least for migrants who make “assisted” crossings. Given these countervailing factors and absent INS data on recidivism among apprehended illegals, it is not possible to determine whether there was a real increase in the number of persons attempting to enter the United States illegally during the 1994–2000 period, net of the increase in apprehensions that can be attributed to stronger enforcement.

Fiscal year 2001 brought a decline of 25 percent in apprehensions along the Southwest border. The reasons for the decline are unclear. Border Patrol officials take it as proof that fewer illegal entries are occurring, because unauthorized migrants are (finally) being deterred by concentrated border enforcement. Other explanations are equally plausible, however. Most importantly, fewer unauthorized migrants were at risk of apprehension in FY 2001 because they stayed in the United States rather than return to Mexico for year-end holidays. Some may have been deterred from returning to Mexico by the increased cost and risk of reentry to the US (a result of the concentrated border enforcement strategy), others by concern that they might jeopardize their chances of securing legalization under the Legal Immigration Family Equity (LIFE) Act passed by Congress on 21 December 2000. Qualifying immigrants had until 30 April 2001 to file applications for legalization under LIFE and could not travel abroad while their application was pending without special permission from INS. Immigrant communities throughout the United States were also swept in 2001 by rumors of a new, broader amnesty, believed to be part of an immigration reform package being negotiated by President Bush and Mexico’s new President, Vicente Fox (see, for example, Rodríguez 2001). As was the case with the general amnesty program enacted by Congress as part of the Immigration Reform and Control Act of 1986, it was widely expected that uninterrupted residence in the United States would be a requirement for eligibility.

Other possible explanations for the 2001 decline in apprehensions include optimism about economic and social progress in Mexico under the democratically elected Fox government (although consumer confidence within Mexico actually declined during the first half of 2001—*Reforma* 2001); the US economic slowdown, which may have dampened expectations for finding employment in the United States; fears of bioterrorism and heightened border security in response to the terrorist attacks of September 11, 2001 (Smith and Ellingwood 2001b); and more undetected crossings by unauthorized migrants through legal US ports of entry. This last explanation is supported by INS data showing a recent doubling of cases of apprehension at the San Ysidro, California border crossing involving immigrants hiding in car trunks and secret compartments. There has also been an upsurge in attempted entries by persons (especially women and children) using false

documents. Waits to cross the border at San Ysidro and other busy ports of entry have lengthened significantly as INS inspectors must screen vehicles and pedestrian crossers more rigorously (Taylor 2001). Whatever the combination of factors that may have reduced Border Patrol apprehensions in FY 2001, it is too early to tell whether the decline will persist. Claims that the US border enforcement strategy is succeeding as a deterrent to new potential unauthorized migrants are premature. Moreover, total apprehensions for FY 2001 still exceed the level recorded in the pre-concentrated border enforcement era by several hundreds of thousands.

An indisputable consequence of concentrated border enforcement operations has been the spatial redistribution of illegal entry attempts. The disaggregated apprehension statistics since FY 1994 (Table 1) suggest the magnitude of this rechanneling effect. The share of apprehensions occurring in California and Texas, where most of the new Border Patrol resources have been deployed, has dropped, while the percentage of apprehensions occurring in Arizona has soared. Apprehensions along the Arizona segment of the border rose by 351 percent between 1994 and 2000. In the Tucson sector alone, over 616,000 apprehensions were made in the 2000 fiscal year—up 31 percent from 1999, up 59 percent from two years ago, and over 170 percent higher than five years ago. In contrast, the San Diego sector accounted for only 9 percent of borderwide apprehensions in the 2000 fiscal year, compared with over 40 percent before Operation Gatekeeper. In FY 2001, apprehensions in the San Diego sector reached a 25-year low. This reflects the fact that migrants and people-smugglers have shifted their routes eastward, well beyond San Diego County. Reviewing these trends, the US General Accounting Office (2001: 28) concluded: “Although illegal alien apprehensions have shifted, there is no clear indication that overall illegal entry into the United States along the Southwest border has declined.”

Other consequences of concentrated border enforcement

In addition to rechanneling the flow of illegal migrants, the current US border enforcement strategy has significantly raised the cost and physical risks associated with illegal entry. These should not be treated as “unintended” consequences, since they were an integral part of the INS’s “prevention through deterrence” strategy from its inception. Indeed, the theory underlying the strategy was that raising the cost, the physical risk, and the probability of apprehension on each entry attempt would eventually discourage the migrant and cause him (or her) to return to the location of origin. Better yet, the prospective unauthorized US-bound migrant would be deterred from leaving his home community in the first place.

Clearly, the border control strategy implemented since 1993 has raised the cost of illegal entry for a large proportion of migrants. Fees charged by

coyotes—the professional people-smugglers who guide migrants across the border, help them to avoid the Border Patrol, and transport them in trucks or vans to safe houses and eventually to places in the interior of the country where relatives or employers await them—have doubled, tripled, or even quadrupled, depending on the entry corridor and the services offered. An analysis of data collected in a sample of Mexican sending communities between 1985 and 1996 (Reyes and Johnson 2000) found that *coyote* fees had been rising for several years before the onset of concentrated border enforcement operations, but the shift to this new strategy of border enforcement reinforced the trend. The median fee had risen to \$700 by 1996. In the pre-Operation Gatekeeper era, *coyotes* charged an average of \$143 for assistance in crossing the border in the San Diego/Tijuana area; by 1995 the average fee had risen to \$490 (Cornelius 1998a: 131). The INS reported in October 1997 that smugglers' fees had doubled in many areas, in some cases increasing from \$250 to \$900 (US Immigration and Naturalization Service 1997). Near Douglas, Arizona, the typical charge to be smuggled across the border to Phoenix in 1999 was \$150; by the summer of 2000, the fee was \$800–\$1,300.⁶ By mid-2001, smugglers operating in the San Diego and El Centro sectors were charging \$1,200–\$1,500 per head.⁷

Professional people-smugglers have not yet priced themselves out of the market, however. They now have more clients because of the increased difficulty of border crossing,⁸ and they can charge considerably more for their services because of the additional risks they must take and the longer trips required to get migrants to their destinations. Migrants take more time to save up what they need to pay a *coyote*, or they borrow more money from relatives already based in the United States—the latter being the most common way of financing long-distance unauthorized Mexican migration to the United States. In this sense, the US economic boom of the 1990s increased the affordability of *coyote* services. Even without such assistance, the increase in smugglers' fees along the Mexico–US border since 1994 has not been of such a magnitude that it would deter economically rational migrants from making the trip (Andreas 2001: 117; Spener 2001: 155). Migrants with jobs waiting for them in the United States can earn enough in a short period of employment to cover the increase.

A genuinely unintended consequence of the new border enforcement strategy has been a higher rate of permanent settlement among undocumented migrants in the United States. Like the rise in *coyote* fees, this trend predates the concentrated border enforcement strategy (Cornelius 1998a: 136–139; Marcelli and Cornelius 2001: 122); but that strategy appears to have accelerated it. Multivariate analysis shows that border enforcement, as measured by the cost of *coyotes* adjusted for inflation, has a strong negative effect on the probability of returning to one's home country, at least among Mexican males. During the period of tougher border enforcement,

the probability of return has declined to levels significantly below what was experienced in earlier periods. For example, when the median *coyote* cost was \$237, 50 percent of Mexican male migrants returned to Mexico after two years in the United States; when the coyote cost was \$711, only 38 percent returned to Mexico after two years of US residence (Reyes and Johnson 2000). By making it more costly and difficult to gain entry illegally, the US government has strengthened the incentives for permanent settlement in the United States. Thus it is entirely possible that the current strategy of border enforcement is keeping more unauthorized migrants *in* the United States than it is keeping out.⁹

Another consequence of concentrated border enforcement has been a sharp increase in the number of migrants who die trying to gain entry (see Table 2). From 1994 through mid-2001, approximately 1,700 deaths were reported to the Mexican Consulates along the Southwest border. The US Border Patrol did not systematically compile statistics on migrant deaths until FY 1998. It has reported that 1,013 migrants died trying to cross the Southwest border illegally between October 1997 and 1 June 2001 (US General Accounting Office 2001: 25). As shown in Table 2, the incidence of deaths rose in tandem with the intensification of border enforcement in California, Arizona, and Texas. The available data understate the actual number of fatalities, since they reflect only migrants whose bodies were recovered by the Border Patrol and authorities on the Mexican side of the border in a given year; unknown numbers of additional bodies lie undiscovered in the mountains and deserts. Moreover, the magnitude of underreporting undoubtedly has increased, as concentrated border enforcement operations in urbanized areas of California and Texas pushed illegal border-crossers into progressively more remote areas.¹⁰

TABLE 2 Deaths among unauthorized border-crossers, total Southwest border and California, Arizona, and Texas segments of the border, by calendar year, 1994–2000

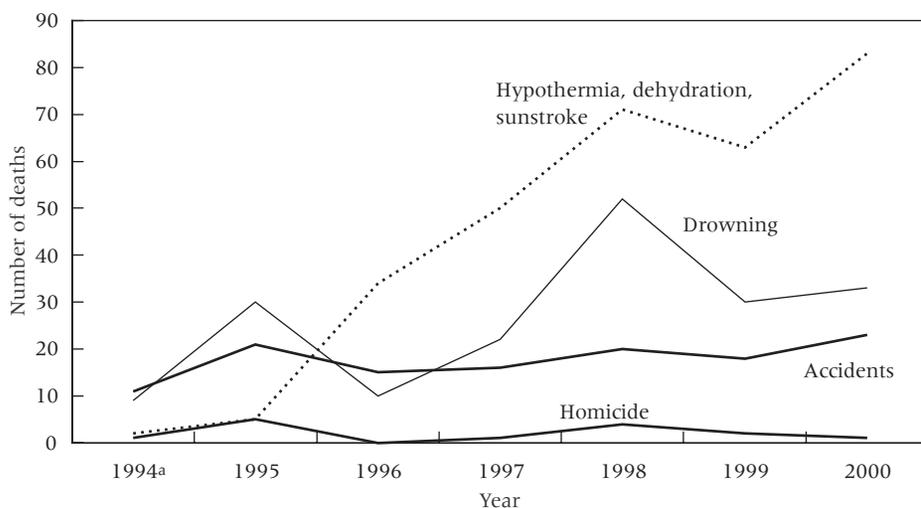
Border segment	1994	1995	1996	1997	1998	1999	2000	Percent increase
California	23	61	59	89	147	113	140	1994–2000: 509
Arizona	—	—	7	26	12	44	90	1996–2000: 1,186
Texas	—	—	21	34	170	201	269	1996–2000: 1,181
Southwest border total	—	—	87	149	329	358	499	1996–2000: 474

NOTE: Data are from the Mexican Ministry of Foreign Relations. They include deaths occurring on the Mexican side of the mountains, deserts, canals, and rivers that straddle the Southwest border. For Arizona and Texas, there are no migrant death statistics before 1996. Data drawn from vital statistics registries in US border counties (see Eschbach et al. 1999; Eschbach, Hagan, and Rodríguez 2001a), while providing a longer time series, are an inadequate substitute for the data reported in this table, since they neither identify the immigration status of the decedent nor specify whether death occurred as the result of an attempted illegal entry into the United States.

Calendar year 1994 can be used as a baseline for this analysis because none of the 23 fatalities recorded along the Mexico–California border in that year occurred after 1 October, the launching date for Operation Gatekeeper. Most of these deaths were the result of traffic accidents caused by migrants’ running across freeways immediately after crossing the border in the urbanized San Diego area and being hit by high-speed vehicles—a long-standing source of migrant deaths. Thus, the effect of the concentrated border enforcement strategy on migrant mortality can only be detected in 1995 and subsequent years. Some portion of the increase in fatalities from 1995 to 2000 can be attributed to a rising volume of unauthorized Mexico-to-US migration during that period; however, the per-year increases in mortality are much larger than the increases in Border Patrol apprehensions. And even as apprehensions, borderwide, declined by 25 percent in FY 2001, migrant deaths declined by only 13 percent. In California’s Imperial Valley (the El Centro Sector), the incidence of migrant deaths rose by 14 percent in FY 2001, despite a 27 percent decline in apprehensions in that sector.¹¹

The most convincing evidence that concentrated border enforcement is largely responsible for the rise in migrant mortality is the change in causes of death among unauthorized border-crossers that can be observed since 1994 (see Figure 2). Most migrant deaths along the Mexico–California border between 1995 and 2000 (308 of them) were the result of “environmental causes”: hypothermia (freezing to death in the mountains), dehydration, or heat stroke (after days of trudging through the desert).

FIGURE 2 Causes of death among unauthorized border-crossers, Mexico–California border, 1994–2000



NOTE: Data shown exclude unknown-cause cases.

^aNone of these deaths occurred after 1 October, the date on which Operation Gatekeeper began. See text.

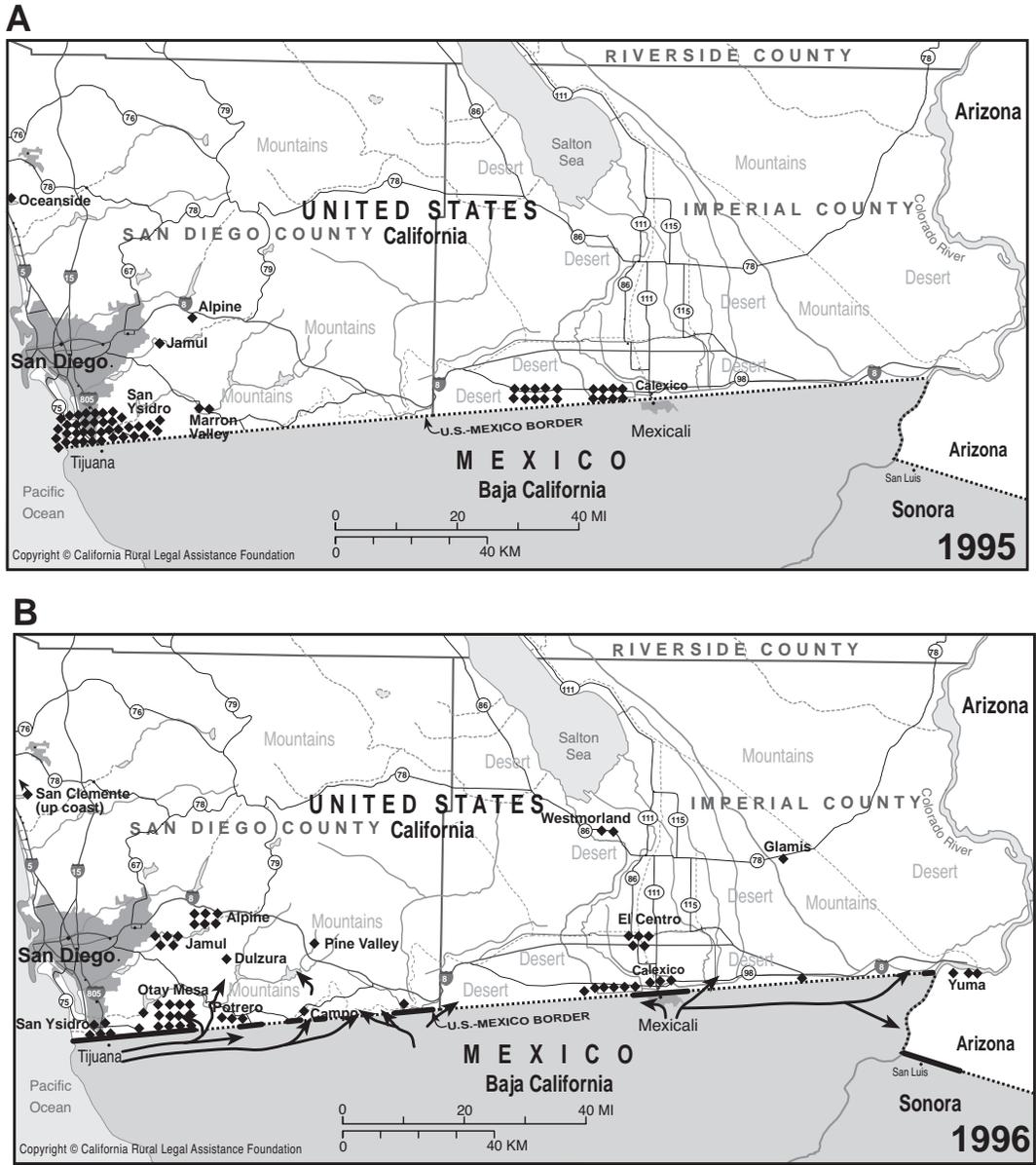
SOURCE: Data from Mexican Consulates.

Another 177 migrants died by drowning, mostly in the All American Canal, an aqueduct for agricultural irrigation that parallels the Mexico–US border for 53 miles near Calexico in Imperial County. The canal averages 21 feet deep and at some places is as wide as a football field. Its undercurrent is deceptively strong (15–20 miles per hour). Moreover, the *lancheros* (boatmen) who ferry would-be illegal entrants across the canal in rubber rafts in groups of eight or more for a fee often overload the rafts, which capsize and dump their human cargo into the canal. Others drown by attempting to float into the United States on inner tubes via the New River, which flows from south of the border into California. The New River is one of the world’s most contaminated waterways—so polluted with industrial wastes and typhoid, cholera, and hepatitis bacteria that the Border Patrol refuses to send agents into it to rescue drowning migrants. Where the New River flows under a bridge, migrants must float submerged, holding their breath, through a 30-foot-long culvert. Drowning is, in fact, the most frequent cause of death among migrants in the El Centro sector. This, too, is a direct consequence of the concentrated border enforcement strategy: migrants entering via the All American Canal and the New River are seeking to avoid the worst of a scorching, multi-day trek through the desert.

The incidence of migrant deaths along the Mexico–California border resulting from traffic accidents and homicides remained essentially stable from 1995 to 2000, while deaths from environmental factors and drowning increased sharply, together accounting for 78 percent of total deaths. Borderwide cause-of-death statistics compiled by the US Border Patrol from FY 1998 through mid-2001 show a similar pattern: 76 percent of migrant deaths for which a cause was known were attributed to heat, cold, and drowning (US General Accounting Office 2001: 25). Unquestionably, concentrated border enforcement has made illegal crossings more dangerous.

A key indicator of the success of Operation Gatekeeper in pushing would-be illegal entrants steadily eastward from the urbanized area of San Diego into the mountains and deserts is the changing spatial distribution of migrant deaths. In calendar year 1995, with Operation Gatekeeper not fully implemented, most migrant deaths were still occurring in the San Diego sector (see Panel A of Figure 3). In 1996, corresponding with Phase II of Gatekeeper, deaths dropped sharply in the heavily fortified coastal area and moved into the mountains of East San Diego County (Panel B). In 1997, as Gatekeeper’s Phase III pushed migrant traffic into the Imperial Valley, we see an upsurge of deaths in the El Centro–Calexico area (Panel C). In 1998, the death toll resulting from dehydration in the desert and drowning in the All American Canal near Calexico continued to mount (Panel D). In 1999, as the Border Patrol sent more agents to fortify the El Centro sector, the frequency of migrant deaths increased in the Yuma, Arizona area (Panel E). In 2000 (Panel F) numerous deaths were still occurring in the El Centro

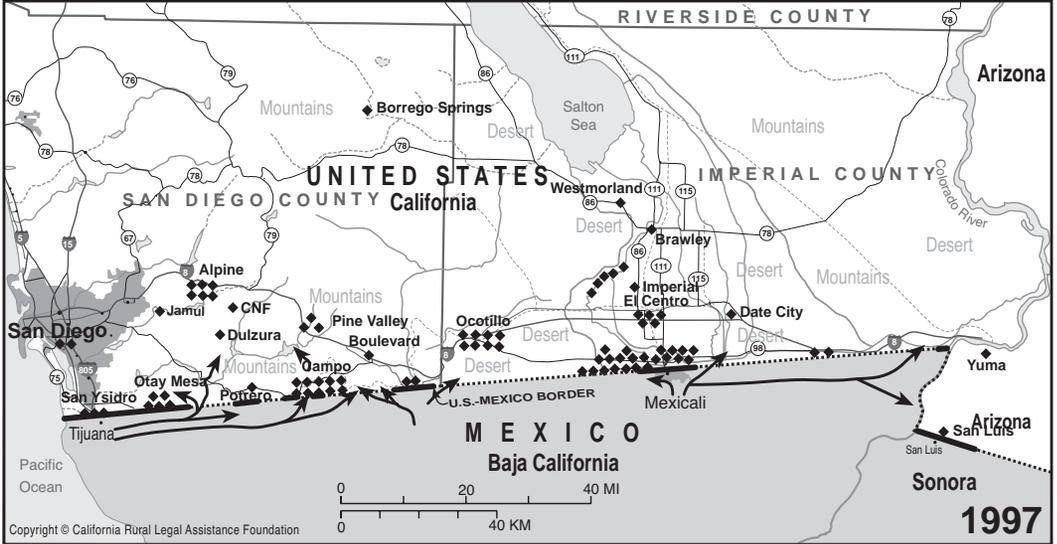
FIGURE 3 Spatial distribution of migrant deaths, Southwestern Mexico-US border, 1995-2001



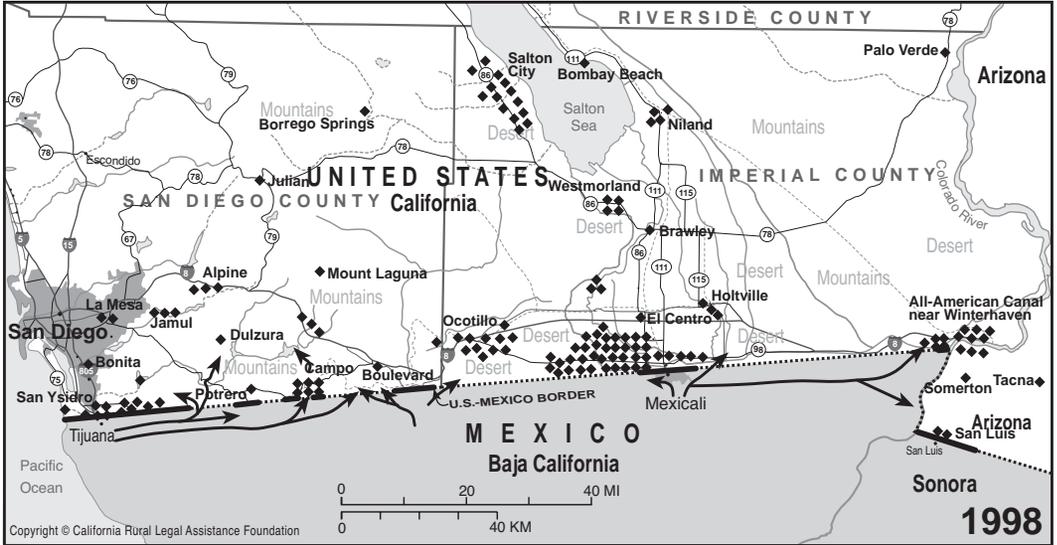
- ◆ Approximate location of a Mexican death (some Central & South American)
- Border area blocked by fencing
- ➔ Diverted migration route due to fencing, lighting, and other tactics

FIGURE 3 (continued)

C

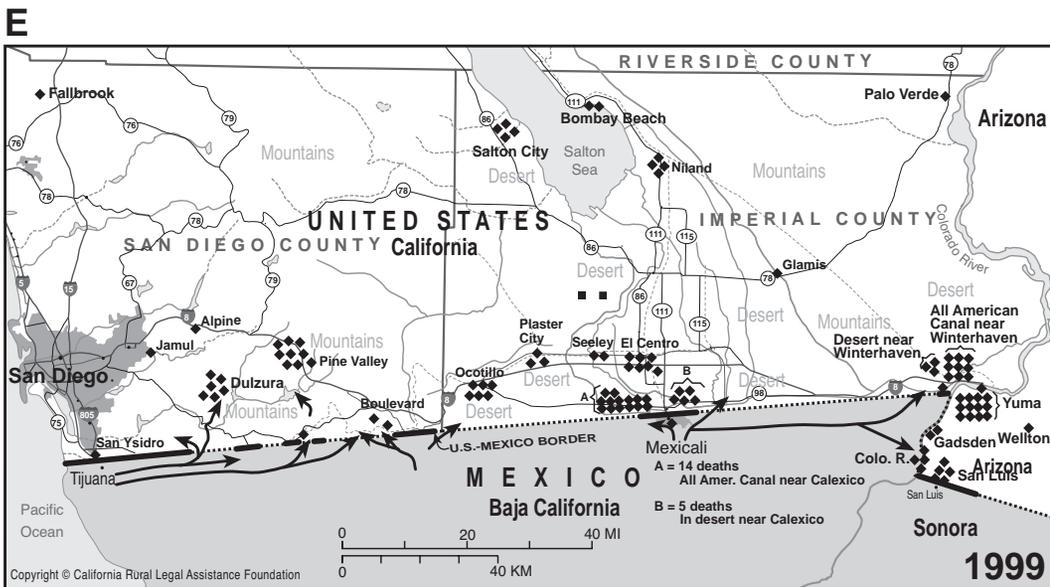


D



and Yuma sectors, but many more fatalities do not appear on the map, since they occurred in central Arizona and Texas. The redistribution of migrant fatalities into the Arizona desert is even more evident in the data from the first half of 2001 (see Panel G). Illegal entries in Arizona have shifted away from larger border towns like Nogales and Douglas, which the Border Pa-

FIGURE 3 (continued)

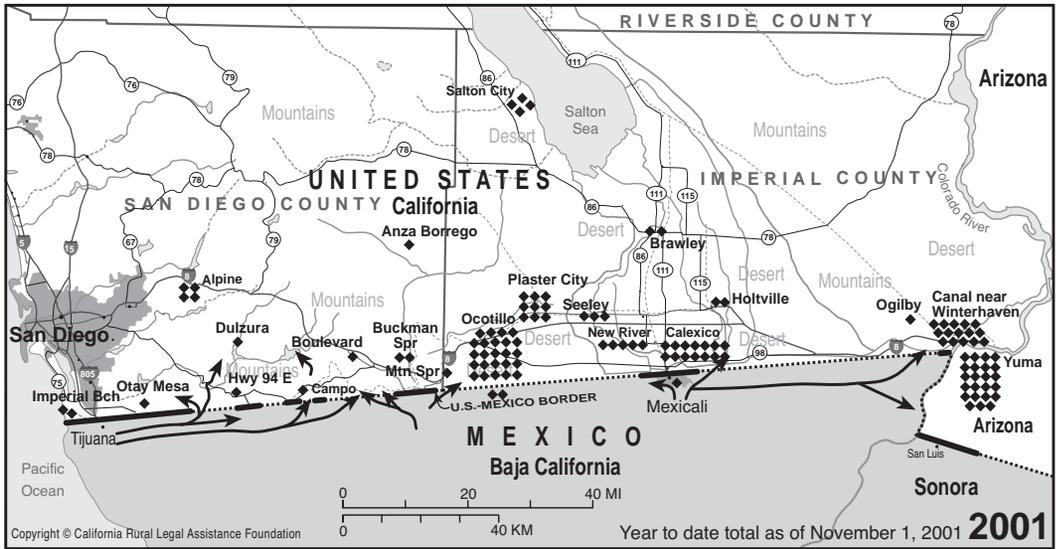


trol has fortified with reinforced steel fencing and more agents. Migrants now pass mostly through outlying rural areas, where the only obstacle to illegal entry is likely to be a few strands of barbed wire strung on three-foot-high stakes (M. Thompson 2000).

If migrants attempt to cross in such terrain, they expose themselves to life-threatening environmental conditions. For example, a hike over the

FIGURE 3 (continued)

G



- ◆ Approximate location of a Mexican death (some Central & South American)□
- Border area blocked by fencing□
- Diverted migration route due to fencing, lighting, and other tactics□

Tecate Mountains in East San Diego County can take two days. If migrants traverse this mountainous region between mid-October and mid-April, there is at least a 50 percent probability that they will encounter sub-freezing temperatures if not snow, conditions for which they are totally unprepared. Migrants entering through the Imperial Valley desert must walk a minimum of 20–30 miles before reaching a major roadway. During the summer, temperatures in this desert average 112°F and frequently reach 120°F in daytime. It is physically impossible for migrants to carry enough water to prevent dehydration during the two-day trek through the Imperial desert; indeed, many are dehydrated by the time they reach the Mexico–US border, having hiked for a day and a half from a bus stop in the Mexican interior.

The spatial redistribution of migrant deaths since 1994 is an impressive demonstration of the Border Patrol’s capacity to herd unauthorized border-crossers into increasingly inhospitable and dangerous areas. But higher physical risk has not translated into a strong deterrent effect. Doris Meissner, INS Commissioner during the Clinton administration, acknowledged: “We did believe that geography would be an ally for us. It was our sense that the number of people crossing through the Arizona desert would go down to a trickle once people realized what [it’s] like” (quoted in Borden 2000). Other INS officials told the General Accounting Office that “as the traffic shifted, they did not anticipate the sizable number that would still attempt to enter

through these harsh environments” (US General Accounting Office 2001: 24). The GAO concluded: “Although INS has realized its goal of shifting illegal alien traffic away from urban areas,...rather than being deterred from attempting illegal entry, many aliens have instead risked injury and death by trying to cross mountains, deserts, and rivers” (ibid.: 3).

Would-be illegal migrants are generally aware of the physical risks of today’s clandestine entry routes, and they clearly experience them. A survey of 262,989 apprehended migrants returned to Mexico by the US Border Patrol from December 1999 to May 2000 found that 70 percent felt they had been exposed to some type of physical risk during their illegal entry attempt. Of these migrants, 36 percent had experienced extreme heat or cold and 35 percent had suffered from lack of water or food (Santibáñez Romellón 2000). Still, they persist.¹²

Expected but unrealized labor market effects

Among the most appropriate indicators for measuring the efficacy of concentrated border enforcement are certain kinds of changes in the US labor market: fewer immigrant workers (especially the undocumented) being employed and higher average wages for workers in traditionally immigrant-dominated sectors of the economy. Thus, if the current border enforcement strategy were succeeding, we should see tighter labor markets resulting from shortages of undocumented immigrant workers in those industries where employers have come to rely on them. There should also be upward pressure on wage scales in industries and regions where undocumented immigrants have previously clustered. None of these predicted labor market effects has materialized thus far.

Data compiled by Reyes and Johnson (2000) from the survey of National Agricultural Workers conducted annually by the US Departments of Agriculture and Labor show that the percentage of undocumented immigrants among farm workers has increased continuously during the period of tighter border enforcement. The number of Mexican nationals employed in various low-level service occupations (e.g., private household workers, food preparation and food services, cleaning and building service jobs) increased far more than the number of non-Mexican workers in such jobs during the period 1993–98. There was also a sharp increase in the proportion of Mexicans among construction workers in the United States from 1993 to 1999. If the proportion of Mexico-born workers is taken as a rough proxy for illegal immigrants,¹³ these findings are inconsistent with the notion that the concentrated border enforcement strategy has been effectively deterring illegal entrants. There is no evidence that the strategy has produced shortages of illegal immigrants in agriculture, construction, low-level service occupations, or the supply of day laborers for landscaping and construction work.¹⁴

Further corroboration comes from a 1996 survey of San Diego County employers who use immigrant labor, conducted by the Center for US–Mexican Studies at the University of California–San Diego. The survey found that only 8 percent of employers had noticed any decrease in the number of immigrant jobseekers showing up at their businesses during the first 18 months of Operation Gatekeeper. In fact, more than one out of five employers had experienced an *increase* in immigrant jobseekers (Cornelius 1998a: 130).

On the wage front, several studies by economists have shown that tougher border enforcement is not benefiting workers in the form of upward pressure on wage scales, especially for farm workers and low-wage service workers (Reyes and Johnson 2000). Recent research also shows that changes in levels of border enforcement are not followed by changes in wages in US border regions, suggesting that, whatever its effect on the flow of illegal migrants, stiffer border enforcement has little impact on US wages (Hanson and Spilimbergo 1999; Hanson, Robertson, and Spilimbergo 1999).

Discussion and conclusions

To demonstrate that the current US strategy of border enforcement is reducing illegal entries by deterring them (the INS’s “prevention-through-deterrence” strategy), we would need evidence that (1) because of the new hazards they face, would-be illegal entrants are being deterred from leaving their home communities for the border; and (2) those who nevertheless proceed to the border are abandoning their efforts at illegal entry after one or more apprehensions by the US Border Patrol and returning to their place of origin. There is as yet no evidence from high-emigration communities in Mexico that appreciable numbers of potential first-time unauthorized migrants are delaying or abandoning their plans to migrate (see, for example, Cornelius 1998b).

Nor is there more than anecdotal evidence that apprehended migrants already at the border are becoming so discouraged that they are returning to their home communities. In fact, most migrants are not “giving up” after their first, second, third, fourth, or even fifth apprehension. Consistent with the behavior of several previous generations of Mexican migrants to the United States, they just keep trying to enter until they succeed. Repeatedly apprehended migrants are not being prosecuted by the INS unless they are found to have been previously deported (as a result of a formal, judicial deportation proceeding) or to have committed a serious non-immigration-related crime. The INS lacks the detention capacity and the federal courts lack the time to pursue routine cases of illegal entry recidivism. In one Border Patrol station visited by the author in July 2000, the “IDENT” system was programmed to flag only those apprehended migrants who had been caught 25 or more times since the system became operational in the mid-1990s. Even those cases were not being prosecuted except under special circumstances (e.g., a previous arrest record for migrant smuggling).

It is significant that the massive border enforcement buildup since 1993 has been paralleled by a further decline in the never-substantial US government effort to enforce immigration control laws in the workplace. Since Congress passed the Immigration Reform and Control Act of 1986, employers who “knowingly” hired immigrants unauthorized to work in the United States have been subject to substantial fines and, in the worst cases, criminal penalties. Enforcement of these “employer sanctions” has never risen above a token level, however. In 1990, barely 8 percent of INS enforcement resources were devoted to workplace raids (Juffras 1991). By 1998, only 2 percent of the INS enforcement effort was being devoted to worksite inspections (US General Accounting Office 1999a). The number of INS cases involving potential violations of the 1986 employer sanctions law declined steadily during the 1990s, from 14,311 cases in FY 1990 to 5,211 cases in FY 1996 (Center for Immigration Studies 1997: 20–21). The number of fines imposed for violations dropped from 7,115 in FY 1998 to just 178 in FY 2000. In the same period, the number of unauthorized migrant workers arrested in workplace enforcement actions dropped from 13,875 to 953 (Peterson 2001). Since then, workplace investigations have virtually ceased.

In recent years the INS has taken the position that worksite enforcement is not a cost-effective method of immigration control, given the weakness of the employer sanctions provision of the 1986 immigration law.¹⁵ But another consideration was articulated by an INS official, who explained why the agency had carried out “Operation Vanguard,” a 1999 investigation of the meatpacking industry in Nebraska and Iowa, without making any raids on plants: “We don’t want to have a negative impact on the production capabilities of these companies.”¹⁶

The unstated subtext is that most members of Congress are not concerned about the absence of workplace enforcement; indeed, many of their constituents and campaign contributors would become very upset if the INS ever became serious about worksite inspections. But actions by Congress since 1993 demonstrate that it does care a great deal about border enforcement, and secondarily about the deportation of “criminal aliens” (those who have been convicted of non-immigration-related crimes). The inevitable consequence of the Congressionally driven pattern of resource allocation for immigration control is that the typical illegal immigrant who enters without detection and finds employment within the country has an annual probability of being apprehended of between 1 and 2 percent (Espenshade 1994: 872; Espenshade 1995). As a senior INS official put it, an undocumented immigrant is at little risk in the US interior, “unless an employer turns a worker in, and employers usually do that only to break a union or prevent a strike” (quoted in Uchitelle 2000).

Meanwhile, border-crossing deaths continue to mount. The Border Patrol routinely blames people-smugglers who callously abandon migrants in

the mountains and deserts if they lag behind or run out of water, and notes that its agents rescued 2,454 migrants from almost certain death during the 2000 fiscal year as part of its "Border Safety Initiative."¹⁷ *Coyotes* have, indeed, been taking greater risks with their human cargo since concentrated border enforcement operations were implemented; but they still have a strong financial incentive to deliver migrants safely to their relatives or employers in the United States, since customarily the bulk of their fee is paid only upon arrival in the place of destination. "Migrants rely upon them because the service of a smuggler makes it more likely that they will 'arrive alive' in San Antonio, Houston, or Dallas" (Spener 2001: 155).

Mexico's President, Vicente Fox, has declared the high death toll resulting from US concentrated border enforcement operations to be "unacceptable," but his proposed alternative—a ten-year or longer process of deepening the North American Free Trade Agreement to permit free labor mobility along the lines of the European Union—has been harshly criticized by US politicians and opinion-leaders and appears to have no constituency in the Congress. George W. Bush and Al Gore both promised during their 2000 presidential campaigns that they would pursue the current strategy of border enforcement "more humanely," but neither offered specifics on how this could be accomplished. The Bush administration has reportedly promised the Fox government that it would conduct a "review" of the concentrated border enforcement strategy, as part of a larger agreement on Mexico–US migration (Smith and Ellingwood 2001). However, the domestic politics of immigration control in the United States could lock in concentrated border enforcement for the foreseeable future, particularly given the conflation of border control and anti-terrorism strategy following the attacks of September 11, 2001. The strategy has many staunch supporters in Congress, and the Bush administration remains committed to it. The President's FY 2002 budget contains an additional \$172 million and 1,206 new Border Patrol positions to bolster the INS's border enforcement strategy (US Immigration and Naturalization Service 2001).

With that strategy left intact, and strengthened, none of the policy proposals under consideration by the US and Mexican governments in 2001 for regularizing the migratory flow could be expected to reduce migrant deaths appreciably. Any temporary worker program of the magnitude that Congress is likely to approve will be able to accommodate only a small fraction of the first-time Mexican migrants who seek access to US employment in the foreseeable future, especially since the bulk of the visas would go to unauthorized migrants already working in the United States. Permanent legalization would be granted selectively to unauthorized Mexicans, primarily on the basis of their length of employment in the United States.

Meanwhile, Mexican government efforts to prevent unauthorized emigration to the United States through certain "high-risk" areas on the Mexican

side of the border—pledged to the US government as a quid pro quo for a new guestworker program (Smith and Ellingwood 2001a)—would force migrants to cross in areas posing an even higher risk. Experienced border observers expect smugglers to respond by taking migrants deeper into the desert. In short, nothing in the “grand bargain” on migration being negotiated by the US and Mexican governments would place significantly fewer unauthorized migrants at risk, and some measures would do exactly the opposite.

One must keep in mind that the rise in migrant deaths from 1994 to 2000 occurred during a period of robust economic growth in Mexico, with GDP growth in the range of 5–7 percent per year. By 2001, Mexico’s economy had been thrown into recession by an economic slowdown in the United States. There is a clear potential for much greater loss of life at the border, if a sustained downturn in Mexico’s economic performance were to compel larger numbers of economic refugees to join the flow of Mexicans migrating for reasons of economic improvement or family reunification. Forcing a substantially larger Mexico-to-US migratory flow, containing a higher proportion of economically desperate people, to detour around the concentrated border enforcement operations now in place (with others possibly to be added) would inevitably increase border-crossing deaths.

Over the longer term, Mexico’s changing demographic circumstances should do more to reduce the flow of unauthorized migrants to the United States than any US government strategy of border enforcement. By 2010, the number of new job seekers entering Mexico’s labor market could be between 500,000 and 550,000 per year, compared with 1 million or more new job seekers in the mid-1990s (Martin 2000). It would be unrealistic, however, to expect emigration pressures in Mexico to disappear by the end of the decade, unless the current real wage differential of at least 8:1 between the United States and Mexico is substantially reduced. This might happen as a consequence of the deepening of NAFTA or some combination of trade integration and successful macroeconomic policies in Mexico. In addition, the Mexican government would have to implement major new programs of targeted investment to stimulate the creation of more and better-paid jobs in high-emigration areas.¹⁸ But only when Mexico’s shrinking labor pool forces Mexican employers to raise wages are we likely to see the gap in real wages and family incomes begin to narrow appreciably.

Against this background, the mounting cost of the current US border enforcement strategy in human lives raises some fundamental issues. INS officials have estimated that it may take another 7–10 years, 3,200–5,500 more Border Patrol agents, and \$450–\$560 million in additional technology to fully implement the agency’s Southwest border control strategy (US General Accounting Office 2001: 7, 10). Even assuming Congressional approval of such continued infusions of manpower, hardware, and technology, is there reason to believe that the strategy can *ever* succeed as an ap-

proach to immigration control, absent the political will in Congress and the country as a whole to do what is necessary to strengthen enforcement of immigration laws in the workplace? Are the conceivable benefits of the strategy offset by such unintended consequences as greater permanent settlement of unauthorized migrants in the United States and a booming people-smuggling industry? If the answers to such questions are elusive, not just the efficacy but the morality of a strategy of immigration control that deliberately places people in harm's way should be debated.

Notes

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1 The most comprehensive study of the development of US border enforcement policy in the 1990s is Andreas (2000). For case studies of intensified border enforcement in California and Texas during this period, see Nevins (2002) and Spener (2000). Efforts to fortify the Mexico-US border from 1978 to 1992 are described in Dunn (1996).

2 In the second and third years of the El Paso operation, apprehensions crept upward again, reaching 38 percent of the pre-FY 1994 level by mid-1996. Over the seven-year period from FY 1994 through FY 2000, apprehensions made in the El Paso sector increased by 45 percent, although apprehensions in FY 2000 were less than half the level of FY 1993. By the late 1990s, many unauthorized migrants who before 1993 would have crossed at El Paso had been diverted to Arizona and the southern Rio Grande Valley.

3 On public pressures in support of stepped-up border enforcement in the San Diego area, see Navarro (2001) and Nevins (2000, 2001).

4 For a catalogue of resources deployed for one of the concentrated border enforcement operations, see US Immigration and Naturalization Service (1998).

5 The INS has been taken to task by the US General Accounting Office for failure to analyze the data that have been collected through the IDENT system since 1995. The GAO insists that these data "could be used to determine the number of aliens Border Patrol agents have arrested..., how many times they have been arrested trying to enter illegally, and what shifts in illegal entry attempts...have occurred over time along the Southwest border" (US General Accounting Office 2001: 29).

6 Carlos Carrillo, Assistant Chief Border Patrol Agent, Tucson sector, quoted in Thompson (2000).

7 Author's interviews with US Border Patrol officials, El Centro sector, California, 27 July 2001. Some migrants seeking work in southern California were paying an additional charge of several hundred dollars to *raiteros*, drivers who transport migrants whose *coyotes* have deposited them on the US side of the border in remote parts of Arizona or the Calexico/El Centro area of California to urban destinations in southern California, bringing migrants' total tab for *coyote* and *raitero* services to \$2,000.

8 Field research conducted in San Diego County revealed that the proportion of unauthorized migrants using the services of *coyotes* increased from 42 percent in the period before January 1995 to 52 percent among those entering during the first six months of 1996, coinciding with the implementation of Operation Gatekeeper (Cornelius 1998a: 131). Bor-

der Patrol officials interviewed by the author in the El Centro sector of California in July 2001 reported that the vast majority of illegal crossings, especially by “first-timers,” are now organized by *coyotes*.

9 This interpretation is consistent with the work of Massey (1998) and Massey and Espinosa (1997).

10 As Eschbach, Hagan, and Rodríguez (2001b: 8) have noted: “Redirection of flows from urban to rural crossing points is likely to lead also to a redirection to causes and places of death that decrease the probability that a death will be discovered and registered.”

11 Data from US Border Patrol for the 12-month period ending 10 August 2001. More than 80 migrant deaths had been recorded in the El Centro sector through the end of August 2001, with a full month left to go in FY 2001, exceeding the 76 deaths reported in all of FY 2000.

12 As *The New York Times* reported, from a vantage point on the Mexico–Arizona border, “It is clear that the increase in [the number of Border Patrol] agents along the United States side of the border has not stopped immigrants from their illegal crossings. It has caused them to cut new channels through more perilous areas” (G. Thompson 2000).

13 Recent INS estimates of the flow of undocumented migrants to the United States suggest that Mexican nationality is useful in this way. The INS has estimated that 88 percent of male migrants from Mexico and 73 percent of female Mexican migrants who entered the US in 1996 were undocumented, compared with 53 percent of males and 34 percent of females from the rest of the Western

Hemisphere, and 20 percent of males and 15 percent of female entrants from the rest of the world (Warren 2000).

14 On the highly elastic supply of migrant day laborers in southern California, see Valenzuela (2000, 2001, forthcoming).

15 Explaining the failure of employer sanctions, former INS Commissioner Doris Meissner observed: “There really is not any reliable way for employers to comply with the law” (quoted in Peterson 2001).

16 Quoted in Hagenbaugh (1999). Another explanation was offered by former INS Associate Commissioner Robert Bach: “It is just the market at work, drawing people to jobs, and the INS has chosen to concentrate its [interior enforcement efforts] on aliens who are a danger to the community” (quoted in Uchitelle 2000).

17 Data from US Border Patrol National Headquarters, Washington, DC, November 2000.

18 The US government has often paid lip service to the “developmental approach” to controlling unauthorized immigration from Mexico, but the time frame needed to create attractive alternatives to emigration in the source communities—probably 10–15 years, at minimum—exceeds what is tolerable to most US politicians. Nor is the US government likely to provide significant financial support for targeted development assistance in Mexico. As a senior US official involved in US–Mexico migration talks in the summer of 2001 put it, the Bush administration “is not in the realistic position of providing massive support to Mexico. We’re no longer in the business of Marshall Plans” (quoted in Schrader 2001).

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Women's Autonomy in India and Pakistan: The Influence of Religion and Region

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THE CULTURES OF South Asia are largely gender stratified, characterized by patrilineal descent, patrilocal residence, inheritance and succession practices that exclude women, and hierarchical relations in which the patriarch or his relatives have authority over family members. Levels and patterns of female autonomy vary considerably within the region, however, and the question is why. Two arguments have been advanced in the literature to support the hypothesis that women in Pakistan have less autonomy and control over their own lives than do women in India. The first argues that in Pakistan as in other Islamic settings, women occupy a separate and distinctive position that effectively denies them education and autonomy. Women's lack of control over their own lives has been cited as the central factor underlying the poorer mortality outcomes experienced by Islamic societies (Caldwell 1986: 175). The second argument draws on research conducted in India that demonstrates the dominant influence of behavior and norms imprinted by regionally prescribed social systems, and points out that the social systems that characterize the southern region provide women more exposure to the outside world, more voice in family life, and more freedom of movement than do the social systems of the north (Dyson and Moore 1983; Basu 1992; Jejeebhoy 2000). In this view, to which we subscribe, region plays the major conditioning role, and once region is controlled, Muslim women exert about as much autonomy in their lives as do Hindu women, wherever they reside. The argument in favor of regional social systems as opposed to religion as the driving force is strengthened by evidence suggesting wide variations in the ways in which gender and behavioral norms are manifested across a range of Islamic countries (see for example, Obermeyer 1992).

This article explores these assertions empirically, using data drawn from India and Pakistan. First, we compare the lives of women in three settings—one (Punjab) in Pakistan, the other two (Uttar Pradesh and Tamil Nadu) in north and south India respectively—and assess similarities and differences in women's autonomy. Second, we examine the extent to which levels of autonomy are in fact explained by such commonly available measures of autonomy as education and economic activity, and such traditional providers of status as age and residence patterns. And third, we explore the contextual factors underlying observed differences. In particular we try to ascertain whether differences in women's autonomy are attributable to religion, to nationality, or to the north–south cultural difference that has been widely acknowledged within India and that can be investigated with the inclusion of data from Pakistan as an additional cultural identity in the subcontinent.

Autonomy has been variously defined as “the ability...to obtain information and to use it as the basis for making decisions about one's private concerns and those of one's intimates” (Dyson and Moore 1983: 45); and “the degree of women's access to (and control over) material resources (including food, income, land, and other forms of wealth) and to social resources (including knowledge, power, and prestige) within the family, in the community, and in the society at large” (Dixon 1978: 6). We define autonomy here as the control women have over their own lives—the extent to which they have an equal voice with their husbands in matters affecting themselves and their families, control over material and other resources, access to knowledge and information, the authority to make independent decisions, freedom from constraints on physical mobility, and the ability to forge equitable power relationships within families.

Data are drawn from surveys designed explicitly to measure women's status, and they pertain to women residing in three culturally distinct sites of South Asia—(1) a rural and periurban setting in the patriarchal Punjab Province, in Pakistan; (2) an equally patriarchal setting of Uttar Pradesh in north India; and (3) the more egalitarian setting of Tamil Nadu, in the south of India. We conducted surveys in all three sites in 1993–94. This study examines the situation of women in five socio-cultural settings and groups, distinguished by region and religion (north: Uttar Pradesh Muslims and Hindus and Punjab Muslims; and south: Tamil Nadu Hindus and Muslims).

The three sites have distinct socioeconomic features. Punjab is the most populous province of Pakistan with 52 percent of the population. The state is primarily agricultural with the exception of a few large cities—Lahore, Faisalabad, and Gujranwala—in which industrial activities are concentrated. Social conditions are quite advanced in Punjab relative to other provinces. For example, literacy rates (population aged 10 and older) in Punjab were 51 percent among males and 25 percent among females, compared to 47

and 21 percent, respectively, in rural Pakistan as a whole. Also, considerable internal and external migration in Punjab has rendered the state open to wider outside influences than either rural Sind or Baluchistan. Infant mortality at 92 per thousand live births, under-five mortality at 115 per thousand, and a total fertility rate of 5.3 remain high (Hakim, Cleland, and ul Hassan Bhatti 1999).

Uttar Pradesh and Tamil Nadu lie at two extremes of the social and cultural spectrum in India, although economically they are similar. Both states are poor, with about 37 percent in Uttar Pradesh and 40 percent in Tamil Nadu living below the poverty line (compared to 33 percent in India), and both states are largely agricultural (Uttar Pradesh 72 percent, Tamil Nadu 61 percent, India 70 percent). Yet social development levels differ greatly. Literacy rates (population aged six and older) are much higher in Tamil Nadu (63 percent) than in Uttar Pradesh (42 percent), and fertility and mortality are much lower—for example, the infant mortality rate is 98 per thousand live births in Uttar Pradesh compared to 58 in Tamil Nadu; the under-five mortality rate is 141 in Uttar Pradesh and 87 in Tamil Nadu; and the total fertility rate is 5.1 in Uttar Pradesh compared to 2.2 in Tamil Nadu. Muslims experience higher total fertility rates than Hindus in Uttar Pradesh (5.3 and 4.8 respectively), but identical rates in Tamil Nadu (2.5 each) (Population Research Centre, Gandhigram Institute of Rural Health and Family Welfare Trust, and International Institute for Population Sciences 1994; Population Research Centre, Lucknow University, and International Institute for Population Sciences 1994).

The few available measures of gender disparities emphasize these regional differences in women's situation and vulnerability. For example, life expectancy at birth in Uttar Pradesh is about five years higher for males than for females (54 and 49, respectively); in Tamil Nadu, life expectancy for both females and males is 61 years. Moreover, the maternal mortality ratio is 931 per 100,000 births in Uttar Pradesh and 319 in Tamil Nadu. And gender disparities in literacy rates are far wider in Uttar Pradesh (25 percent for females compared to 56 percent for males) than in Tamil Nadu (51 percent for females compared to 74 percent for males).

Both states of India and Punjab in Pakistan are typically patriarchal and patrilocal, and the region as a whole is well known for inegalitarian gender relations. But beyond these broad generalizations, social systems and the ways in which kinship norms affect women's lives vary widely. In India, for example, there is considerable ethnographic evidence of regional differences in the situation of women, and female powerlessness is much more acute in north India than in south India (Karve 1965; Altekar 1962). Women in the north have relatively little autonomy or freedom of movement, limited inheritance rights in practice, and limited opportunities for control over economic resources. After marriage, a young woman is ex-

pected to remain largely invisible to outsiders and under the authority of her husband's family. She has little say in domestic decisions and little freedom of movement. About the only means available to enhance her prestige and even security in her husband's home is through her fertility, and particularly the number of sons she bears. The situation in Punjab is similar and well documented (Sathar and Kazi 2000). In contrast, women in south India have more autonomy in all of these respects: they have greater decisionmaking authority, are less secluded and more likely to work outside the home and control resources, and are less likely to perceive sons as their only source of prestige.

In terms of marriage patterns, all three settings are characterized by arranged marriages, patrilocal residence, and large dowries. The one big difference relates to support from the natal family after marriage. In north India, and particularly among Hindus, young girls marry into distant villages and into families with whom previous contact has been limited and subsequent contacts are usually infrequent; this practice is expected to heighten women's powerlessness. Women are perceived traditionally as temporary members in their natal homes (Dube 1988), who take resources from their natal families in the form of large dowries; even after marriage, the pattern and flow of resources is strictly one way (Das Gupta 1987). By contrast, in south India as well as Punjab, kin marriage and close natal family ties ensure that women are not cut off from family support to the same extent as they are in north India. A further distinguishing feature in Punjab, Pakistan, is the greater prevalence of exchange marriages between families. Marriages involving the exchange of female siblings reduce dowry requirements but can increase tensions between families: in cases where one couple experiences discord, the other suffers almost necessarily.

Less can be said about Hindu-Muslim differences. It has been argued that Islam restricts women's freedom to a greater extent than other religions. In India, for example, the general impression is that Muslim women are more likely than Hindu women to be denied work opportunities outside the home, a secular education, control over economic resources, recourse in case of abandonment or divorce, and choice in reproductive behavior. However, Muslim marriage patterns, at least in north India, and particularly in Pakistan, are less alienating from natal kin than those among Hindus (see, for example, Mandelbaum 1986).

The hypothesis that women exposed to the social system of the southern regions of the subcontinent have greater autonomy than women from the northern regions must be interpreted keeping in mind that autonomy levels in the subcontinent remain among the lowest in the world. Hence while women in Tamil Nadu are expected to have more autonomy than their northern counterparts in Uttar Pradesh, their autonomy is far more limited than that of women in other parts of Asia (Malaysia, the Philip-

pires, and Thailand, for example), and certainly more limited than that of men in the same settings (Mason et al. 1995).

Data

The datasets employed in this study are among the first seeking to operationalize autonomy among women from different cultural and religious settings. The main objective is to assess the relationship between women's autonomy and their reproductive behavior. Similar studies have been conducted in Malaysia, the Philippines, and Thailand (Mason et al. 1995).

In Pakistan, Punjab was purposively selected. It is the most developed province in the country in terms of agricultural productivity, road structure, sanitation, communications, and availability of health and educational facilities. Yet it is by no means homogeneous: the province houses three distinct agro-economic zones, with central and south Punjab quite different from north Punjab not only in level of development and agricultural patterns (rainfed versus irrigated agriculture) but also in terms of feudalism and consequent female autonomy (rainfed rural areas and the periurban areas are less feudalistic than the irrigated areas). In order to represent this diversity, ten communities were selected from the three agro-economic zones. Nine of these were rural and one was periurban. The ten communities are: (1) northern rainfed (barani) districts (Rawalpindi, Attock, and Chakwal) and northern semi-irrigated districts (Mianwali and Khushab); (2) central (Faisalabad and Sahiwal) and south Punjab irrigated districts (Multan and Bahawalpur); and (3) a periurban area in central Punjab (Gujranwala district).¹

In India, Uttar Pradesh in the north and Tamil Nadu in the south were also selected to represent a range of gender-related and socio-cultural conditions. Within each state, two districts were selected (on the basis of an index of development, constructed from such indicators as income, percent of roads surfaced, and other economic criteria) so as to maximize differences in socioeconomic conditions, while at the same time allowing for comparisons of Hindu and Muslim women. And from each district, one taluka (subdistrict) was selected similarly. The four sites thus selected included: from Tamil Nadu, Pollachi taluka from Coimbatore district (ranked 1 of 21) and Mudukulathur taluka from Ramnathpuram district (ranked 18 of 21); and from Uttar Pradesh, Kunda taluka from Meerut district (ranked 2 of 63) and Baghpat taluka from Pratapgarh district (ranked 51 of 63).²

From each of the four sites in India, a cluster of contiguous villages of roughly 1,000–2,000 households was randomly selected, and about 800 currently married women aged 15–39 were randomly selected for interview. Husbands who were present were also interviewed. In each setting, on the assumption that socio-cultural norms governing female autonomy vary

widely between Hindus and Muslims, half of all respondents were selected to be Hindu and the other half were selected to be Muslim. As a result, a total of eight communities are covered: four geographical sites and, within each site, two distinct religious groups, Hindus and Muslims. In one setting, Mudukulathur, in which many husbands migrate out for work and would not be available for interview, a larger Muslim sample was selected to allow for the inclusion of the required number of husbands; however, to prevent Mudukulathur Muslims from dominating the findings for all Tamilian Muslims or the average for all women from Tamil Nadu, means for these two groups are weighted to adjust for this disparity. A total of 1,842 women, aged 15–39, constituted the sample.

In Punjab, currently married women were selected from ten communities ranging in size from 2,500 to 5,000 households. In the ten villages selected, all households were listed and about 100 households were sampled randomly to interview one currently married woman in each. About half the husbands were successfully interviewed. Unlike the Indian sample almost all women in Punjab were Muslims; 1,036 currently married women aged 15–39 were interviewed along with 436 husbands.

In the course of interviews with women, respondents were asked not only about their education and their work status but also about a variety of dimensions of female autonomy, including their decisionmaking authority, their personal freedom of movement, control over economic resources, and wife–husband power relations.

For ease of analysis, we collapse the 18 communities under study into five. For India, findings are presented separately for Hindus and Muslims in each state. In Pakistan, data from the ten communities are combined into a single figure. Of course, pooling data risks obscuring important influences of patriarchy, religion, and other conditioning factors. In other articles using these data, within-region levels of female autonomy have been explored and findings are largely consistent with those reported below (see, for example, Jejeebhoy 2000, 2001; Kazi and Sathar 2001; Mason et al. 1995).

Table 1 summarizes economic characteristics of households in the five communities. Economic status appears to be similar across sites as measured by per capita income. Land ownership status is closely linked with income levels, with landlessness being most pronounced in Tamil Nadu and among Muslims in Uttar Pradesh, followed by Punjab and Hindus in Uttar Pradesh; it is, however, the Hindus of both Uttar Pradesh and Tamil Nadu, and women in Punjab who report large, that is, six or more acres of landholdings. Finally, Punjabi women are more likely than other groups to live in pucca homes (made with high-quality materials, including the roof, walls, and floor) and to have electricity and indoor toilets; they also own more consumer goods.

Economic status can also be assessed by a profile of husband's education and occupation. School attendance levels are highest among the north

TABLE 1 Household characteristics in the study sample, by site

	Punjab	Uttar Pradesh		Tamil Nadu	
		Muslim	Hindu	Muslim ^a	Hindu
Land ownership					
Percent landless	53.9	66.3	25.8	71.9	64.2
Percent owning 6+ acres	11.1	3.4	14.1	2.5	12.8
Per capita annual income (\$)	89	71	101	68	89
Housing: Percent pucca	72.1	55.3	55.7	51.3	34.9
Household amenities					
Percent with electricity	81.3	35.3	29.8	64.7	41.8
Percent with indoor toilet	20.0	16.9	12.1	11.6	10.0
Consumer durables					
No. consumer goods owned ^b	3.6	1.5	2.3	2.2	1.9
Number	1,036	421	438	547	436

^aWeighted to adjust for larger sample size of Ramnathpuram Muslims.

^bOf the following nine: any transport (car/tractor/motorcycle), clock, fan, radio, refrigerator, sewing machine, stereo, TV, iron.

Indian Hindus followed by Tamilian Muslims and are lowest among north Indian and Punjabi Muslims. On average, however, husbands' educational levels are similar in India and Pakistan, suggesting that educational attainment levels of men are sensitive neither to region nor to religion. The occupational profile of husbands, by contrast, suggests clear differences by religion in the Indian sites: Hindus are, by and large, more likely to be engaged in agriculture than are Indian Muslims, both as cultivators and, to a lesser extent, laborers. Conversely, Indian Muslims are more likely to be engaged in nonagricultural activities: as salesworkers (mostly petty), skilled workers, and unskilled laborers. In Punjab most of the population in rural areas is engaged in agriculture and a substantial proportion is engaged in wage work also.

The situation of women

The most commonly used measures of women's status are their educational and economic activity levels. Evidence that disparities are wider when the two geographic regions are compared than when either country or religion is considered emerges from the data on women's educational status in Table 2. Large proportions of women in all five communities have never been to school, and few have completed primary school; note that these data refer to women's attainment of a secular education only. Considerably larger proportions of women in Tamil Nadu have attended school than have women in either Uttar Pradesh (India) or Punjab (Pakistan). Patterns by religious

TABLE 2 Levels and patterns of educational attainment in the study sample, by site

	Punjab	Uttar Pradesh		Tamil Nadu	
		Muslim	Hindu	Muslim ^a	Hindu
Women with some education (%)	19.6	19.5	38.8	71.5	53.9
1–6 years	14.7	15.4	16.9	55.7	36.5
7+ years	4.9	4.0	21.9	15.9	17.4
Mean number of years of schooling					
All women	1.1	1.0	2.9	3.6	3.0
Women with some education	5.5	5.3	7.3	5.0	5.7
Husband's educational status (%)					
Some education	60.8	57.2	82.0	88.0	69.1
1–8 years	34.7	34.9	24.4	74.1	49.8
9+ years	26.2	22.3	57.5	13.8	19.3
Mean number of years of schooling of husbands					
All husbands	4.8	4.4	8.2	5.4	4.6
Husbands with some education	8.0	7.7	10.0	6.1	6.7
Gender disparity in education between wives and husbands (%)					
Husband (5+ years better educated)	45.2	39.7	55.9	25.0	23.2
Husband (1–4 years better educated)	11.1	11.9	20.8	35.2	28.2
Wife better educated	4.4	3.3	3.0	23.3	19.3
Wife and husband same education	39.2	45.1	20.3	16.6	29.4
Worked for wages in last 12 months (% of women)	36.1	18.0	13.2	21.4	56.4

^aWeighted to adjust for larger sample size of Ramnathpuram Muslims.

group are less consistent: while it may be tempting to draw the inference, from the findings of Table 2 with regard to women from Uttar Pradesh and Punjab, that Muslim women are more likely to be denied education than are Hindu women, a comparison with Tamilian women questions this assumption. For example, 54 percent of Tamilian Hindus compared to 72 percent of Tamilian Muslims have attended school; in the north in contrast, school attendance rates are as low as 20 percent in Punjab and among Muslims of Uttar Pradesh, and 39 percent among UP Hindus. While the typical Tamilian woman, both Hindu and Muslim, had three or more years of schooling, women from other communities have typically attended school for lesser durations: just under three years among Hindu women from Uttar Pradesh, and about one year among Muslim women from both Uttar Pradesh and Punjab. In short, while it is apparent that among northern respondents, Hindu women—and men—are considerably better educated than Muslim women, the evidence from south India weakens the argument that Islam denies women an education.

Gender disparities in educational attainment levels (Table 2) are pronounced in all settings, but once again the north–south comparison yields the widest disparities. Among over 40 percent of women in both the north Indian and Pakistani communities, educational attainment of husbands exceeds that of wives by five years or more, and the gender disparity in education is widest among Hindus from Uttar Pradesh. In comparison, among south Indian women, irrespective of religion, this proportion is about 25 percent. Interestingly, about 20 percent of south Indian women are better educated than their husbands by about one to four years, compared to a negligible 3–4 percent in Pakistan and north India.

Although women are universally involved in unpaid household work, economic independence is usually measured in terms of wage-earning economic activity. In India and Pakistan, however, where wage work for women is often unacceptable and poverty-induced, working for wages is not necessarily an indicator of autonomy. Wage-earning women are not likely to have made the decision to work on their own, nor do they always have control over their earnings. Nevertheless, even in situations of dire need, families in cultures in which women are secluded may be reluctant to allow women to work for wages outside the family farm or business. Work histories suggest that in the 12 months preceding the survey, well over half of all women were engaged in some form of work other than unpaid household work—either wage-earning activities or unpaid labor on the family farm or plantation, in the family business, or tending family livestock. Regional profiles suggest that working women in Punjab and Uttar Pradesh are largely occupied in tending animals and working on family farms; in Tamil Nadu, in contrast, agricultural labor occupies the majority of working women, followed by working on the family farm in the case of Muslims and at skilled and unskilled nonagricultural work among Hindus. Yet, comparatively few women worked for wages, and here it is largely the Tamilian Hindus who stand out compared to the others (Table 2): 56 percent of them were engaged in wage work.

Many authors have suggested other measures of women's status. Prominent among them is delayed age at marriage: women who delay marriage are observed to be more independent and have more autonomy and self-confidence than those who marry at earlier ages. Additional measures include spousal age difference (Cain, Khanam, and Nahar 1979), joint family residence, and village endogamy.

Data shown in Table 3 bear out the finding that few South Asian women have a say in their choice of husband or the timing of their marriages. However, Tamilian women are considerably more likely than women from Uttar Pradesh or Punjab to have had a say in choosing their husbands.

Although the large majority of women from all five communities resided with their in-laws after marriage, current residential patterns tell a different story. In India, women in Uttar Pradesh, both Muslim and Hindu,

TABLE 3 Family variables in the study sample, by site

	Punjab	Uttar Pradesh		Tamil Nadu	
		Muslim	Hindu	Muslim ^a	Hindu
Marital age (women aged 20–39)					
Percent married at 16 or younger	37.1	55.1	48.4	32.3	21.5
Mean age at marriage (years)	18.0	16.4	16.6	17.4	18.3
Woman had a say in marriage decision (%)	16.1	12.8	9.8	31.7	42.0
Dowry amount (in US\$)	865	488	700	938	769
Living in natal village (village of birth or childhood) (%)	49.4	16.6	5.7	45.9	34.2
Current living arrangement					
Living with mother-in-law (%)	57.0 ^b	40.1	58.9	31.9	24.5
Residence after marriage					
Coresided with in-laws (%)	89.6	87.2	93.6	69.3	75.7
Age difference between spouses					
Mean age difference (years)	5.9	5.4	4.7	6.6	6.0
Percent distribution of couples					
Wife older	6.0	0.0	0.2	0.2	0.2
Same age or husband 1–4 years older	38.2	43.7	47.7	32.8	38.5
Husband 5–9 years older	35.3	48.0	49.1	51.9	47.9
Husband 10+ years older	12.6	8.3	3.0	15.1	13.3

NOTE: US\$1=Rs 51.2 (Pakistan) and Rs. 43.3 (India).

^aWeighted to adjust for larger sample size of Ramnathpuram Muslims.

^bPercent in non-nuclear households.

are more likely than women in Tamil Nadu to currently reside with their mother-in-law. In Punjab 57 percent of respondents currently reside in a non-nuclear household. South Indian women are also significantly less likely to report difficulties in their relationships with their mothers-in-law than are Punjabi women or those from Uttar Pradesh (not shown in tabular form).

In some instances, however, the situation of Punjabi women resembles that of south Indian rather than of north Indian women. Marital age (Table 3) varies markedly among the five communities, with southern women—particularly Hindus—reporting higher ages at marriage, and women from Uttar Pradesh reporting the lowest. In Uttar Pradesh, over half of all respondents aged 20 and older are married by age 16. In contrast, about one-quarter of Tamilian women and over a third of Punjabi women are married by age 16.

Finally, village endogamy (Table 3) is considerably more likely to be practiced among Tamilian and Punjabi women than among women from Uttar Pradesh. Although Muslims in both Indian settings are more likely

than Hindus to live in their natal villages, it appears that north Indian Muslims, like north Indian Hindus, marry their daughters into distant households; they are considerably less likely to reside in their natal homes than are Muslim women from Tamil Nadu or Punjab.

The importance of dowry in securing a woman's place in her husband's home is increasingly apparent in India, both through the alarming numbers of dowry harassment and death cases reported in the media, and through studies that suggest that women whose dowries are large are less likely to have suffered domestic violence than other women (Rao and Bloch 1993). Available in our dataset is information obtained from the respondent on the size and contents of her dowry: in jewelry and gold, cash, and a variety of other property including expensive consumer goods (vehicle, refrigerator, stereo, utensils) and (rarely) land. Approximate rupee values have been imputed for each of the items in the dowry, and the total value of the dowry in rupees has been assessed. Clearly, this is an approximation, since women—particularly those from Uttar Pradesh—appear not to have been fully aware of the extent of their dowry. Results suggest that the size of the dowry is generally large in all contexts, but that Punjabi and Tamilian women report substantially higher dowries (equivalent to about US\$865 and \$854 respectively) than those from Uttar Pradesh (\$594). Again, in this respect, the situation of Punjabi women resembles that of south Indian rather than north Indian women.

For the most part, however, a comparison of the conventional measures of the status of women reported in Tables 2 and 3 confirms that differences are more likely to reflect social systems as marked by region than by nationality or religion. South Indian women appear to enjoy greater levels of autonomy than north Indian women. For the most part, the situation of Punjabi women resembles that of north Indian women—although in some respects they fall between north and south Indian women.

Measures of women's autonomy

The literature suggests several separate but interdependent components to autonomy. These include the autonomy conferred by knowledge or exposure to the outside world; decisionmaking authority, or the extent to which women have a say in family decisions and decisions concerning their own lives and well-being; physical autonomy in interacting with the outside world, or the extent to which women are free of constraints on their physical mobility; emotional autonomy, or the extent to which women enjoy close bonds with spouses and are free from the threat of violence and abuse; and economic and social autonomy and self-reliance, namely the extent to which women have access to and control over their own and their household's economic resources (see, for example, Mason 1984; Caldwell

1979; Caldwell, Reddy, and Caldwell 1982; Jejeebhoy 1995). To assess these components of female autonomy, women in this survey were asked a battery of questions concerning their status within the household. From these responses, four dimensions of autonomy were selected and indexes were created for each: (1) economic decisionmaking; (2) mobility; (3) freedom from threat from husband; and (4) access to and control over economic resources. These and similar measures have been used elsewhere by the authors and others (see, for example, Jejeebhoy 2000, 2001; Mason et al. 1995; Kishor 2000).

Economic decisionmaking authority is represented by information on the participation of women in three economic decisions, selected to capture a range of decisions, from the routine to the out of the ordinary, and also to capture the extent to which women simply participate in the decision or actually perceive themselves to have a major say in it. Items include: the purchase of food, major household goods, and jewelry. The index sums the number of these three purchases in which the woman participates, assigning a score of 1 if she only participates in the decision and 2 if she also has the major say. The index thus ranges from 0 to 6.³

The mobility index sums the number of five places—the health center, the village community center or market, home of a relative or friend, a fair, and the next village—to which the woman can go unescorted. Again, the intent was to select a range of places, both within and outside the village, both easy and more difficult to access. Hence for example, while a community center or market and friend's home were always within the village, the health center and fairs were usually outside the village. The index ranges from 0 if the woman must be escorted everywhere to 5 if she can move about unescorted to each of the five places.

The index of freedom from threat ranges from 0 to 3: 0 is assigned if the woman both fears her husband and is beaten by him; 1 if she is beaten but does not fear her husband; 2 if she fears him but is not beaten; and 3 if she neither fears nor suffers beating at the hands of her husband.⁴ Here, the intention was to capture the continuum of power relations between spouses. In focus group discussions participants agreed that women who experienced a nonphysical rebuke from their husbands were indeed more likely to assert themselves than those who feared physical reprisals, and the index was constructed to reflect this.

The index of access to and control over economic resources covers two aspects of women's use of family and own resources: their freedom to use or manage household resources and the extent to which they have independent control over any resource. The index thus sums responses to seven questions. Four of these relate to access to household resources: (1) having a say in how household income is spent; (2) getting cash to spend; (3) being free to purchase small items of jewelry; and (4) being free to purchase

gifts. Three relate to aspects of women's expression of independent control over resources: (1) whether any of the family's valuables (land/jewelry/utensils) belong to the woman (that is, are in the woman's name) and are controlled by her; (2) whether she has or had some say or the major say (assigned a value of 0.5 and 1.0 respectively) in how the valuables from her dowry are used or spent; and (3) whether she expects to support herself in old age through her own savings. The index ranges from 0 to 7.

In focus group discussions it became clear that women were acutely aware of these and other aspects of autonomy. They discussed, for example, the extent to which young women's freedom was curbed by social norms and the watchful eyes of the men and elders in the family. They pointed out that older or educated women exerted more say in their own lives than did the uneducated; they discussed the ways in which women who coresided with their parents-in-law were watched with regard to their behavior and movements. And in responding to the survey questionnaire, women were clearly able to outline the limits of their decisionmaking power.

Table 4 presents mean values for each of the four measures, as well as the distributions of the variables constituting each measure. Results confirm the limited autonomy of women in all spheres, but suggest strong regional differences. Women from both northern sites—Uttar Pradesh and Punjab—fall significantly below women from Tamil Nadu in almost every measure of autonomy, a finding that strongly supports the argument that the north-south cultural divide described earlier powerfully conditions the extent of women's autonomy. In contrast, there is far less support for the commonly held assumption that Muslim women have less autonomy than Hindu women.

Women in general have limited economic decisionmaking authority: large numbers are excluded from even the most routine decisions, and few have the major say in any decision. There is a definite pattern to the kinds of decisions in which women participate: they are far more likely to be involved in decisions that are perceived as routine in the family economy, such as those relating to food purchases, than in decisions that involve major purchases. South Indian women exhibit far more decisionmaking authority than Punjabi or north Indian women. In contrast, there is no evidence of differences in decisionmaking authority among Hindus and Muslims.

Focus group discussions reiterate regional disparities in decisionmaking authority.⁵ Women in Punjab and Uttar Pradesh are far more likely than Tamilian women to recognize and justify their exclusion from household decisions:

The husband is responsible; after him come the father-in-law and mother-in-law and then come the brothers of the husband. (Muslim, central Punjab)

TABLE 4 Indexes of women's autonomy in the study sample, by site (percent of respondents)

	Punjab	Uttar Pradesh		Tamil Nadu	
		Muslim	Hindu	Muslim ^a	Hindu
Mobility: can go unescorted to					
Friend's home	57.4	35.1	34.1	77.6	90.3
Market or community center in village	35.4	34.8	34.7	81.7	94.7
Health center	27.8	38.4	43.0	59.1	76.4
Fair	7.9	16.8	19.3	20.9	41.8
Neighboring village	11.5	20.2	22.0	15.0	33.5
<i>Index</i> (maximum value = 5)	1.40	1.36	1.47	2.00	2.88
Economic decisionmaking					
Participates in decisions					
Food	71.2	17.8	18.3	84.4	89.0
Jewelry	31.1	35.3	32.2	49.9	56.9
Major goods	16.5	8.7	12.1	43.6	49.0
Is main decisionmaker					
Food	51.1	5.7	4.8	69.6	72.4
Jewelry	6.9	5.2	4.5	9.3	11.1
Major goods	4.6	1.4	0.9	9.5	11.8
<i>Index</i> (maximum value = 6)	1.82	0.74	0.73	2.67	2.90
Access to and control over economic resources					
Has a say in household spending	59.0	80.1	75.0	88.9	93.5
Gets money to spend	70.4	77.6	72.1	90.9	94.7
Is free to purchase small jewelry	16.1	32.3	29.1	24.3	28.0
Is free to purchase gifts for relatives	23.7	13.5	14.5	19.0	17.8
Owens and controls household valuables	61.0	15.5	16.0	26.1	22.6
Plans to use own savings for future support	29.9	26.3	28.5	43.9	43.8
Has some say in disbursement of dowry	33.6	16.6	24.2	27.7	29.2
Has major say in disbursement of dowry	28.9	23.8	18.0	61.0	62.2
<i>Index</i> (maximum value = 7)	3.06	2.60	2.50	3.66	3.71
Freedom from threat					
Has been beaten by and fears husband	32.0	35.8	38.1	15.7	18.6
Has been beaten by husband	34.5	41.6	47.8	35.8	38.6
Fears husband	82.0	58.6	59.1	35.6	36.8
Neither beaten by nor fears husband	14.9	35.7	26.3	44.3	43.3
<i>Index</i> (maximum value = 3)	1.49	1.58	1.45	1.93	1.86

^aWeighted to adjust for larger sample size of Ramnathpuram Muslims.

In our village, the woman does not have any value, so most of the decisions are taken by men only. (Brahmin, Pratapgarh district, Uttar Pradesh)

We do not have any right to make decisions. The one who is uneducated, what decision could she take? She could only fight and quarrel. So it is right that the man alone takes decisions. (Jat, Meerut district, Uttar Pradesh)

Tamilian women, in contrast, are more involved in decisionmaking and are also more likely to believe that they are entitled to this authority:

Decisions should be taken jointly. A good decision can be made only when taken together—three-quarters of the time, decisions are taken by men and one-quarter of the time, they are taken by women and men together. (Scheduled caste, Ramnathpuram district, Tamil Nadu)

It is we who look after them [children] at home; they [husbands] go out to work, they don't have the time to look after children, therefore it is the mother who should take decisions. (Gounder caste, Coimbatore district, Tamil Nadu)

We [women] know more about the difficulties [of childrearing], we have the ability to think and see, men don't see, so we should take the decision regarding children, thinking that tomorrow our children should not be like us, they should be more than us. (Gounder caste, Coimbatore district, Tamil Nadu)

That women have limited mobility is evident. First, of the five places included in the index, the average woman can visit fewer than two places unescorted. Second, as expected, there is greater freedom to visit nearby or relatively friendly places such as the village market or community center, the health center, or the home of a relative or friend in the village than other more remote places, such as a fair or an adjoining village. Third, results point strongly to the north–south contrast: of the five places included in the index, the average Tamilian woman can visit 2.4 unescorted, whereas the average respondent from Uttar Pradesh and Punjab can visit only 1.4 places without an escort. And finally, among north Indian respondents, Hindu and Muslim women's freedom is about equally constrained, whereas Tamilian Hindus have moderately greater mobility than Tamilian Muslims.

Women's access to and control over resources is also limited. While the majority of women have a say in how household income is spent and get cash to spend, few feel free to make small purchases of jewelry or gifts on their own. Regional variation persists: Tamilian women are more likely to have a say in the disbursement of household income and to get cash to spend compared to women from the other sites (about 90 percent compared to about 75 percent in Uttar Pradesh and 70 percent in Punjab). South Indian women are, however, as unlikely as women from other sites to feel free to spend the household's resources on themselves without permission from their husbands or mothers-in-law.

With regard to ownership and control over household valuables and own dowry, some interesting disparities emerge. In the two Indian settings, the majority of Tamilian women, compared to one-quarter of women from Uttar Pradesh, report owning land or household valuables "in their own name" (not shown here). Tamilian women, however, are considerably more likely than women from Uttar Pradesh to report also that these are owned jointly with their husbands and hence that they would not be able to control the disbursal of these assets without the consent of their husbands. In contrast, Tamilian women, whose dowries are predominantly in the form of gold and jewelry, are considerably more likely to report owning and controlling their dowries than are women from Uttar Pradesh (whose dowries are more likely to consist of household goods). The situation in Punjab is different. Punjabi women in our sample come from higher-income households, and over three-fifths of them report owning and controlling household valuables, some of which come from their dowries, as in the case of women from Uttar Pradesh. As a result of these different patterns, profiles of control over resources also vary. Punjabi women appear more likely to own and control valuables than women from Uttar Pradesh or Tamil Nadu. Yet it is Tamilian women who report greater control over their dowries. And when asked about plans for supporting themselves in the future, it is once more Tamilian women who are most likely to report expectation of self-reliance. Despite these differences in patterns of control, the index of access to and control over resources suggests again that Tamilian women are better off than women from Uttar Pradesh and Punjab. Average scores range from 2.5–2.6 (of a possible 7.0) in Uttar Pradesh to 3.1 in Punjab and to 3.7 in Tamil Nadu.

In comparison to the regional disparities recorded for decisionmaking, freedom of movement, and access to and control over economic resources, regional variation in women's freedom from threat is muted. The index of freedom from threat suggests that south Indian women are slightly freer from threat than are northern women from Uttar Pradesh or Punjab.

Focus group discussions underscore the extent to which women in both settings accept these unequal power relations, and accept beating as the husband's prerogative (see Jejeebhoy 1998; Jejeebhoy and Cook 1997). The general impression is that women who are disobedient or who "misbehave" deserve to be beaten.

A woman is beaten if she does not wash her husband's clothes, does not cook food or quarrels with the husband's mother and sister or argues with the husband. If the work is done properly the husband does not beat her. (Muslim, barani region, Punjab)

If she has committed a sin she should be beaten, although many women are beaten without any reason. (Muslim, central Punjab)

Beating is for the woman's own good. If she does something wrong, loses something, or hits children in anger, the husband must show her [by beating]. (Brahmin, Pratapgarh district, Uttar Pradesh)

If it is a great mistake, then the husband is justified in beating his wife. Why not? A cow will not be obedient without beatings. (Muslim, Ramnathpuram district, Tamil Nadu)

Even so, Tamilian women argue for greater understanding and less force:

The husband should not beat his wife. He should advise her. It is better to advise than to beat. (Muslim, Ramnathpuram district)

Men should not beat their wives. But being born as females, we should listen to what our husbands say. (Scheduled caste, Ramnathpuram district)

In summary, women in Tamil Nadu have significantly more decision-making authority, mobility, and access to and control over resources than women from Uttar Pradesh and Punjab and somewhat more balanced power relations with their husbands. A comparison between women in Uttar Pradesh and Punjab, however, suggests greater similarities: while Punjabi women have more decisionmaking authority and access to and control over resources than women from Uttar Pradesh (irrespective of religion), indexes of mobility and freedom from threat are virtually identical for the two samples. Such findings offer strong support for the argument that region rather than religion has the stronger influence on women's autonomy in South Asia.

Dimensions and determinants of women's autonomy

In patriarchal contexts, tight controls are exerted on women in every sphere of their lives: their free movement, their voice in family affairs, their economic independence, and their relations with their husbands. In the absence of direct data relating to women's autonomy, previous studies have relied on a number of available indicators—years of education, work force participation, marital age, and spousal age difference in particular—as proxies for autonomy. On a policy level, moreover, it is often assumed that enhancing women's educational attainment and economic activity status and raising their marital age can directly increase their autonomy and the extent to which they have a say in matters concerning their own lives. Our dataset allows us to assess the effect of these proxies on the various dimensions of women's autonomy. We have constructed a summary index of autonomy. This index sums values in the four indexes discussed above and

shown in Table 4. Values on each index are standardized to vary between 0 and one; the summary index then combines each of these four values and thus ranges from 0 to 4.

Table 5 presents the results of regression analyses (OLS), which regress the summary index of autonomy on three sets of factors assumed to measure autonomy. The first set includes indicators usually assumed to measure women's autonomy, that is, education and wage-work status. The second includes other factors that may affect women's status in the gender-stratified settings of South Asia: age, parity, residence pattern, and size of dowry (in equivalent US dollars). The third is household economic status (as measured by the number of consumer goods owned). For Indian data, moreover, community-level factors (district, religion, state) are included among the explanatory variables. Results are presented for all women in Punjab (Pakistan) and separately for women from Uttar Pradesh and Tamil Nadu.

The most striking result of Table 5 is the different pattern of correlates affecting dimensions of autonomy in Punjab and Uttar Pradesh, on the one hand, and Tamil Nadu on the other. Also notable within each setting is that the summary index of autonomy appears to be explained by a different set of determinants. And third, socio-cultural context does indeed appear to condition the relationship of several correlates with autonomy, as evident from our study sample.

Determinants of the summary index of autonomy highlight the different patterns in the three sites. In Uttar Pradesh and Punjab, traditional measures of autonomy are far more powerful determinants of this index than they are in Tamil Nadu. In both Punjab and Uttar Pradesh, significant de-

TABLE 5 Correlates of the summary index of women's autonomy in the study sample: OLS regression coefficients, by site

	Punjab	Uttar Pradesh	Tamil Nadu
Attended primary school	0.07	0.10	0.17**
Attended secondary school	0.31**	0.19*	0.35**
Wage work in last 12 months	0.02	0.29**	0.08†
Age	0.02**	0.02**	0.03**
Number of surviving sons	0.01	0.04*	-0.02
Number of surviving daughters	-0.03†	0.02	-0.02
Resides with mother-in-law/extended family	-0.30**	-0.17**	-0.06
Size of dowry (\$)	0.0002**	0.0002**	0.0001
Number of goods owned	0.04**	0.05**	0.02*
District (ref.=less developed)		-0.03	0.04
Religion (ref.=Muslim)		-0.10*	0.19**
Constant	0.89**	0.57**	1.04**
Adjusted R-squared	0.13	0.17	0.12

ref. = reference category. ** p<=.01 * p<=.05 † p<=.10

terminants of this summary index include coresidence with mother-in-law, size of dowry, and age; additionally in Uttar Pradesh, autonomy is linked positively to the number of sons a woman has, and in Punjab it is related negatively to the number of daughters she has. In Tamil Nadu, by contrast, the only traditional factor to play a significant role is age.

Education and wage-work status are also significant determinants of the autonomy index, but their influence is generally stronger in Tamil Nadu than in the two northern sites. While in Tamil Nadu even a primary education significantly influences this index, in the other two sites it is not until a secondary education is attained that autonomy is significantly enhanced. And while wage-work status influences autonomy in all three settings, its influence is most significant in Uttar Pradesh. Finally, economic status is uniformly associated with autonomy in each site.

There is considerable regional variation in the determinants of other autonomy indicators (data not presented). Within each site, however, a relatively common set of socio-cultural factors is important in defining at least three indicators of autonomy, namely, decisionmaking authority, mobility, and access to and control over resources. These socio-cultural factors are economic activity, age, coresidence with mother-in-law, and size of dowry in Uttar Pradesh; family structure, age, and less consistently secondary education and wage work status in Punjab; and education, work status, age, and, to a lesser extent, coresidence with mother-in-law in Tamil Nadu.⁶

Our results therefore suggest that traditional factors conferring authority on women—age, marital duration, number of surviving sons, nuclear family residence, and dowry—have a more powerful effect on women's autonomy in Uttar Pradesh and Punjab, the settings with wider gender disparities, than in Tamil Nadu, where gender relations are more egalitarian. In contrast, in Tamil Nadu, education (even a primary education) plays a prominent role in enhancing almost every dimension of autonomy; wage work has a positive but less consistent effect. In Uttar Pradesh and Punjab, while both education and especially wage-work status enhance aspects of autonomy, their effect is less consistent.

Influences of region and religion on women's autonomy

The findings discussed above suggest that socio-cultural and regional context makes a difference in shaping factors that determine women's autonomy in South Asia. Our dataset enables us to discern, moreover, the extent to which such contextual factors as region, religion, and nationality play a role in determining the four selected components and the combined index of female autonomy in the subcontinent. In Table 6, data from the three sites are pooled, and the regression analysis performed in Table 5 is repeated. The aim is to focus directly on the three contextual factors.

TABLE 6 The influence of cultural, national, and geographic context on indicators of women's autonomy in the study sample, OLS regression coefficients^a

All women by religion, country, and region	Decision-making (0-6)	Mobility (0-5)	Freedom from threat (0-3)	Access to and control over resources (0-7)	Summary index of autonomy (0-4)
Religion					
Muslim	ref.	ref.	ref.	ref.	ref.
Hindu	0.125*	0.45**	-0.14*	-0.12 [†]	0.05
Country					
Punjab, Pakistan	ref.	ref.	ref.	ref.	ref.
Uttar Pradesh and Tamil Nadu, India	-1.11**	-0.01	0.17*	0.10	0.14 [†]
Region					
Northern subcontinent (Punjab and Uttar Pradesh)	ref.	ref.	ref.	ref.	ref.
Southern subcontinent (Tamil Nadu)	1.79**	0.63**	0.34**	0.80**	0.65**
Adjusted R-squared	0.36	0.18	0.07	0.18	0.30

ref. = reference category. ** p<=.01 * p<=.05 † p<=.10

^aControlling education, economic activity status, age (also as a proxy for marital duration), parity, coresidence with mother-in-law, dowry, economic status.

First, our findings suggest that religion plays a modest role in influencing female autonomy in our study sample. We have already seen in Table 5 that the hypothesis that Muslim women experience greater constraints on their autonomy than Hindu women is not borne out. In Tamil Nadu, for example, Hindu women do indeed experience more autonomy than Muslim women in such areas as decisionmaking and mobility. In Uttar Pradesh, by contrast, Hindu and Muslim women are equally constrained in terms of decisionmaking and mobility, and Muslim women are significantly freer from threat and experience greater access to and control over resources than Hindu women.

With the inclusion of Muslim women from Pakistan in Table 6, findings clearly suggest that differences between Indian and Pakistani women can be attributed neither to nationality nor to religion. Rather, after controlling for the effect of a host of socio-cultural factors, every indicator of autonomy remains strongly conditioned by region within the subcontinent, with Tamilian women (representing women from the south) experiencing significantly greater autonomy than women from either Uttar Pradesh or Punjab (jointly representing women from the north). There is greater distinction in women's autonomy by a broad north-south residence than by religious ascription.

Findings clearly reiterate the modest and inconsistent influences of religion and nationality in shaping female autonomy in the region. For ex-

ample, as far as religion is concerned, Hindu women have about as much autonomy (as measured by the summary index) as Muslim women do, particularly in the areas of access to and control over resources; Hindu women, however, have significantly more decisionmaking authority and mobility, and significantly less freedom from threat. Nationality, similarly, has an inconsistent influence: after controlling for region and religion and other socio-cultural factors, Indian women appear to have less decisionmaking power than do Pakistani women, greater freedom from threat, and about as much mobility and access to and control over resources.

In contrast, region plays a strong and consistent role in shaping female autonomy. No matter which indicator of autonomy is considered, women residing in the southern part of the subcontinent consistently display significantly higher levels of autonomy than do women residing in the north. Moreover, the influence of social system, as measured by region, in almost every case is far stronger than that of religion or nationality.

Conclusions and discussion

Our findings, based on a sample from Pakistan's Punjab Province and from two Indian states, indicate that women's autonomy—in terms of decisionmaking, mobility, freedom from threatening relations with husband, and access to and control over economic resources—is highly constrained in Pakistan and in north and south India. Findings suggest that South Asian women are largely excluded from family decisionmaking; they have limited access to, and exercise limited control over resources; their freedom of movement is severely constrained; and few are free from threat and violence at the hands of their husbands.

Our findings also confirm considerable variation in the levels and determinants of women's autonomy in this region. The evidence suggests that in the more gender-stratified settings of Uttar Pradesh and Punjab, autonomy is largely the result of factors that traditionally confer status, notably family structure or absence of controls implicit in coresidence with mother-in-law and size of dowry, along with economic activity (in Uttar Pradesh) and a secondary education (in Punjab). In contrast, in the more egalitarian setting of Tamil Nadu, education and to a lesser extent economic activity are powerful determinants of almost every indicator of autonomy. Traditional forces such as coresidence with mother-in-law and dowry continue to affect several measures of autonomy, but these effects are generally modest. The implication, in short, is that in the highly stratified settings of rural Uttar Pradesh and Punjab, autonomy continues to be shaped largely by traditional factors. Although secondary education in Punjab and economic activity in Uttar Pradesh tend to enhance autonomy also, their effects are less consistent, so that they may be relatively poor proxies for autonomy. In Tamil Nadu, by contrast, education and to a lesser extent economic activity

tend to raise almost every indicator of autonomy, and the use of education in particular as a proxy for women's autonomy is more justified. These findings suggest that the cultural context—operationalized here by region—influences the factors associated with autonomy, and they argue for context-specific measures of women's autonomy.

Our findings demonstrate the centrality of social institutions of gender within each community, as defined here by region of South Asia—rather than primacy of religion or nationality—in shaping women's autonomy. A clear regional divide, net of individual and household characteristics, is evident in almost every index of autonomy: decisionmaking authority, mobility, access to and control over economic resources, and, to a lesser extent, freedom from threat by husbands. Women in Tamil Nadu experience far greater autonomy than women in either Uttar Pradesh or Punjab. Also striking are the similarities in female autonomy in Uttar Pradesh in India and Punjab in Pakistan. Most convincing are findings from the pooled regression analysis, demonstrating the relative strength of the prevailing social system, as operationalized by region, in conditioning female autonomy levels in the subcontinent.

In comparison, the influences of religion and nationality are less consistent and powerful. There is little support for the argument that Muslim women are disadvantaged in terms of autonomy, at least when compared to Hindu women from the same region. For example, the pooled regression analysis provides further evidence that once region and nationality are controlled, the influence of religion is moderate and inconsistent. Also, a comparison of Hindu and Muslim women in the Indian sample, and a comparison of Indian and Pakistani women in the pooled sample suggest that, once region is controlled, levels of autonomy are not very different among Hindus and Muslims. In South India, however, there is moderate support for the argument that Hindu women have greater autonomy than Muslim women. In Uttar Pradesh, by contrast, Hindu–Muslim differences in every dimension of autonomy are insignificant. When women from all three sites are considered, Tamilian Muslims exhibit far greater levels of autonomy than do either Hindu or Muslim respondents from Uttar Pradesh and respondents from Punjab.

Some political and literary commentators have argued that religion remains an important part of the explanation of social conservatism in the subcontinent and that the greater conservatism in north India can be dated to the advent of Islam in the northern part of the subcontinent and the establishment of Muslim culture there. Furthermore, in contrast, the direct influence of Islam has been modest in the south and Hindu culture has dominated there. This view has been questioned by a number of scholars. For example, as Dyson and Moore (1983: 47) argued: “[W]e note the general view of scholars that both the essential features of the regional differ-

ences in kinship..., and their broad geographical distribution, are of very long standing. It is widely held that they mostly predate the Muslim presence, reflecting instead basic differences between northern 'Aryan' and southern 'Dravidian' culture areas." Our finding that, once region is controlled, differences in autonomy between Hindu and Muslim women are modest is consistent with this latter view.

Our findings also have implications for policy. In particular, the findings that education and employment do not necessarily enhance women's autonomy and that traditional factors conferring status on women remain strong suggest that strategies to enhance women's autonomy need to expand beyond education, employment, and delayed marriage. More comprehensive, direct, and context-specific strategies to increase women's autonomy must simultaneously be sought. These include raising women's gender consciousness, enabling women to mobilize and access community resources and public services, providing support for challenging traditional norms that underlie gender inequities, facilitating the acquisition of usable vocational and life skills, enhancing women's access to and control over economic resources, and enabling women to establish and realize their rights (see, for example, United Nations 1995; Batliwala 1994; Mahmud and Johnston 1994; World Health Organization and UNICEF 1994). These strategies are particularly important for the northern cultures of the subcontinent—whether Pakistani or north Indian, whether Hindu or Muslim.

Notes

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1 A profile of the ten communities in Punjab underscores their agro-ecological and socioeconomic differences. Corresponding differences are also observed in farming systems, employment, and land ownership patterns. In the northern rainfed (barani) regions, land ownership is broad based, with a large majority of households owning small parcels of land. Agriculture does not offer a reliable and adequate source of income, is constrained by the uncertainty of water supply, and is chiefly geared to subsistence production with the main

crops being wheat, maize, and millet. A large proportion of men are employed in nonagricultural occupations, particularly in the armed forces, and in formal-sector jobs including in the nearby urban center; subsistence agricultural production is managed largely by women.

The northern semi-irrigated villages are similar to the northern barani areas except that some cash crops are grown and land ownership is more concentrated. In contrast, in the canal-irrigated communities of central and southern Punjab, agriculture is market oriented and a lucrative source of income. It is also more feudal, with land concentrated among a small proportion of the population. Whereas central Punjab is characterized by a substantial number of medium-sized farms, southern Punjab contains large farmers and feudal lords. It is not unusual, in southern Punjab, for a family of large landlords to own the entire agricultural and residential area of certain villages. Correspondingly, households

in central Punjab have greater access to urban white-collar employment than do those in the south.

2 A profile of the eight communities in India also highlights considerable heterogeneity. In Uttar Pradesh, Pratapgarh district, in the east, is a poor, largely wheat-producing area, with few amenities; while theoretically available, health and educational facilities function only sporadically. Brahmins represent the dominant Hindu caste. In contrast, Meerut district is very well off: its main crop is sugarcane, although wheat, millet, and maize are also produced. Our sample sites lie in relatively close proximity to the main town, and less than 100 km from New Delhi. Amenities and services are largely available, as are a host of private health and educational facilities. Jats are the main Hindu caste.

In Tamil Nadu, Ramnathpuram district lies on the southeastern coast. Palmyra is the main crop, and occupations revolve around tending plantations, cutting down and marketing coconuts, and processing fiber. Villages tend to be poorly connected by roads, have severe water supply problems, and are often reduced to depending on rain and river water. School and health facilities exist in, or within walking distance of, most villages and, by and large, function regularly. In contrast, Coimbatore is one of the richest districts of Tamil Nadu: its main crops are cotton and groundnut. Transportation, communication, and other amenities are of good quality; piped water is available in many villages. The main Hindu castes of Ramnathpuram and Coimbatore are, respectively, Nadars and Gounders, both from the upper castes.

In each selected taluka, village lists were drawn up; these included information on the total number of households in each village by religion and caste. In order to adequately represent Muslims and scheduled caste households, contiguous villages were merged into sampling units of roughly 1,000–2,000 households, in a way that would allow for adequate representation of the different groups in our design. As a result, in Tamil Nadu, where there are generally few Muslims, clusters of villages were much larger than in Uttar Pradesh, where Muslims represent a substantial proportion of the population. The primary sampling unit (PSU) included in the sample was then selected randomly.

In Tamil Nadu, the selected PSU contained 12 villages from Pollachi (Coimbatore district) and 15 from Mudukulathur (Ramnathpuram district). The selected PSUs in Uttar Pradesh contained fewer villages: 7 from Kunda (Pratapgarh district) and two large villages (with many “petis,” or identifiable clusters) in Baghpat (Meerut district). Each household in the selected cluster of villages was listed and this list constituted the sampling frame. The difference in the number of villages selected in each state is attributed to the following: (1) village sizes tend to be larger in Uttar Pradesh than in Tamil Nadu, and (2) since Muslims constitute less than 10 percent of the population of Tamil Nadu, a larger number of villages was required in order to reach our target composition of respondents.

3 A limitation of the index of decision-making authority is that the way it weights women's participation raises equivalence problems with certain scores where it could be suggested that having the major say in fewer decisions yields more autonomy than merely participating (but not having the major say) in many.

4 The decision to assign a value of 2 (lower autonomy) to women who fear but are not beaten by their husbands, and 1 to those who are beaten but do not fear their husbands (more autonomy) was made on the basis of focus group discussions: women spoke of fearing husbands as a way of showing respect to them and of endeavoring not to displease or disobey them. Beating was described, however, as a humiliating experience, in which husbands demonstrate their displeasure with the failings of their wives; and members of the community tend to see women who were beaten as having been disobedient.

5 The quotations from women that follow were translated from the original Urdu and Hindi by the research teams.

6 The remaining two indicators of autonomy (freedom from threat and control over resources) are defined by a somewhat different group of socio-cultural factors. A look at the determinants of freedom from threat suggests quite diverse inter-site patterns. In Uttar Pradesh, for example, larger family size appears to enhance freedom from threat; in Tamil Nadu and Punjab, by contrast, women with many children have more unequal relations with their husbands than low-

parity women. While education clearly enhances freedom from threat, its influence is most powerful, as expected, in Tamil Nadu. In Punjab, while wage-earning work is associated with greater violence, size of dowry appears to free women from

the threat of violence. Finally, and particularly in the two Indian sites, women from better-off households tend to report greater freedom from threat than other women.

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Spatial Patterns of Fertility Transition in Indian Districts

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OVER THE PAST five decades, numerous studies have assessed levels, trends, differentials, and determinants of fertility in India. The conclusion is that fertility decline has been low to moderate except for a few pockets of more rapid transition. Until recently, the analysis of demographic transformation in India has been limited to fertility indicators at the state level. The district-level data from the censuses of 1981 and 1991 portrayed a much more complex situation in the country as fertility differentials often proved to be as substantial within states as between states. (Figure 1 displays Indian states and large cities.) Because of India's cultural, economic, and geographical diversity, the magnitude of regional variations in fertility levels is much larger than that in China, and comparison with the demographic history of Europe or of the former Soviet Union would be more appropriate.¹

Within the context of demographic heterogeneity, this article seeks to update our knowledge on fertility levels in India and to extend our understanding of the mechanisms behind regional variations. We present the results of a new estimation procedure to reconstruct the Indian fertility transition and describe some of its spatial and statistical properties. Rather than test hypotheses on fertility–economy–society linkages through an econometric model,² we focus on the spatial structuring of reproductive behavior in India: fertility is examined as a *regionalized* variable, that is, a variable which is assumed to be spatially continuous.³ As our maps suggest and the geostatistical analysis demonstrates, spatial variations of fertility in India are far from random, a fact that has potentially significant implications for our interpretation of fertility decline. Specifically, we suggest that preoccupation with the effect on fertility of factors that are poorly correlated with spatial location, such as family planning campaigns or structural transformations of the economy, may have concealed the progression of fertility change through diffusion processes at the microlevel.

FIGURE 1 Indian states and major cities

Sources for studying the Indian fertility transition

Using the age distribution of the 1961 and 1971 Indian censuses, Adlakha and Kirk (1974) concluded that the level of fertility during the early 1960s did not differ substantially from the level during the early 1950s. As they summarized their findings: “The crude birth rate in India declined by between seven and 10 per cent, from a level of about 45 in 1951–61 to about 40.5–42.0 in 1961–71” (p. 400). Extending the same data up to the 1981 census, Rele (1987) concluded that the total fertility rate remained stable at around 6 during the 1950s and into the first half of 1960s. The turning

point in Indian fertility seems to have occurred around 1966, with an estimated TFR of 5.8 in 1966–71, 5.3 in 1971–76, and 4.7 in 1976–81. The estimated levels and trends of fertility for 14 major Indian states showed remarkable geographic consistency, with northern states having higher fertility than southern states in 1961–66 and, with only slight modifications, in 1976–81.

Jain and Adlakha (1982) corroborated that the fertility rate in India before 1961 was high and stable. Their analysis indicated that the crude birth rate in India fell from 41 births per thousand in 1972 to 35–37 in 1978 and that the decline was primarily caused by declining age-specific fertility rates. As in the case of two national surveys, analysis of the age distributions of the censuses of 1971 and 1981 suggested that a major fertility decline was underway during the intercensal period (Preston and Bhat 1984). A large share of this decline probably occurred in the late 1970s; the fertility reduction seems to have been slightly faster in the southern states.

Assessing the degree of heterogeneity in fertility behavior within Indian states, Guilmoto (2000) concluded that fertility decline began in the periphery along the coasts and in the extreme south, and spread progressively to encircle the region around the Ganges Valley, the heart of traditional India, where fertility has scarcely declined. The Hindi-speaking core region is characterized by high fertility, an entrenched patriarchal value system, economic underdevelopment, predominance of Brahminical influence, and exclusion of women from education. In south India, Kerala has long been recognized for its rapid fertility transition, occurring in the absence of significant economic development as conventionally measured. Female literacy is the single most frequently cited indicator in explaining this achievement.⁴ A few studies have also focused on the recent fertility experience of Tamil Nadu and of south India in general.⁵ Tamil Nadu is notable for having achieved replacement-level fertility without reaching Kerala's high level of female literacy or its low level of infant mortality. Using the state-level indicators of fertility, a number of researchers have grouped Indian states into two demographic regimes: south with low fertility and north with high fertility.⁶

Very few studies permit assessment of fertility levels and trends at the district level. For the first time in the 1981 census, the Registrar General of India provided district-level estimates of fertility and mortality using indirect techniques (Registrar General of India 1988, 1989), and a few studies using these data have appeared since then.⁷ For the 1991 census, the Registrar General's indirect estimates of fertility and mortality at the district level as well as a few estimates by individual researchers are now available.⁸ Bhat (1996), using the reverse-survival method, produced birth rates at the district level for the periods 1974–80 and 1984–90 and also analyzed cross-sectional variations in fertility.

More recently, using data generated by the National Family Health Survey (NFHS), Bhat and Zavier (1999) analyzed the differentials in fertility within 76 regions of the country. Among the 76 regions around 1975, none had a total fertility rate under 3 births, 12 regions were in the range of 3–4, 35 in the range of 4–5, and the remaining 29 regions were above 5. In 1987, ten regions had total fertility rates under 3, 34 were in the range of 3–4, 24 in the range of 4–5, and only eight regions were above 5. In effect, after a lapse of 12–13 years, only 30 percent of the regions had remained in their previous total fertility class. The NFHS estimates confirm the earlier finding (Bhat 1996) of substantial reductions in fertility throughout the country, not just in a few pockets.

Although the Sample Registration Survey (SRS) has been widely used to study fertility since the 1970s, it provides estimates of fertility for major states only. In recent years the SRS has published estimates for smaller states and union territories but does not provide information on fertility at the district level. Surveys conducted in a number of states (Mysore Population Study, for Karnataka; Gandhigram Institute Survey, for Tamil Nadu; Kerala Fertility Survey, for Kerala, to name a few) focus on fertility and its determinants at the state level. Against this background, this article aims to provide a new set of fertility estimates at the district level starting from the 1950s. We use a new database that integrates all district-level age data drawn from the 1961, 1971, 1981, and 1991 censuses. We devise a new index based on child–woman ratios computed from census age distributions.⁹

From the child–woman ratio to the child–woman index

Estimates derived from child–woman ratios (CWRs)¹⁰ can be used to reconstitute fertility trends at the district level over a 40-year period. The combination of two CWRs calculated by using different numerators and denominators in the ratio enables us to estimate fertility levels for each five-year interval. Specifically, we calculated CWRs as children aged 0–4 divided by women aged 15–49 and as children aged 5–9 divided by women aged 20–54. For example, 1961 CWR values yield estimates for 1951–55 (using the 5–9-year age group as numerator) and for 1956–60 (using the 0–4-year age group). Our 1961–91 database thus provides a set of district-level fertility measurements for eight five-year intervals from 1951–56 to 1986–91.

Compared with other fertility indexes, the child–woman ratio has several limitations. The main shortcoming is that this ratio is based on surviving children in different age groups and not on the number of live births. The following simplified formula highlights this flaw:

$$\text{CWR} = \text{children/women} = \frac{(\text{births} \times \text{child survival})}{(\text{women} \times \text{adult survival})}$$

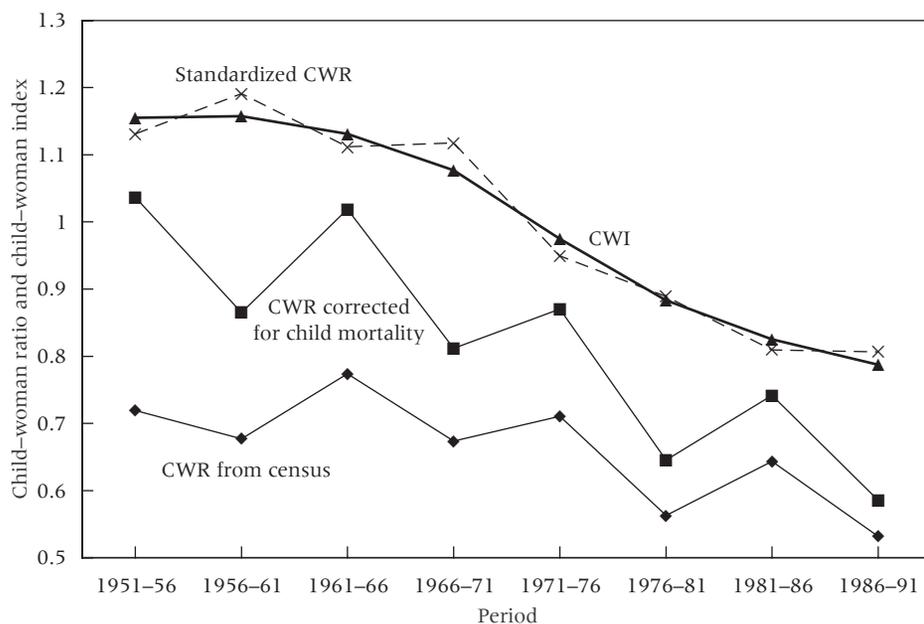
Mortality differentials between regions or census periods may cause variations in CWRs and make comparisons between CWRs potentially misleading. Raw values of the CWR reflect fertility levels as well as child mortality levels. Furthermore, because India was characterized by rapid mortality decline during the period in question, inter-temporal variations in CWRs imperfectly reflect fertility changes over time. Correcting for the mortality factor is therefore a first requirement in improving the index.

A second limitation relates to the simultaneous use of two different CWRs that are not directly comparable because they are based on different age groups. As noted above, we use both the 0–4 and the 5–9-year age groups to reconstruct fertility during the two quinquennia preceding the census year: the two CWRs require adjustment to be comparable.

Apart from the effects of child and adult mortality just noted, other limitations to the use of the CWR are related to the changing shape of the age distributions of women 15–54; gains and losses through net in- or out-migration; and the accuracy of age enumeration and age reporting. No fully adequate correction is feasible for the distortions these factors cause in the CWRs as an index of fertility. It is worth stressing, however, that our attempt is not to estimate fertility values per se, but to arrive at a comparative index of fertility trends over time and of differentials by region. Hence, we have not attempted a correction for mortality variations among the adult female population, nor have we tried to take into account migration. We consider these factors minor in explaining CWR differentials compared to the effect of the fertility component as reflected in the census data for children aged 0–9. We have also used the raw total of the adult female population aged 15–49 and 20–54 as a denominator for calculating CWRs instead of weighting age groups by their respective share according to an estimated period fertility schedule.¹¹ For lack of reliable and detailed estimates, we have also ignored the possible impact of changes in the completeness of the census and in the accuracy of age reporting. In view of the poorer quality of registration and age reporting in the earlier censuses, such changes may be responsible for the erratic age distributions observed in some districts. The standardization procedure explained below, which yields a measure we call the child–woman index or CWI, seeks to minimize the effect of these problems in the underlying statistics.¹²

Details of the computation of the new CWI are given in the Appendix. The index provides a comparative fertility indicator at the district level for the eight five-year periods starting with 1951–55 and ending with 1986–90. As a feature of the standardization, we equate the average value of the CWI over 1961–91 to one.

To illustrate the effect of our correction and standardization procedures, Figure 2 presents the all-India values derived from child–woman ratios, starting with the original CWR values computed from the raw census age distributions. Once corrected for mortality, the ratio displays more pronounced

FIGURE 2 Child–woman ratio and child–woman index, all-India, 1951–91

NOTE: CWRs are computed from census data, using successively children aged 5–9 and children aged 0–4. For description of the correction and standardization procedures, see the Appendix.

variations because the impact of declining mortality over four decades is removed. However, the serrated profile of these lower two curves shows that the CWR based on children aged 5–9 and women aged 20–54 is not directly comparable to the CWR based on children aged 0–4 and women aged 15–49. In fact, the former ratio (used for the 1951–56, 1961–66, 1971–76, and 1981–86 quinquennia) seems to overestimate fertility when compared to the latter ratio (used for the other quinquennia). After standardization and limited smoothing (see Appendix), the final child–woman index provides a reliable indicator of fertility variations across periods and districts.

A statistical description of the results (see Table 1) shows that while average fertility levels decreased after 1961, the variability of fertility indi-

TABLE 1 District averages of the child–woman index, India, 1951–91

	1951–1956	1956–1961	1961–1966	1966–1971	1971–1976	1976–1981	1981–1986	1986–1991
District average	1.155	1.157	1.131	1.076	0.974	0.883	0.825	0.787
Standard deviation	0.144	0.137	0.147	0.149	0.136	0.140	0.151	0.166
Coefficient of variation (percent)	12.5	11.9	13.1	13.8	14.0	16.0	18.4	21.2
Number of districts used in the computation	316	316	338	338	331	331	328	328

cators in India almost doubled during the same period. Demographic change in India is now a major differentiating factor among regions that shared a common profile during the colonial period.

Mapping fertility transition from 1951 to 1991

The next step is to plot the district values on a series of maps covering 40 years (1951 to 1991) of demographic transition in India. CWI values are first plotted on the map, using the geographical coordinates of district headquarters. The local fertility values are then converted to a surface map using kriging, a standard geostatistical procedure, to interpolate fertility values on the entire map of India.¹³ Kriging is the optimal method of spatial interpolation, and it generates the most accurate estimates of surface values among available methods of spatial smoothing (see Appendix for details on our kriging procedure). The outcome consists of a “grid” made of small square cells (20 km by 20 km). After kriging, local grid values are contoured using seven value classes. The final result is a set of eight five-year maps depicting fertility transition from 1951 to 1991 (see Figure 3). Because the CWI is corrected and standardized for mortality, comparisons across regions and across five-year periods are possible. Although regional biases—resulting from such factors as serious age misstatement or differential underenumeration of children—may persist, especially for the 1961 and 1971 census data, the mapping enables us to follow regional and all-India trends and to examine 40 years of changes in fertility behavior.

The first two maps in Figure 3, for the 1951–61 period, show the limited variation in pretransitional fertility within India. The northeast, where the data quality is admittedly poor, exhibits the highest fertility levels. Several pockets of moderate fertility are visible both in south India (south Kerala and Tamil Nadu) and in mountainous districts in the western Himalayas. As further maps will show, these areas remain characterized by below-average fertility. An area of moderate fertility comprises several adjacent rural districts in central India across Madhya Pradesh and Maharashtra. This feature disappears in later maps. Although the possible effect of region-specific age misstatement cannot be ruled out, more information is required on pretransitional fertility regimes in India to elucidate this feature.¹⁴

Fertility increased across India after 1956 except in the moderate fertility areas of south India and the mountainous districts. The rise is especially discernible in western India and in the northern part of the subcontinent until the 1960s. (This pretransitional rise in fertility is highlighted in Dyson and Murphy 1985.)

In the 1960s, fertility decline began in several areas. This reduction is most pronounced in the southern tip of India.¹⁵ The drop in fertility is also visible in Tamil Nadu and Kerala, as well as in south Karnataka and Andhra

FIGURE 3 Child–woman index, 1951–91

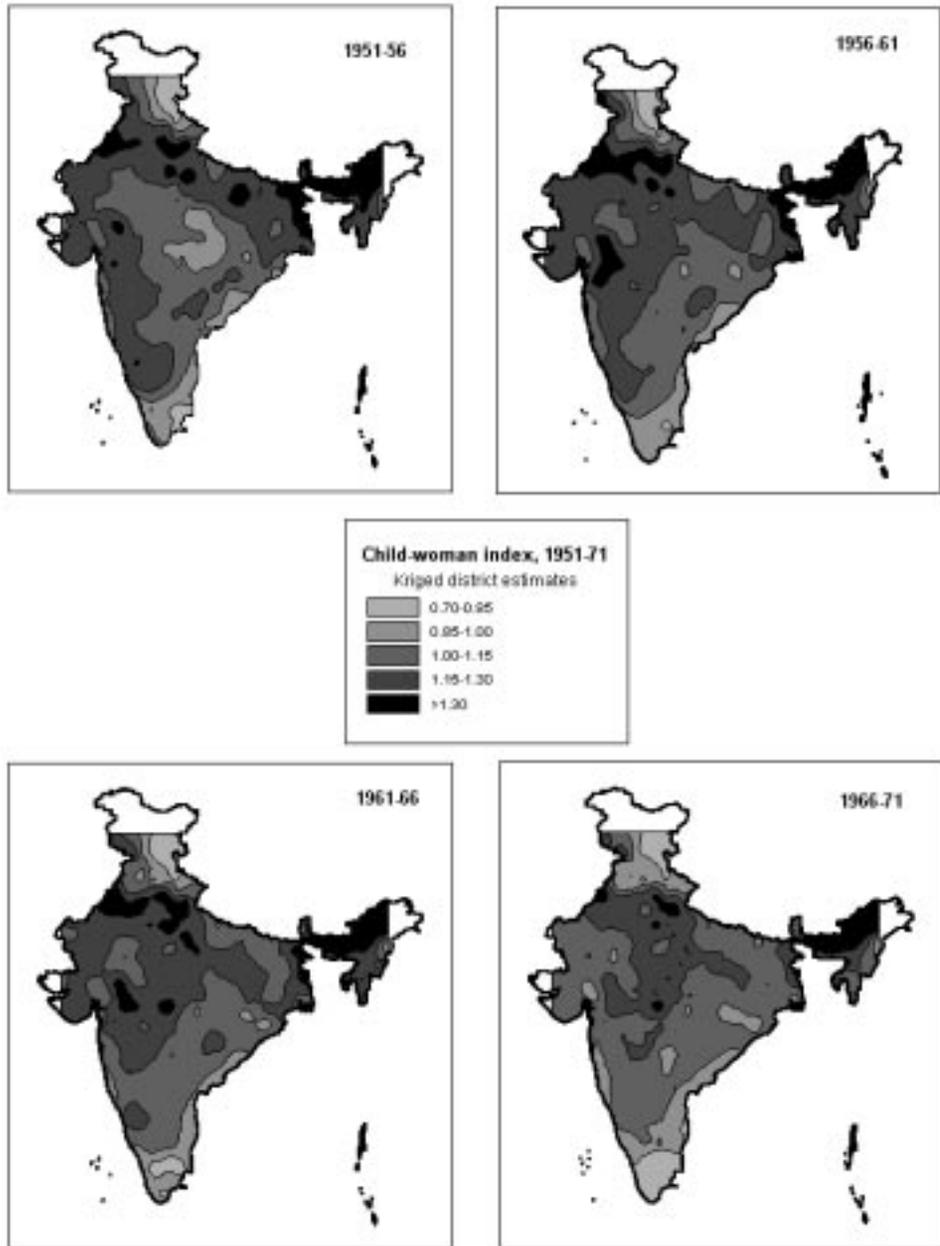
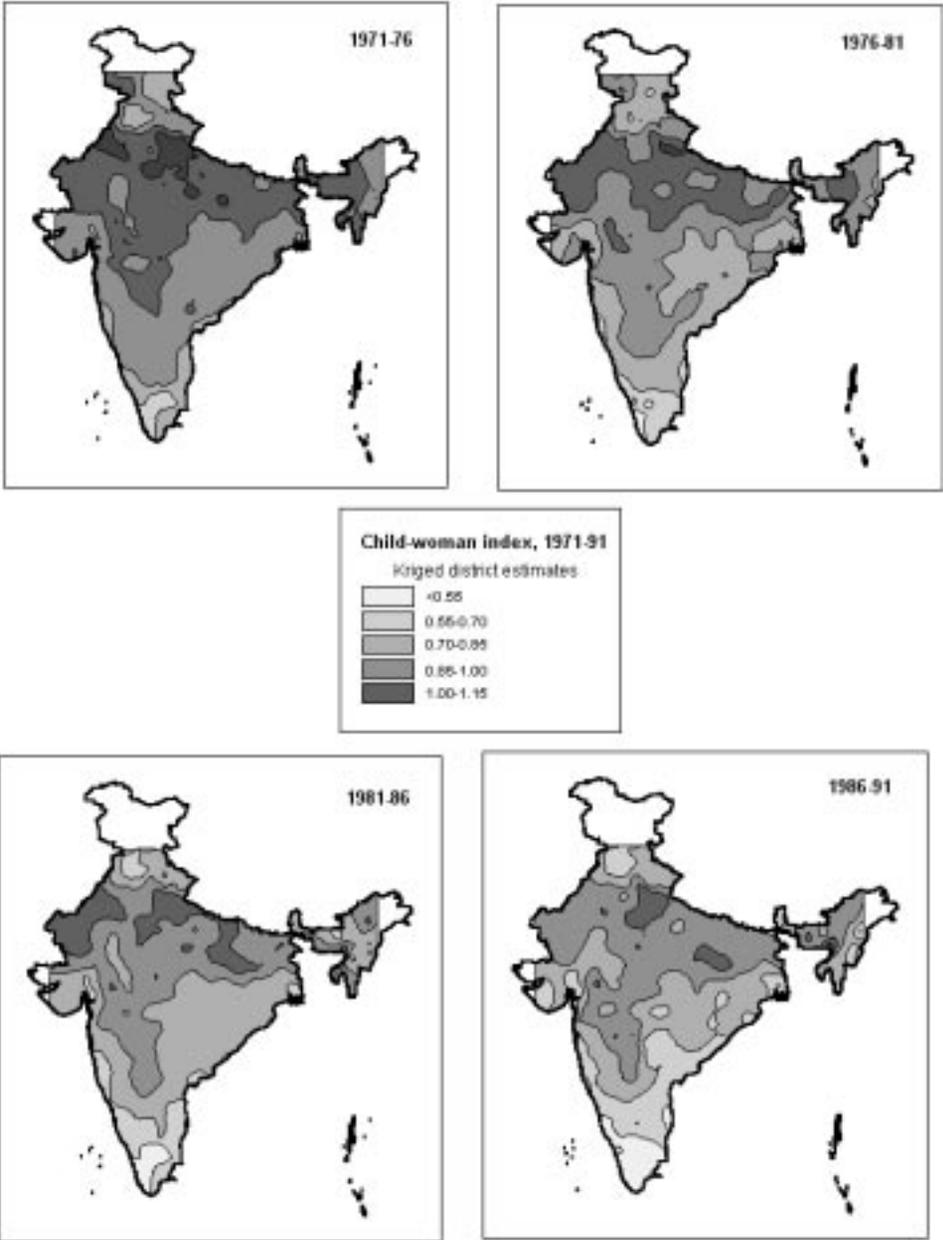


FIGURE 3 (continued)



NOTE: Blank areas refer to areas for which kriging is not possible (see Appendix).

Pradesh. Interestingly, coastal areas both in the west (from Mumbai in Maharashtra to Goa) and in the east (almost the whole of coastal Andhra Pradesh) are affected.¹⁶ The geographical logic of this decline is pronounced as all the affected regions are contiguous. Other regions in India exhibiting a downward trend in the fertility index were the high-altitude areas in Himachal Pradesh and Jammu-Kashmir as well as the north of Punjab. Ironically, Punjab, whose wheat-growing plains had reaped the early benefits of the Green Revolution during the 1960s, was cited during the same period as the high-fertility area par excellence in an influential debate on the rationale for fertility behavior in developing countries.¹⁷ Adjacent areas in the state of Haryana and western Uttar Pradesh, where agriculture also made great progress during the same period, show no trace of fertility reduction. The Punjab–Haryana border, which more or less demarcates Sikh-dominated Punjab from Hindu-dominated areas (Haryana and Uttar Pradesh), still separates areas of low and high fertility, as is evident on our maps for the late 1980s.

The first four maps in Figure 3, covering the period 1951–71, show that the high-fertility areas of northern India gradually formed a single block centered near the border between the three states of Rajasthan, Uttar Pradesh, and Madhya Pradesh. Another core area characterized by high fertility comprises the Brahmaputra Valley in the northeast state of Assam, the northern portion of West Bengal, and some smaller states in the northeast such as Meghalaya and Arunachal Pradesh. In other states of the northeast (Nagaland, Manipur, and Mizoram), where many regional tribes have been Christianized, literacy tends to be higher and fertility is more moderate.

The maps in Figure 3 that pertain to the 1970s, a period characterized by aggressive family planning campaigns in India, show the gradual spread of fertility decline across most regions. The fall is most visible in southern India, below a line that could be drawn from Gujarat in the west to Calcutta in the east. Although the pioneering districts of Kerala and Tamil Nadu are still far ahead, fertility decline has been rapid everywhere in south and central India. New pockets of pronounced fertility reduction have become visible in Gujarat and in southern districts of West Bengal. In the latter region, fertility has already reached a low value in and around the city of Kolkata (formerly Calcutta) by the 1970s, but this downward trend also manifests itself in the entire southern part of the state.

The coastal pattern of fertility change is still evident, especially as interior districts in the Deccan Plateau have experienced a less rapid pace of demographic change. The eastern tip of Maharashtra (Vidharba) and a few districts in Uttar Pradesh around the cities of Lucknow and Kanpur represent exceptions to early fertility decline in interior India. In the northwest, the fertility decline becomes pronounced in all Punjab districts and in the union territory of Chandigarh. Fertility decline has still not spread as might

have been expected to adjacent rural areas in Haryana and western Uttar Pradesh. On the contrary, the decline seems to have expanded from Himachal Pradesh toward the northwest of Uttar Pradesh (Kumaon), which comprises several mountainous districts at the foothills of the Himalayas.

The picture becomes more complex during the 1980s. Although fertility decline is occurring in almost every region of India, persistent differentials between subregions give our maps a patchwork appearance. A major feature of this period is the significant contraction of the high-fertility zone in India that formerly covered most of Bihar, Madhya Pradesh, Rajasthan, and Uttar Pradesh. Fertility has decreased considerably in central Uttar Pradesh, while less concentrated decline was underway in Rajasthan and Bihar. In the northeast, the demarcation between the western states (Assam, Meghalaya, and Arunachal Pradesh) and the eastern states (Manipur, Mizoram, Nagaland, and Tripura) became more acute, as the latter have recorded rapid fertility decline. By the late 1980s, fertility in Manipur and Nagaland was as low as in south Indian states.

In the south, the fall in fertility rates in the 1980s accelerated nearly everywhere. In many districts of Kerala and Tamil Nadu, values of the child-woman index reached a value less than half of those estimated for north India. Districts with the lowest values of the index were still highly concentrated in two pockets, in west Tamil Nadu (Coimbatore region) and in south Kerala. Whereas fertility decline in north India has profoundly redrawn the map of fertility differentials, relative variations between subregions in the south have been more or less preserved. Only the central region of the Deccan Plateau (central Maharashtra, north Karnataka, and sections of western Andhra Pradesh) seems to have remained a partial exception to fertility decline—not unlike the western districts of Uttar Pradesh, where any diffusion of the rapid decline in the Punjab and Himachal Pradesh seems to have remained minimal.

Three fertility profiles

Exploring the individual cases of hundreds of districts that may have little in common in terms of social, cultural, or economic characteristics would be a tedious exercise. Many local fertility trends may be explained by unique sets of historical characteristics, bearing little resemblance to conditions in neighboring areas. At the same time, paying attention only to broad regional aggregates, such as state average values, would obscure fertility trends that stand out on our contour maps. We have, therefore, opted for a statistical reexamination of our district-level estimates in order to identify major “fertility profiles” as a means to describe 40 years of demographic change in India. Because correction and standardization procedures for the child-woman index yield a consistent time series for several hundred Indian dis-

tricts, we have performed a cluster analysis on this database, using districts as observation units and the various five-year fertility estimates as variables. The cluster analysis is a technique aimed at providing the best partition of our district units. After repeated trials, we opted for a three-way partition of our district sample as the most convenient for analysis and presentation.¹⁸

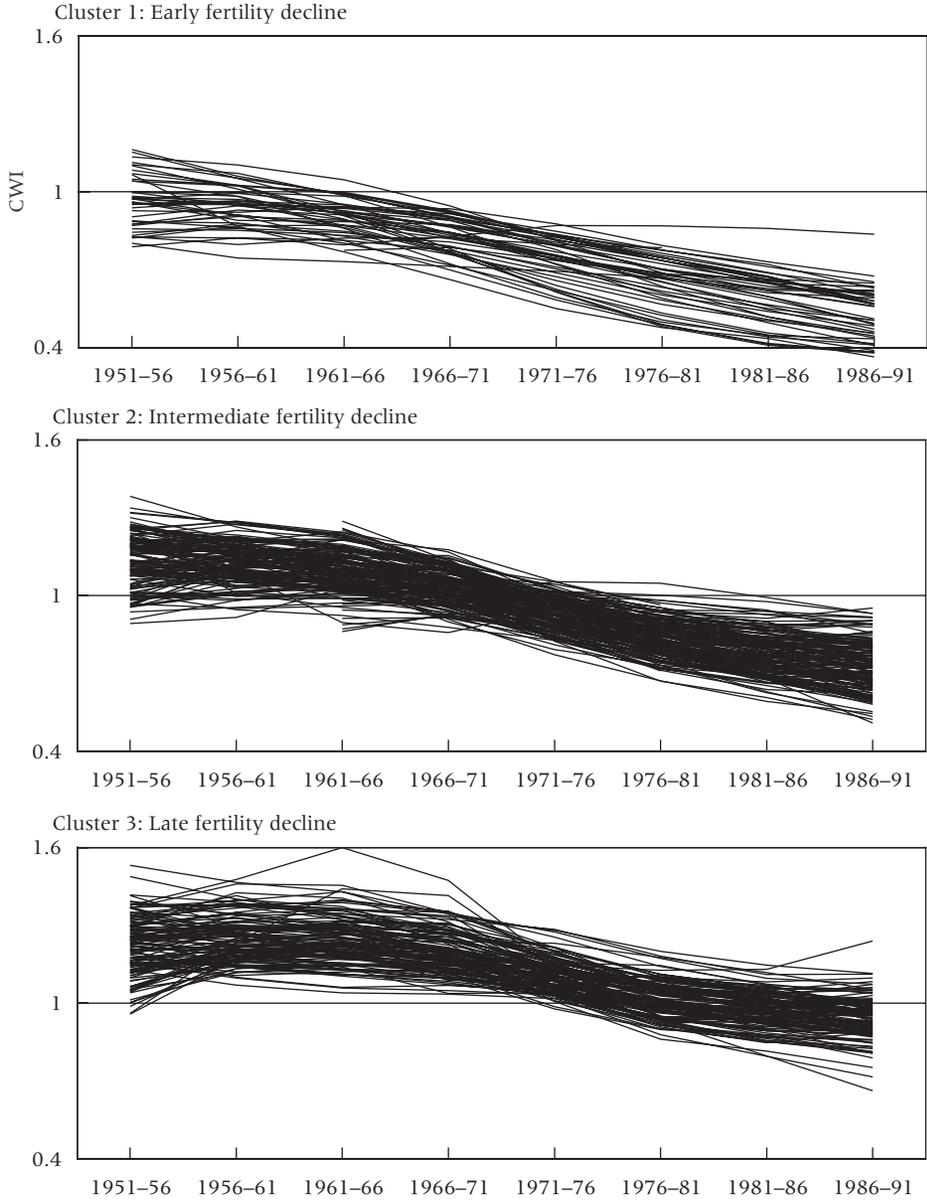
The clusters that divide Indian districts into three fertility groups include respectively 44, 159, and 135 districts. While fertility characteristics in the three clusters share some structural features such as the downward trend over the last 30 years, they differ widely in three highly visible characteristics: maximal observed fertility level, date of onset of sustained fertility decline, and fertility level in the most recent 1986–91 period. Figure 4 brings together the values of the child–woman index for all districts in each category. In spite of the groupings, a significant degree of heterogeneity remains within each fertility cluster. The smoothing procedure has not removed all traces of local differences. A few districts still display extreme levels of fertility or abrupt changes. Reasons for such fertility profiles are many,¹⁹ but these exceptional districts represent less than 5 percent of the sample.

Figure 5 provides a summary of our cluster analysis, with average values for each fertility profile and five-year period, while the clusters are mapped in Figure 6. Because some districts were excluded from the analysis for lack of consistent time series, the map in Figure 6 does not follow the customary administrative boundaries.

Using the summary offered by the average values of the clusters (shown in Figure 5), we can delineate the distinctive features of each profile using the highest recorded fertility as the most significant marker. The main traits of this demographic turning point consist of its date of occurrence and level. The first cluster is characterized by a low level of highest fertility, which (as is also true for most districts in this cluster) falls below the 1951–91 Indian fertility average. From 1956–61 on, the average of CWI values has always been below one. The first cluster is also characterized by its early attainment of maximum fertility (before 1956), and hence by early fertility decline (1951–56). In this cluster, fertility seems to have started declining from the first decade of observation. Indeed, it has not been possible to ascertain the period of the onset of fertility decline in these districts because the highest level of recorded fertility might have occurred before the 1950s. The overall picture is one of early fall coupled with low or moderate fertility.

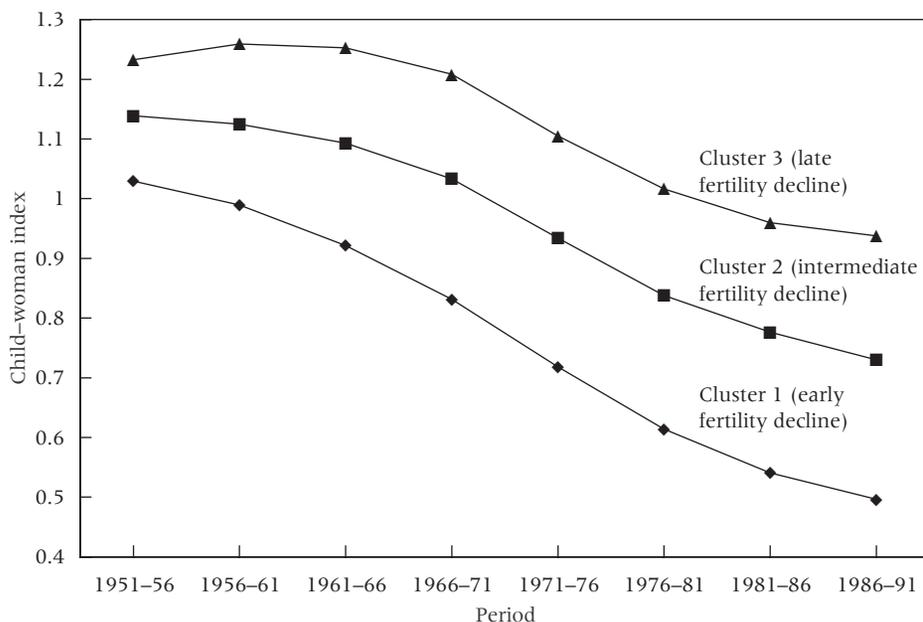
This first cluster includes a compact area covering most of Tamil Nadu and Kerala, as well as contiguous areas in coastal Andhra Pradesh and Karnataka. It also comprises several coastal areas in the west, covering Goa as well as patches in coastal Maharashtra. The only distinct region of early fertility decline to emerge in interior India is located in the northwest across Punjab, Jammu and Kashmir, Himachal Pradesh, and Uttar Pradesh. As ex-

FIGURE 4 Child-woman index, 1951-91 (districts classified by fertility profile)



pected, the first cluster includes all the forerunners of fertility decline that were identified on the previous set of fertility maps. Interestingly, this “pioneer cluster” includes few isolated areas other than Kolkata and Sambalpur district in Orissa.

FIGURE 5 Average child-woman index, all-India, 1951-91 (three fertility profiles)

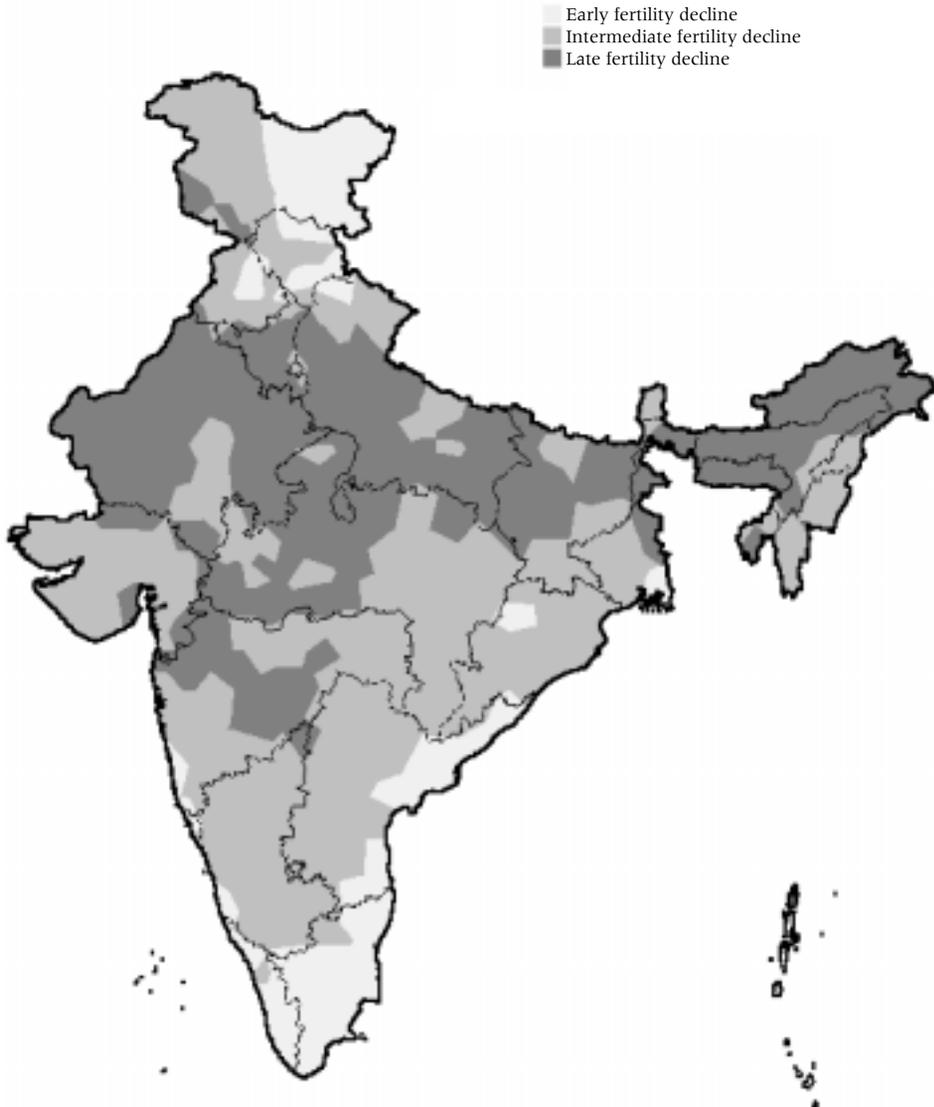


The second cluster brings together districts whose fertility profile runs almost parallel to that of the first cluster of early decliners. The major difference lies in the level of highest fertility as estimated from 1961 census data. The gap in terms of fertility levels between the two clusters is substantial and has persisted over the years, as cluster averages show. This gap corresponds roughly to a period of 10 to 15 years. The spatial distribution of these areas is clearly demarcated from that of the first cluster, with very few overlapping segments such as those in Punjab or Himachal Pradesh. These districts occupy a middle position, very close to the average Indian fertility profile.

The third cluster comprises the late decliners. It forms a large contiguous block, comprising the greater part of Bihar, Madhya Pradesh, Rajasthan, Uttar Pradesh, Haryana, and the northern tip of West Bengal, and the Brahmaputra valley including most of Assam, Meghalaya, and Arunachal Pradesh. It also includes pockets in Maharashtra and Karnataka. The third cluster is clearly separate from the first, with no common borders. Because this map involves no geographic smoothing, the fact that the resulting spatial patterning is so pronounced confirms that this striking feature of Indian fertility patterns is not a geostatistical artifact.²⁰

The initial rise in fertility, which was conspicuous until the 1960s, may be another distinctive feature of so-called late decliners: pretransitional fertility during the 1950s was characterized by a significant upward move-

FIGURE 6 Three profiles of fertility transition (results from cluster analysis) by district



ment, with an increase of more than 20 percent in some districts. When fertility reached its plateau at a very high level in this cluster, it was already declining in the rest of the country. The rapid decline during the 1970s may have been partly fueled by the new population policy during the Emergency; however, fertility reduction in the 1980s seems to have decelerated

substantially. Consequently, fertility levels in 1986–91 were much higher than elsewhere in India, and the gaps between fertility in the late-declining districts and fertility in other districts has consistently widened over time.

Fertility decline is a transformation affecting the social and economic structures of society down to the household level. The first phase of fertility transition is characterized by strong differentiation as some sections of the population opt progressively for new patterns of reproductive behavior, while the fertility regime remains stable in the rest of the society. In India, rising marital fertility during the 1950s and the early 1960s has undoubtedly affected a significant proportion of the districts and introduced an additional differentiating factor. Fertility in the 1970s and in the 1980s decreased, seemingly in a process of spatial (or horizontal) diffusion across all districts. The results from our cluster analysis show that the tempo of decline was faster among early decliners.

This suggests that fertility decline did not affect social structure uniformly and that vertical diffusion across local social groups was more pronounced among early decliners. In south India, data from the Sample Registration System and from the NFHS point to the rapid diffusion of fertility reduction within society in 1970–90. For instance, SRS estimates for 1971 and 1990 show that the rural–urban gap in fertility rates was reduced substantially in Tamil Nadu and disappeared in Kerala, while it actually increased in India during the same period.²¹ Similarly, NFHS data and census estimates indicate that fertility decline among illiterates has been more rapid in Kerala, Punjab, and Tamil Nadu than elsewhere in India.²² This narrowing gap between rural and urban areas and between illiterate and educated women accounts for the acceleration of fertility decline observed among early decliners.

Vertical diffusion has been less conspicuous among late decliners, and the variations across social groups increased significantly during the 1970s and the 1980s. Moreover, the spatial impact of rapidly declining fertility in Punjab and north Uttar Pradesh (Uttaranchal) on adjacent districts in Haryana or west Uttar Pradesh appears extremely limited.²³ This situation may result from the strong resistance of local institutions to the effect of social and economic changes witnessed locally and in nearby areas. They seem to be “locked-in” to a specific social and cultural configuration characterized by deeply ingrained patriarchal values that check social development.²⁴

Fertility in India and spatial autocorrelation

We now address an issue common to all map-based studies of social change. Our description of the geographical features of the spread of fertility decline in India has been on a stylized level, based on the visual impression derived from our cartographic rendering. Geostatistical tools permit quan-

titative assessment of these spatial traits. Surprisingly, such tools have seldom been employed by demographers and other social scientists to verify their findings based on impressionistic interpretation.²⁵ We now present the results of a simple analysis of spatial structure using our database. Instead of using smoothed data as in our first series of maps and in the cluster analysis, we revert to the original child–woman ratios. Thus, our analysis uses the entire district sample for 1961–91, even when some district units are not present in all Indian censuses because of the recurrent process of administrative redistricting.

The index we calculated from these data is Moran's *I*. It measures spatial autocorrelation, a concept closely related to that of autocorrelation used for time-series analysis (see Appendix for detail). Spatial correlation analysis aims at capturing the effect of distance on another variable of interest. In our case, we assess the covariance between district fertility levels measured by child–woman ratios and the geographic distances that separate districts from one another. Our hypothesis is that districts that are geographically closer to one another will display the most similar fertility values.

The result of our analysis is shown in Figure 7, which plots the degree of spatial autocorrelation (Moran's *I* coefficient) on the vertical axis against discrete categories of distance measured in kilometers on the horizontal axis. A value of 1 for the coefficient would indicate perfect positive correlation, 0 no correlation, and –1 perfect negative correlation.

In calculating the coefficients, we used the location of district headquarters for computing distances between districts. Coefficients pertaining to distances greater than 600 km are not shown in the figure as spatial correlation beyond this limit is invariably very low. We confine ourselves to a few comments in interpreting our calculations:

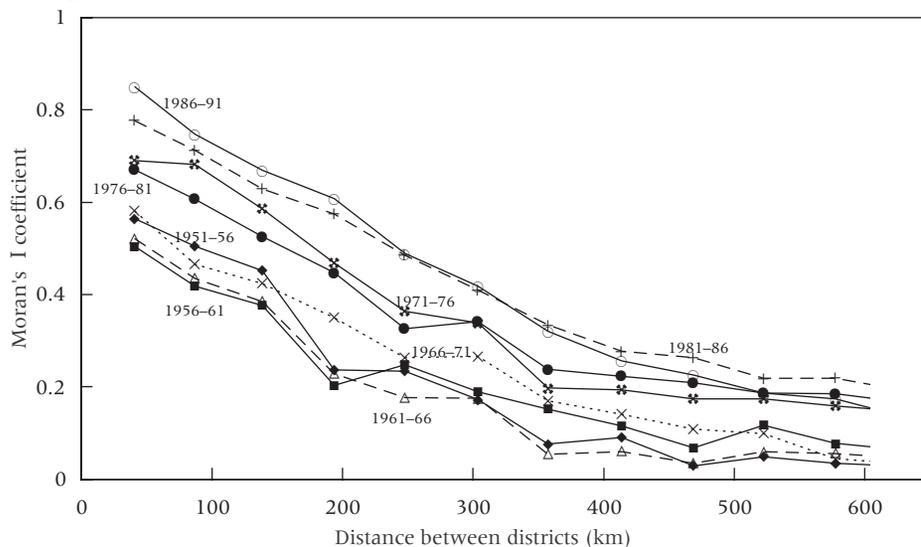
—As expected, spatial correlation decreases regularly as distance between districts increases.

—Spatial correlation coefficients are very high for short distances (above 0.5 for distances between districts of less than 50 km).

—Spatial correlation coefficients tend to increase regularly over the five-year periods shown.

These results help to confirm some of our previous descriptions of spatial patterns of Indian fertility. Spatial structuring has a strong influence on fertility levels and trends at the district level, and this “neighborhood effect” is still felt at distances greater than 300 km. Districts separated by greater distances display very low spatial correlation. On balance, the major finding of this analysis is that observed spatial correlation among fertility indexes increases regularly over the years. It moved from moderate values in the 1950s to very high values during the 1991 census. The coefficients attain their highest values during the latest reference period (1986–91).

**FIGURE 7 Moran's I coefficient for district fertility, 1951-91
(computed on child-woman ratios)**



The interpretation of this specific feature is crucial to our argument. If the increase in spatial autocorrelation coefficients is not spurious, then fertility decline has intensified the spatial structuring of fertility behavior in India. One might argue that this increase is due in part to factors such as improvements in the quality of the data. However, it is also reasonable to assume that age misstatement affects the quality of fertility estimates more than it affects the spatial distribution of errors since adjacent districts may be similarly affected by measurement errors. As a result, we can safely assert that the spatial features of fertility levels in Indian districts have become increasingly relevant as fertility transition has advanced.

The decline of fertility has been accompanied by intensified spatial patterns. If one assumes that fertility decline results from external structural changes, which rarely follow a distinct spatial pattern, one would expect spatial structuring to weaken during fertility transition. The evidence points to the reverse. This suggests diffusion of fertility behavior across adjacent areas independent of other factors.

Appendix: Statistical and geostatistical estimation procedures

Child–woman index: Mortality correction and standardization

Two different child–woman ratios are used:

$$\begin{aligned} \text{CWR}(0-4) &= \text{Children}(0-4) / \text{Women}(15-49) \\ \text{CWR}(5-9) &= \text{Children}(5-9) / \text{Women}(20-54) \end{aligned}$$

The first CWR is used for the quinquennium preceding each census, while the second CWR refers to the previous quinquennium. For instance, age data from the 1961 census provide CWRs referring to 1956–61 and 1951–56.

A major distortion in the use of these raw CWRs for estimating fertility trends arises from variations in infant and child mortality levels over time that affect differentially the surviving child population. As mortality is reduced, changes in raw CWRs reflect the joint effect of fertility and mortality changes.²⁶ The impact of mortality tends, however, to be relatively modest. A simple illustration might be useful in order to assess the impact of mortality variations on CWRs. Consider a stable population with a life expectancy of 55 years (West model) and a net reproduction rate of 2.24 (Coale and Demeny 1966). This approximates the average conditions in India during the period under study. Let each of the two types of CWRs for the reference stable population be equal to 100.0. Keeping the fertility level constant, we may compute CWRs for stable populations with different mortality levels. Table A-1 shows that in stable populations, a one-year increase in life expectancy would result in an increase of 0.5–0.6 percent in the two corresponding CWRs. A five-year increase in life expectancy, which is the average rate of increase of life expectancy in India between two successive censuses, would result in an increase of 2.4 percent (CWR 0–4) and 2.9 percent (CWR 5–9).

This illustration shows that the impact of mortality on CWRs is limited. When comparison is restricted to a single intercensal interval period or to geographically neighboring areas, mortality seems to have a moderate impact on CWRs. However, mortality variations between populations over a 30-year period or between regions characterized by marked mortality differentials (such as low-mortality

Table A-1 Illustration of the effect of the level of mortality on child–woman ratios

	Life expectancy at birth				
	50	54	55	56	60
CWR(0–4) ^a	96.7	99.3	100.0	100.5	102.4
CWR (5–9) ^b	95.6	99.1	100.0	100.6	102.9

^a Children aged 0–4 divided by women aged 15–49

^b Children aged 5–9 divided by women aged 20–54

NOTES: Child–woman ratios shown in the table are computed using a stable population with various specified mortality levels (West model, female), and with fixed net reproduction rates of 2.24. The ratios are scaled by equating the values at e₀=55 to 100.

Kerala and high-mortality Uttar Pradesh) may have more serious consequences on fertility estimations, and it would be unwise to disregard mortality differentials altogether.

In order to correct for mortality change, we calculated a corrected set of CWRs by dividing the raw CWRs by the appropriate survival rate from birth to the corresponding age group. For example, the corrected CWR(5–9) is computed by dividing by $L_{5-9} / 5$, where L_{5-9} is taken from West model life tables of an appropriately chosen mortality model. Mortality estimates for 1970 and later dates were derived from the Sample Registration System, which has provided reliable life tables for Indian states since the 1970s.²⁷ For previous periods, we used estimates derived by Bhat from the census. We combined SRS and pre-SRS estimates of life expectancy for both sexes by fitting a trend line from 1951–61 to 1992–96 for each state.²⁸

State-level life expectancy estimates are shown in Table A-2.²⁹ Examination based on a Lexis graph shows the reference years for mortality-corrected CWRs to be 1.25 years before the census year for the age group 0–4 years and 3.75 years before the census year for the age group 5–9 years.

A further difficulty is that the two types of CWRs are not exactly comparable (see also Figure 2). For example, the average values for CWR(0–4) and CWR(5–9) are 0.727 and 0.917 after mortality correction. The gap between the two types of CWRs is linked to different factors such as the specific denominator values (females aged 15–49 and 20–54 respectively) and the differential quality of age enumeration among the 0–4 and the 5–9 age groups in India. This last factor is especially important, because the proportion of children below age 5 years is known to be systematically underestimated while the population aged 5–9 years is overestimated.

TABLE A-2 Estimates of life expectancy at birth for India and selected states, 1951–90

	1957	1960	1967	1970	1977	1980	1987	1990
India	41.4	42.8	46.8	48.2	52.2	53.6	57.6	59.0
Andhra Pradesh	37.6	39.3	44.5	46.3	51.5	53.2	58.4	60.1
Assam	37.5	38.8	42.8	44.1	48.1	49.4	53.3	54.7
Bihar	38.7	40.0	44.1	45.5	49.6	51.0	55.1	56.5
Gujarat	41.5	42.9	46.9	48.3	52.4	53.7	57.8	59.1
Haryana	44.0	45.4	49.7	51.1	55.3	56.7	61.0	62.4
Karnataka	39.7	41.4	46.6	48.3	53.5	55.3	60.4	62.2
Kerala	48.8	50.5	55.7	57.5	62.7	64.4	69.6	71.4
Madhya Pradesh	37.4	38.6	42.4	43.7	47.5	48.8	52.6	53.8
Maharashtra	40.3	42.1	47.4	49.2	54.5	56.3	61.7	63.5
Orissa	38.1	39.4	43.2	44.5	48.4	49.7	53.5	54.8
Punjab	47.6	49.0	53.2	54.6	58.9	60.3	64.5	65.9
Rajasthan	39.6	40.9	44.9	46.3	50.3	51.6	55.7	57.0
Tamil Nadu	38.7	40.6	45.7	47.4	52.5	54.3	59.4	61.1
Uttar Pradesh	31.6	33.4	38.6	40.3	45.5	47.3	52.5	54.2
West Bengal	37.4	39.1	44.5	46.2	51.5	53.3	58.6	60.4

NOTES: The states for which estimates are shown contain 95.8 percent of the population of India according to the census of 1991.

SOURCES: The estimated values are computed from trend lines based on estimates from Bhat (1987) and Registrar General of India (1999).

The method proposed here relies on direct standardization of the mortality-corrected CWRs and on limited smoothing.³⁰ Standardization is done independently for each CWR using the grand average of the mortality-corrected CWR for all available values (districts for all censuses). Because of this standardization, CWR is now an index centered on 1.

Standardized CWR = mortality-adjusted CWR / average₁₉₆₁₋₉₁ (mortality-adjusted CWR). Smoothing of the standardized CWR values is then performed by a moving-average technique using weights 1/4, 1/2, and 1/4.³¹

$$\text{CWR}(t) = [\text{CWR}(t-5) + 2 \times \text{CWR}(t) + \text{CWR}(t+5)] / 4$$

District units that have appeared (or disappeared) during the 1951–91 period had to be excluded from our sample since smoothing on a limited set of values was likely to oversimplify fertility trends during the period under study. We have kept only districts present during at least three consecutive censuses. Districts that have changed names or lost territories (to newly formed district units), however, have been retained. This procedure yielded data for 338 districts, while the total number of districts in our database increased steadily from 317 in 1961 to 450 in 1991.

We call the resulting mortality-adjusted, standardized, and smoothed fertility index the child–woman index (or CWI). The CWI estimates provide the first-ever continuous series of a fertility index for Indian districts for the period 1951–91. These are available from the authors upon request. This index could be further improved through more precise mortality corrections,³² but our experimentation with various correction techniques indicates that further refinements are unlikely to yield significantly improved estimates of changes and differentials in district-level fertility.

Geostatistical procedures: Kriging and spatial autocorrelation

In this article we used a standard geostatistical technique called kriging to interpolate a continuous surface (India) from a sample of observations (a fertility index estimated for district headquarters). The method was developed by D. G. Krige and Georges Matheron in the 1960s and is described in detail in Bailey and Gatrell (1995) and in Haining (1990). A kriged estimate is a weighted linear average of the known sample values around the point to be estimated. In our case, we aggregated districts whose headquarters was less than 20 km distant (which is also the size of our grid). Because our geographical coordinates correspond to district headquarters and not to their geometrical centers, this aggregation has proved very useful; in some cases such as the Kolkata region, district headquarters can be in close proximity while corresponding districts are comparatively distant. Because of our smoothing, the observed semivariance for the smallest distance (less than 50 km) is almost zero and kriging acts as an exact estimator.

The method used in this article (ordinary kriging) assumes that the data have not only a stationary (or constant) variance but also a non-stationary mean value within the search radius limited to the 20 nearest districts. This method does not allow for the estimation of local values in edge areas situated beyond locations for

which CWI values are available. For this reason no estimate is available for some border areas such as North Kashmir and West Gujarat.³³

Spatial autocorrelation describes how an attribute such as fertility levels is distributed over space and to what extent the value observed in one zone depends on the values in neighboring zones. In this article, spatial correlation is computed with Moran's *I* coefficient.³⁴ This coefficient is based on correlograms, that is, graphs of spatial autocorrelation (y-axis) between pairs of observations classified by distance (x-axis). Moran's *I* coefficient is a standard measure of spatial autocorrelation, roughly analogous to the correlation coefficient used for ordinary regression analysis. For a given distance, the Moran coefficient of spatial autocorrelation is computed for a variable *z*:

$$I = \frac{\sum_{i,j} (z_i - \bar{z})(z_j - \bar{z})}{n \sum (z_i - \bar{z})^2}$$

for *n* pairs of locations *i* and *j* such as distance (*i, j*) = *h*

When the Moran coefficient is computed for a variety of distances *h*, we get a correlogram showing the trend in spatial autocorrelation with respect to distance, with *I* = 1 when the correlation is perfect between observations. In Figure 7, the average distance between pairs of observations is used to plot spatial autocorrelation of raw CWRs.

Notes

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1 On European and Soviet fertility decline, see Coale and Watkins (1986) and Jones and Grupp (1987).

2 For recent explanatory models of Indian fertility down to the district level, see Malhotra, Vanneman, and Kishor (1995); Murthi, Guio,

and Drèze (1995) for 1981 data and Bhat (1996) for 1991 data.

3 Matheron (1970) pioneered the concept of regionalized variables. See also Houling (2000).

4 On Kerala, see Krishnan (1976); Zachariah (1984); Bhat and Irudaya Rajan (1990); Zachariah and Irudaya Rajan (1997); Nair (1974).

5 See Savitri (1994); Srinivasan (1995); Kishor (1994); Guilmo and Irudaya Rajan (1998).

6 See, for example, Dyson and Moore (1983); Malhotra, Vanneman, and Kishor (1995).

7 See Kishor (1991); Malhotra, Vanneman and Kishor (1995); Murthi, Guio, and Drèze (1995); Drèze and Murthi (2001).

8 See Registrar General of India (1997); Bhat (1996); Irudaya Rajan and Mohanachandran (1998).

9 We have restricted ourselves to the years 1961–91 for several reasons. First, the 1951 administrative units, which followed closely the boundaries of British and Princely India, underwent radical changes during the 1950s. Moreover, 1951 data are incomplete and are not in all cases readily available for five-year age groups. Furthermore, the 1951 age distributions reflect fertility changes during the 1940s, a period that witnessed large-scale mortality crises in India (the Bengal famine among them). Fertility variations derived from the 1951 census are driven more by crisis and post-crisis recovery than by secular change and spatial heterogeneity.

10 This measure is computed by dividing the number of children under age 5 by the number of women between ages 15 and 49. By analogy, we can compute the ratio of children 5 to 9 to women 20 to 54. Both numerators and denominators are taken from census enumeration.

11 This method has often been applied to generate small-area fertility indexes in contexts where age distributions from regular censuses are available, but where births are not properly recorded. Child–woman measurements are more closely analogous to the general fertility rate (births per women of childbearing age) than to total fertility rates.

12 We have assessed the quality of age data and of the child–woman ratio, using the raw as well as the corrected (smoothed) age distributions for the four censuses, 1961 through 1991. The error on account of age misstatement is here computed as the relative difference between the CWRs calculated from raw data and the CWRs calculated from corrected data. For the CWR computed as children(0–4) / women(15–49), the percent of error stood at 9 percent in 1961, fell to 2 percent in 1971, and hovered around 5 percent between 1981 and 1991. However, for the CWR computed as children(5–9) / women(20–54), the picture is quite different. The percent error was 6 in 1961, increased to 14 in 1971, and fell markedly to 3 in 1981 and to less than 1 percent in 1991.

13 For reasons explained in the Appendix, we excluded some district units with incomplete data series.

14 One of the few studies on this period is Anderson (1974). See also Chakraborty (1978).

15 A more detailed mapping of fertility during the 1960s would show Coimbatore and Madras regions in Tamil Nadu, as well as Alappuzha in Kerala, to be the forerunners of this decline. Although these areas are not far apart, they nevertheless belong to different states and are separated by several districts.

16 For historical reasons, coastal areas in India have long been especially permeable to external influences. They constitute peripheral areas, very distinct from the central core of India. See Sopher (1980).

17 About the Khanna study, see Wyon and Gordon (1971) and Mamdani (1972). Das Gupta (1995) stated that fertility began declining much earlier in several parts of the Punjab, although our estimates do not support this early decline.

18 We have used here a procedure known as the k-means method, which minimizes the within-group sum of squares in each cluster (see Bailey and Gatrell 1995).

19 Reasons for such erratic fertility profiles may include actual demographic conditions, changes in district boundaries, or an especially poor enumeration record.

20 The pattern contrasts with the much more fragmented map of estimated dates of fertility decline in Europe (Coale and Watkins 1986: map 2.1).

21 In 1971–73, rural fertility rates were respectively 13.8 percent, 31 percent, and 38 percent higher than urban rates in Kerala, Punjab, and Tamil Nadu, as against 32.5 percent in India as a whole. In 1989–91, the rural–urban gap decreased to 18 percent, 0 percent, and 20 percent in Kerala, Punjab, and Tamil Nadu, while it increased to 48 percent in India as a whole. SRS data are from the compendium published by the Registrar General of India (1999).

22 See estimates by Bhat (2000). On fertility decline among illiterates, see also Arokiasamy, Cassen, and McNay (2001).

23 In spatial analysis, this situation corresponds to the existence of “barriers.” For a classic study of spatial diffusion, see Cliff et al. (1981).

24 One of the highest-fertility spots (in Uttar Pradesh) is depicted in Jeffery and Jeffery (1997). For a recent example of path dependency analysis applied to birth control history, see Potter (1999).

25 For some applications, see Bocquet-Appel, Courgeau, and Pumain (1996) and Airlinghaus (1996). See also Fotheringham and Rogerson (1994).

26 Because mortality rates are much lower among women, we believe that inter-district variations of female adult mortality rates are unlikely to disturb the values for the denominator.

27 Some district-level mortality estimates are available from the 1981 and 1991 censuses (Registrar General of India 1989, 1997). These district mortality indicators are based on indirect estimation using the proportion of surviving children. Because of discrepancies between 1981 and 1991 estimates and between these sources and regional SRS estimates, we considered it unwise to use these estimates to compute life expectancy values for 1951–91.

28 Life expectancy estimates for 1951–61 and 1961–71 are found in Bhat (1987). SRS estimates for 1970–75, 1976–80, 1981–85, 1986–90, 1991–95, and 1992–96 are from Registrar General of India (1999). For census estimates prior to 1971, see also Agarwala (1985).

29 For states for which no mortality estimate is available, we used mortality levels of the closest state or the all-India level. Tamil Nadu values are applied to Pondicherry, all-India averages to northeast states, and so on.

30 For another application of the method to Indian historical data, see Guilmoto (1992: 76).

31 To smooth extreme values for 1951–55 and 1986–90, we applied the average smoothing factor as obtained respectively for CWR2 and CWR1.

32 To name a few possible refinements: correcting for adult mortality, using different model life tables, accounting for different mean age at childbearing.

33 Because the census was not held in Kashmir in 1991 owing to political turmoil, the area with no estimate is even larger in the two maps shown for the 1980s.

34 On spatial autocorrelation, see Bailey and Gatrell (1995) and Fotheringham, Brundson, and Charlton (2000).

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On Population and Resources: A Comment

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IN A RECENT ARTICLE in this journal Partha Dasgupta concludes: “Judging by level of analysis, most of those who have been investigating economic growth, poverty, environmental stress, and fertility behavior have not read widely beyond their particular fields of interest” (2000: 644–645). Unfortunately, Dasgupta has not referenced widely even in areas that one might assume were in his fields of interest.

Let me note one example. Dasgupta mentions (p. 648) only two of the many statistical estimates of the relationships between population and economic growth that were made following the 1986 report of the US National Academy of Sciences on population and economic growth. While these two studies showed a negative relationship, other studies found a variety of outcomes: no statistically significant effect of population growth on economic growth, occasionally a positive effect, and also occasionally a negative effect (Levine and Renelt 1992; Kelley and Schmidt 1996; Barro 1997; Burkett, Humblet, and Putterman 1999; Johnson 1994). The regression analyses have not produced the uniform result Dasgupta implies.

I am among those whose analyses of the interrelationships between population and resources Dasgupta criticizes for being overly optimistic. My presidential address to the American Economic Association, “Population, food and knowledge” (Johnson 2000), and my role in the 1986 NAS report for which I served as co-chair were both noted critically. Thus I have been motivated to respond to some of his inferences and conclusions.

Is disinvestment common?

An important element in the relationship between population and economic growth is the rate of investment. The usual measures of investment in na-

tional accounts do not take into consideration depletion of natural resources or additions to human capital in the form of education. Dasgupta refers to Hamilton and Clemens's (1999) estimates of "genuine investment and capital deepening" for many countries. Dasgupta summarizes these estimates for five countries—Bangladesh, India, Nepal, Pakistan, and China—and for sub-Saharan Africa. He notes that two of the countries and sub-Saharan Africa had negative rates of "genuine investment" for the period 1970 to 1993. But comparisons of this nature can be misleading. The total population of developing countries with negative investment rates, shown in his Table 4, was 777 million in 1998, while the developing countries with positive investment rates had a combined population of 2,350 million, or three times as many. Moreover, the data in Hamilton and Clemens (1999) do not indicate that sub-Saharan Africa, with a population of 628 million, had a negative rate of investment—they show a small positive rate for the period.¹ After making the assumption that the growth of per capita wealth was proportional to the growth of per capita GNP, Dasgupta states that per capita wealth declined in four of the five countries and in sub-Saharan Africa. From the data in his Table 4, however, this conclusion is in error—in all five Asian countries per capita wealth increased, although it did decline in sub-Saharan Africa.²

While the effort to provide a fuller estimate of investment than the usual national income accounts data is laudable, an estimate that ignores a major change that has occurred over the past half-century in developing countries may be misleading as well. It seems odd that an effort to measure "what really happened" neglects a major form of capital accumulation in developing countries—namely, the increase in human capital attributable to the increase in life expectancy. This gain is over and above the increase in education as a measure of human capital formation. For most developing countries, the 1970–93 period saw significant increases in both life expectancy and years of education attained. For example, the increase in life expectancy in 28 low-income countries between 1960 and 1996 was 20 years: from 44 to 64 (World Bank 1984: 262; 1998: 12–14, 104–107).³ The percentage of time that people spent in their productive years increased significantly; the percentage of children in the population declined; and the percentage who were elderly had not yet increased significantly. Should this not be included in the measurement of capital if one wants to measure the productive capacity or wealth of a country? The measure of human capital Hamilton and Clemens used was the annual expenditure on education, probably only by governments. The measure does not include the time input of students and thus is an underestimate of the actual investment in education, although probably not much of an underestimate since years of education in most developing countries are limited.

Increased life expectancy and population growth

Of course, the gain in life expectancy that occurred in the developing countries increased the population, which Dasgupta apparently considers to be a negative factor in the development of nations. Would he argue that, had life expectancy in the developing countries remained constant at the level of 1965 or 1970, well-being would have been improved? I hope not. The population growth of the last half-century was caused by mortality decline, not by increased fertility. The rapid population growth in the developing countries occurred because while fertility fell, mortality declined even more rapidly. In sub-Saharan Africa, where there has been little real per capita income growth and modest improvement in the education of females, the fertility rate, according Dasgupta's Table 3, declined from an average of 6.6 in 1980 to 5.4 in 1998. This is a substantial change in less than two decades.

Limited female education and fertility

I find it odd that Dasgupta gives so much attention to a few countries in Africa where females with limited years of primary schooling have higher fertility than females who have no schooling. The argument seems to be that we cannot be sure of the causality between schooling and fertility. Achieving one to four years of primary schooling adds little to the human capital of individuals; many in this group may not be functionally literate. If we wish to pull people out of poverty, the case can be made that one of the most appropriate ways is to increase the level of education, which is in fact happening in sub-Saharan Africa (Sender 1999: 94).⁴

The relationship between primary education and fertility may have already changed. Data for 13 sub-Saharan countries for 1990–94 give fertility rates for women with no schooling and primary and secondary schooling (Kremer and Chen 2000). In only two of these countries was fertility higher for women who had primary education than for those who had none. This does not directly contradict the earlier data that found that in some countries the fertility rate for women with one to four years of schooling was greater than for women with no schooling. What these data show is that in 1990–94 in 11 out of the 13 countries women with any level of primary schooling had lower fertility than women with no education. The two countries where women with primary education had higher fertility than women with no education had a combined population of 17 million, a small percentage of the more than 600 million total population for the region. As noted by Dasgupta, the total fertility rate in sub-Saharan Africa declined by 18 percent between 1980 and 1998. Thus even if in some countries women with one to four years of primary education have more chil-

dren than women with no education, this phenomenon has not halted a significant decline in the total fertility rate.

Why recent history is relevant

Dasgupta faults those of us who have optimistic views of the future for relying on the experience of only two centuries of growth, following at least 5,000 years when growth was modest at best. We are in agreement that up to 1750 there had been little improvement in the conditions of life in Europe and Asia. But in making this point, Dasgupta undermines his case that population growth works against the reduction of poverty. By today's standards nearly all of the world's population in 1750 lived in poverty. Most of the people now classified as poor have a much higher level of living than the average individual who lived in Europe in 1750. Life expectancy in the 28 low-income countries in 1996 was 64 years, as noted earlier, roughly double what prevailed in Europe in 1750 and nearly three times what it was in India at the beginning of the twentieth century (Bogue 1969: 572). This great improvement occurred while the world's population increased sevenfold.

Dasgupta faults those who believe that population size has no adverse effect on economic well-being: such optimists assume instead that future innovations will "make no more than a finite additional demand on the natural-resource base" (p. 645). What he apparently has in mind is how the demand for natural resources will change as further economic growth occurs and new products emerge. Obviously we do not know what new products will emerge or we would invent them now. But we do know that in many ways the natural-resource intensity of today's output is significantly less than it was a century ago and far less than it was five centuries ago. I grant that, because total output has increased so much, the fact that the natural-resource intensity has declined does not tell us what change has occurred in the total demand for natural resources. But if we should run short of coal, oil, and natural gas or need to reduce their use to reduce global warming, we have a source of energy—nuclear—that requires no significant amount of natural resources.

We know as well that with an approximate doubling of the world's population and increased per capita consumption of agricultural products, the total land cultivated for crops in the world has not increased since 1967 and over that period of time the real price of grains in international markets has fallen by 40 percent. Such a development could not have been imagined when Ricardo and Malthus wrote, when natural-resource constraints were a major factor limiting national output; in fact, it could hardly have been imagined as recently as 50 years ago. For example, agricultural land in the United States now contributes less than 0.5 percent of GDP.

Even adding in the cost of all fertilizers and pesticides as natural-resource inputs, the share of natural resources used in agriculture remains at less than one percent of the national GDP.

Why can one have a high degree of confidence that economic growth can continue with increased population? I believe the reason is knowledge—the world has shown an amazing capacity to advance knowledge over the past two centuries compared to all previous time. There are two reasons for this. First, there are more people and thus there are more people who can contribute to the creation of new knowledge (Kremer 1993). Second, we now have people who specialize in the creation of knowledge—we have research institutes and universities, institutions that hardly existed more than a century and a half ago (Johnson 2000). A single American state university now has more faculty members than all colleges and universities in Germany, then the world's center for graduate study and research, had in 1900 (Johnson 2000). As the world's population and income have grown rapidly over the past two centuries, the economic gains from new knowledge have increased and the number of persons engaged in advancing knowledge has increased in response.

Knowledge increased dramatically in the twentieth century. Is there any reason to doubt whether the rate of flow of knowledge will be maintained? In fact, it is highly probable that knowledge will grow at a faster rate in the future as the share of the world's resources devoted to the creation of knowledge continues to increase as a consequence both of the efforts of developing countries to establish and expand their own research universities and institutes and of the continued growth of population, until population stabilizes before the end of the twenty-first century.

We also have the record of the past century of a relative decline in the resource-intensive sectors—agriculture, mining, and manufacturing—and a sharp increase in services that rely far less on natural resources. More significant than anything else, we have seen a decline in the real prices of resource-intensive products, such as oil and steel.

Some other points

Let me briefly note a few other points Dasgupta makes that I do not think the evidence supports. On page 663 he argues that "...[i]ncreased population size implies greater crowding..." Then he argues that crowded centers of population "provide a fertile ground for the spread of pathogens..." The implication is that in developing countries cities are less healthy than rural areas, but this seems not to be the case. Brockerhoff and Brennan (1998) show that infant mortality rates were lower in large cities in the 1990s than in rural areas (p. 91). And there is evidence, both current and past, that population density has a positive effect on the level of per capita income

(Burkett, Humblet, and Putterman 1999). Dasgupta presents no evidence to support either of his propositions. A striking contradiction of these two presumed facts is found in China, where (a) the highest-income area is the most densely populated (the coastal region) and the lowest-income area is the least densely populated (the western region) and (b) life expectancy is greater in urban than in rural areas.

Dasgupta emphasizes the possible relationship between fertility and the fact that parents do not bear the full cost of rearing their children, either because society pays a significant part of the cost (free public education, subsidized health care) or other family members do. While these considerations may influence fertility decisions, it remains true that as society has borne larger shares of the costs of children, fertility has fallen. In part, this is because education, especially secondary education, has direct effects on reducing fertility. But it may also be that as real per capita incomes increase, society bears an increasing share of the costs of rearing children while there are offsetting effects of higher incomes on the number of children desired. In any case, there is no direct evidence that this kind of externality has had a positive effect on fertility; a theory can be created that says this, but a theory could also be constructed to show a contrary effect. All that is required is the appropriate assumptions in the model. Any such model needs to be tested against actual experience, and this Dasgupta does not do.

I find it difficult to accept the extreme pessimism that is displayed in Dasgupta's section "Household labor needs and the local commons" (pp. 671–674). He argues that there may be communities in which there is no way out of poverty because circumstances associated with poverty induce either higher fertility or an increase in the number of children as infant mortality declines: "[A]long this pathway poverty, household size, and environmental degradation would reinforce one another in an escalating spiral" (p. 674). Here, as elsewhere throughout his article, there is no indication of how common such communities are: are they isolated examples or do they represent the majority of poor communities in the world? If it were true that the majority of the communities now in poverty were as poor as or poorer than such communities were 50 years ago, then the arguments presented might warrant great concern. But this is not the case. If one considers the poorest countries in the world, numerous measures of well-being have improved over the past half-century—life expectancy, infant mortality, per capita food supply, percent of the population malnourished, availability of education, and decline in the percent illiterate. Even in sub-Saharan Africa, where measured per capita incomes have fallen in many countries since the 1970s, life expectancy, education, and literacy have improved. Consequently, if some communities have either seen no improvement or have declined in welfare as a result of population growth, they must have been a significant minority of the total population. Do such communities exist? Are there areas in India with life expectancy of 23 years as

was the case in 1900, or 32 years as in the 1940s (Bogue 1969: 572)? Dasgupta provides no evidence that such communities exist.

Concluding comments

There is much one can readily agree with in Dasgupta's analysis of factors that have reduced fertility and will continue to do so. While there may be a small number of countries where women's attainment of a very limited amount of education does not reduce their fertility, no evidence contradicts the conclusion that education beyond the primary level for women is associated with lower fertility. The reasons for this are several and are not in doubt: education through the secondary level delays marriage and the age at which the first child is born; education increases the value of a woman's time and the cost of a child; and education is generally associated with the degree of autonomy accorded to a woman. Dasgupta's Table 7 reports that fertility declines as women's share in paid employment increases; this relationship is similar to what would have been obtained had the share of the population in urban areas been the variable.

Dasgupta notes the effects of property rights and institutional changes on fertility, and properly so. In some, probably a majority, of the villages in China, government policy has reallocated the rights to use farm land in response to demographic change. Thus while an explicit policy restricted population growth, another policy that favored large families existed as well. But all such policies need to be evaluated in the context of the totality of factors that affect fertility. And we know that for developing countries over the past several decades, the totality of factors have resulted in declines in fertility, not in a few instances but everywhere at the national level.⁵

Notes

1 In Table 4 Dasgupta presents estimates of the ratio of investment to GNP for five developing countries and sub-Saharan Africa. He says the data were derived from Table 3 in Hamilton and Clemens (1999) but there is no summary for sub-Saharan Africa in Table 3; the data for that region apparently came from their Table 2. I had difficulty duplicating the estimates that he presents of the ratio of genuine investment to GNP for 1970–93. The differences for the five countries are small—his estimate for China is 10.0 percent while from the same data I derive an estimate of 11.3 percent. But for sub-Saharan Africa he states that the investment ratio is a negative 2.8 percent. The raw data in the source are as follows:

1970–79, +7.3; 1980–89, –3.2; 1990, –3.8; 1991, –1.2; 1992, –0.6; and 1993, –1.1. I am assuming that he arrived at his figure by averaging the data for the 24 years. When I average these estimates, the mean is +1.4 percent, not –2.8 percent. This positive rate is very low, but it does not imply disinvestment. Note that if the period 1970–79 included growth for nine rather than ten years, the average investment rate is reduced to 1.2, but is still positive.

2 Table 4 includes estimates of the average annual percentage rate of change in per capita GNP (fourth column). Dasgupta does not have an estimate of per capita wealth but estimates its growth by assuming an output–wealth ratio of 0.25—wealth

per capita is four times GNP per capita. This assumption means that per capita wealth would increase at the same annual rate as per capita GNP: if the output-to-wealth ratio is constant, there can be no other result. Since the annual rate of growth of per capita GNP is positive for the five countries in the table, the annual rate of growth of per capita wealth must also be positive if his assumption about the relationship between output and wealth is accepted. In sub-Saharan Africa the rate of GNP growth is negative and if one accepts his assumption about the relationship between income and wealth, the growth of per capita wealth would also have declined by 0.2 percent annually. It is difficult to understand how he obtained his result that per capita wealth declined in each of the countries except for China.

3 Two adjustments were made to the 1960 data. First, the World Bank listed 34 countries as low income; however, data were not available for six countries in 1996 and these countries were deleted from the list. These countries accounted for only 2.8 percent of the population of the 34 countries listed as low income. Second, the life expectancy for China for 1960 was given as 41. This

life expectancy was low as a consequence of the famine for the period 1959–61. The life expectancy used for China to derive the average life expectancy for 1960 was 47.

4 Sender (1999) presents data on the increase in female enrollment in primary schools for 16 sub-Saharan African countries between 1960 and 1990–94. The unweighted average of the percent of females enrolled in primary school increased from 28 percent in 1960 to 78 percent in 1990–94. The increase in gross enrollment in secondary school ranged from 2 percent to 17.5 percent for approximately the same period of time. These data indicate that the percent of females who receive only one to four years of primary schooling decreased significantly.

5 According to World Bank estimates of fertility (2000: 286–287), every country out of more than 100 that had a fertility rate in excess of the replacement rate in 1980 experienced a decline in fertility between 1980 and 1998. Consequently, of all the factors that affect fertility, both positively and negatively, those that had a negative effect dominated.

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On Population and Resources: Reply

PARTHA DASGUPTA

I HAVE ADMIRED Professor Johnson for so many years that, for me, it is an honor even to be chastised by him for what he sees as the errors of my ways. On studying his comment, however, I can only think that while Johnson is an outstanding economics writer, he is a less than outstanding reader.

Consider first the admonishment with which he opens his comment, that I cannot have read widely on statistical estimates of the relationships between population and economic growth, because the regression analyses have not produced the uniform result that he says I took the literature to imply. In point of fact, I drew the very conclusion he says he himself has drawn from that literature, namely “cross-country regressions in which population is a determining factor [of economic growth] have given us mixed messages” (Dasgupta 2000: 648).

Why recent history is relevant

It is in part because the estimates in question have given us mixed messages that I explored the links between population, poverty, and the natural-resource base by pursuing a very different route from the one offered by cross-country regression analysis. Moreover, it should be more than a mere puzzle to readers of my article that Johnson thinks I need to be reminded of the relevance of recent history for understanding the links that I explored. He recounts aggregate statistics from recent history—on life expectancy, agricultural output, and gross national product—statistics that I clearly acknowledged in my article, but showed to be wanting in their scope for summarizing the human experience in South Asia and sub-Saharan Africa. These two regions have continued to house the bulk of the world’s poorest people, with a tenacity that should surprise anyone who had expected wealth in growing economies to trickle down faster than it appears to have done since World War II (Dasgupta 1993; World Bank 2000). There is no suggestion in Johnson’s comment, though, that he is willing to con-

sider the possibility that we are in need of a reading that is wider, finer, and deeper than is obtainable from a perusal of cross-country data on indexes of current well-being (e.g., life expectancy at birth and GNP per head) and from the oft-repeated observation that education and knowledge creation can be relied upon to counter the human problems I was addressing.¹

In the long run education and knowledge creation may well ease all such problems. Death, in any case, is a great leveler of statistics, in that past anguish is not discernible in data on current well-being. My article, however, was not an exercise in futurology involving global aggregates of current well-being indicators. It was instead prompted by a number of messages that evidence from the recent past seemed to be giving me, messages that I summarized at the beginning of my article. In response, I not only explored the possibility that there are spatially localized poverty traps, but I also explored the thesis that high fertility and degradation of the local natural-resource base are linked to the poverty that is experienced by those who are caught in such traps. In my models none of the three spatially localized variables (population, poverty, and the natural-resource base) is a prior cause of the other two; rather, each influences, and is in turn influenced by, the other two.

The reasoning deployed in the models was based on externalities, in both reproductive behavior and the use of the local natural-resource base (hence the subtitle of my article). Now, externalities are a symptom of institutional failure. And although there can be no doubt that institutional failure is frequently traceable to corrupt governments and inept economic policies, the data I had collated made better sense to me only when I was able to identify additional sources of externalities, such as those operating through social practices involving nonmarket exchange. I did not present an overarching model, but instead offered a variety of models, each focusing on a given type of externality and each making sense of some aspects of the evidence I had compiled. However, even though the models I constructed are formulations of different pathways, their structures share a core commonality, one that is becoming increasingly familiar from the theory of locally interacting systems (Dasgupta 2000: 653).

Of course, Johnson may not like this sort of reasoning; he may even dislike my appeal to a heterodox body of evidence and my reliance on a collection of models rather than a single aggregate model of the kind made familiar by old and new growth theories. But I cannot understand how he thinks he can counter my arguments by rehearsing trends that have been detected in world-aggregate data as early as some one million years ago (see his reference to Kremer 1993).

Kremer's excellent article is of interest to the present exchange because it highlights differences in the economic models that Johnson and I believe should be deployed for understanding recent history in the poorest

regions of the world. Kremer's model embeds a supposedly positive link between the creation of ideas (technological progress) and population growth in a world where the natural-resource base consists of a fixed, indestructible factor of production. In contrast, I was concerned to include degradable resources, especially local ecosystems (e.g., soil, forests, fisheries, and sources of fresh water). In the sort of models I studied, biases in the direction of technological progress would mean that new ideas are not able to offer adequate substitutes for local resource bases in a world where out-migration is frequently not a viable option to an often intolerable rural situation.²

Is disinvestment common?

I am grateful to Johnson for pointing out the computational error in Table 4 of my article and in the claim I made that "genuine investment" (investment in all forms of capital assets, including natural capital) in sub-Saharan Africa was on average negative during 1970–93. From the data in Hamilton and Clemens (1999), I mistakenly estimated the share of genuine investment in GNP to have been on average –2.8 percent in sub-Saharan Africa, whereas the figure should have been 1.4 percent. But aggregate investment, even if it is aggregate genuine investment, on its own tells us little: it should be adjusted for population change if we seek an understanding of movements in per capita wealth. Given that population in sub-Saharan Africa grew at 2.7 percent per year, I now calculate that wealth per head there declined, at the very least at 2.3 percent per year, not at the 3.4 percent I had mistakenly reported.

Johnson misunderstands the basis of my calculations for arriving at the third column of Table 4. I used 0.25 per year as the output–wealth ratio to convert figures for (genuine) investment–output ratios to the corresponding figures for rates of change in per capita wealth, and remarked that 0.25 per year is in all probability an overestimate. Johnson thinks that I must have assumed the output–wealth ratio to have remained constant during the entire period in question (see endnote 2 of his comment). Rightly, he thinks that if I had made the assumption he takes me to have made, it would not have been possible for me to claim that a country had experienced both a decline in wealth per head and an increase in GNP per head over an entire period of time. So he thinks that only sub-Saharan Africa among the regions appearing in Table 4 had suffered from a decline in per capita wealth.

But I did not assume a constant output–wealth ratio. I interpreted the entire period 1970–93 to be a unit of time. The figure I assumed for the output–wealth ratio was taken to be valid only for that unit. I have reread the relevant passages in my article and do not understand why Johnson has not understood what I was doing. It is possible that he found my interpretation unnatural and therefore dismissed it. So, here is the extended reasoning.

Let $W(t)$, $w(t)$, and $Y(t)$, respectively, be wealth, wealth per head, and GNP at time t . Assume that the period to be studied is $[0, T]$. Hamilton and Clemens (1999) provided estimates of $[dW(t)/dt]/Y(t)$, which were collated in the first column of Table 4 as time averages.³ In the third column I offered estimates of time averages of $[dw(t)/dt]/w(t)$. I remarked in the text following the table that the estimates are upper bounds of the true figures. How did I arrive at the estimates?

Consider first a region where $[dW(t)/dt]/Y(t)$ was positive (China, India, and Pakistan).⁴ Let $\beta(t) = Y(t)/W(t) > 0$, and define β^* by the equation,

$$\int_0^T \left([dW(t)/dt] / W(t) \right) dt = \beta^* \int_0^T \left([dW(t)/dt] / Y(t) \right) dt. \tag{1}$$

Notice that β^* is a time average of $\beta(t)$. Let n be the average growth rate of population. Now suppose that the right-hand-side of (1) is less than nT . I would then be justified in concluding that wealth per head had declined in the region in question.

The problem is that we do not know $\beta(t)$. Therefore, we cannot estimate β^* . So, I write β_{\max} as the maximum value of $\beta(t)$ during $[0, T]$. Obviously, $\beta_{\max} > \beta^*$. It would be astonishing if β^* exceeded 0.25 per year: I know of no modern estimate of a country-wide capital–output ratio that is less than 3 years; and as those estimates do not include human capital and natural capital, it would be astonishing if even β_{\max} were as high as 0.33 per year. Very conservatively, then, I assumed $\beta^* = 0.25$ per year. Figures for n were given in column 2 of Table 4. It is then simple to confirm that rates of change in per capita wealth in India, Pakistan, and sub-Saharan Africa were negative, as shown in the table.⁵ The Hamilton–Clemens data imply that in those three “countries,” $\beta(t)$ increased over the period. In India and Pakistan, GNP per head grew, even while wealth per head declined. Economic growth took place in tandem with a “mining” of the natural-resource base relative to population growth. In sub-Saharan Africa, even GNP per head declined.

For a region where $[dW(t)/dt]/Y(t)$ was on average negative (Bangladesh and Nepal), the matter is simpler: given that population had grown in both countries, wealth per head must have declined there. Now, per capita wealth could not possibly have declined at a rate less than n (the rate of decline would equal n only if the output–wealth ratio were zero, a patently absurd figure). So I used 0.25 per year for estimating the average rate of change in per capita wealth even in Bangladesh and Nepal. The calculations, taken together, imply that there has been net disinvestment on a per capita basis in regions that in total housed over 2 billion people in 1998. I believe Johnson would agree that disinvestment has been common.

And finally, using the above argument, I noted that wealth per head in China increased during 1970–93. However, given that genuine investment in the Hamilton–Clemens estimates did not include soil, water, fisheries, biodiversity, and air and water pollutants, the possibility that even in China wealth per head has declined cannot be ruled out.⁶

Increased life expectancy and population growth

Increased life expectancy is a good thing. But if it is not accompanied by an increase in genuine investment, there is a loss as well. Since my article noted both points, I find it incomprehensible that Johnson should think I would regard gains in life expectancy to be a negative factor in the development of countries. Johnson is, of course, right to draw attention to the many items of potential importance that Hamilton and Clemens omitted from their estimates of genuine investment. But their attempt was only a first cut. We would by now have been far ahead in our understanding of “what really happened” in poor countries if demographers and development economists, such as Johnson himself, had taken environmental and resource economics seriously in the past.⁷

Limited female education and fertility

I do not know why Johnson finds it perplexing that I noted examples of places in sub-Saharan Africa where there appeared to have been a positive link between early, limited education and fertility. My reason was to show that the now-conventional wisdom that women’s education is a powerful force against pronatalism needs to be qualified: the level of education can matter (Dasgupta 2000: 657). I noted too that, if educated women were role models, female education would be a trigger for fertility decline. I developed a “contagion” model to illustrate a pathway by which fertility transitions can occur (pp. 664–670). On such matters, however, Johnson is silent.⁸

Some other points

A novel technique Johnson deploys to criticize my article is to claim that I must be against something if I draw attention even to a single bad feature of that thing. But my article emphatically acknowledged multiple factors underlying the phenomena I was studying. Responding to an unremarkable observation in my article, that urban crowding (e.g., as manifested in shanty towns) creates a fertile ground for the spread of pathogens (a “bad” externality), Johnson imagines I must be unaware of many other features of urban infrastructure that make urban life in poor countries on the whole less insalubrious than rural life.

Johnson criticizes my attempt in the Appendix to develop a formal model in which, for a range of parameter values, there is positive feedback between fertility and deterioration in the local natural-resource base. But the model also identifies parameter values for which the feedback process is negative. I would have imagined he would appreciate being offered a model sharp enough to be tested against data from different regions of the world. Instead he complains that I have not tested my model against data before presenting it.

Concluding comments

Why is it that whenever someone draws attention to possible contemporary links in the world's poorest regions between poverty, population, and the natural-resource base, he is immediately accused by someone else of "extreme pessimism"? Since I was not engaged in making forecasts, the accusation is a non sequitur when applied to me. What then is the reason for the suspicion?

Deep down, it seems to me, the differences between Johnson and me, as revealed in this exchange, stem from the fact that his perspective is drawn from a study of contemporary history's winners, whereas the questions I studied in my article were prompted by a concern for the large number of contemporary history's losers. Modern economics urges us to recognize that "getting prices right" is necessary if economic problems are to be eased. And yet, if an economist working on population, poverty, and the natural-resource base in poor regions explores why prices there could be "wrong," the suggestion is put forward immediately that he is a Malthusian, the worst sin among contemporary demographers and development economists. I find this hard to understand.

Notes

In order to help readers, I respond to each of Professor Johnson's criticisms on a section-by-section basis. For expositional ease, however, I have reordered Johnson's sections. I am most grateful to Sriya Iyer for her comments on my reply.

1 For earlier observations of the kind Johnson makes, see Simon (1981).

2 See Agarwal (1986) and Chopra and Gulati (2001) for fine empirical studies on rural poverty and resource depletion in semi-arid poor regions.

3 What I have just said is not theoretically quite correct, in that, in estimating changes in wealth in closed economies, the capital gains

terms should be ignored (Dasgupta and Mäler 2000; Dasgupta 2001). I ignore such refinements here because, owing to a lack of data, Hamilton and Clemens (1999) were unable to offer guidance on such matters.

4 The argument can be adapted to the case of sub-Saharan Africa, where genuine investment was positive for an initial number of years, but negative thereafter.

5 As mentioned earlier, the estimate for sub-Saharan Africa should be increased from -3.4 percent per year to -2.3 percent per year.

6 Judith Shapiro's (2001) study of ill-conceived economic programs in Maoist China is

a convincing account of how suppression of civil and political liberties can contribute to the destruction of a country's natural-resource base.

7 One way to recognize the value of increased life expectancy is to regard expenditure on health as a form of investment. Katie Bolt of the World Bank (private communication) has redone the Hamilton–Clemens estimates of genuine investment by adding total expenditure on health to them. (This is an overestimate of what we want: what is relevant is the increase in health expenditure per head over the period.) If 0.25 per year were taken as the figure for β^* , Bangladesh, Nepal, Pakistan, and sub-Saharan Africa would continue to be regarded as countries that had decumulated wealth per head, but India would be seen as a slight accumulator. However, 0.25 per year is overly high. If β^* were taken to be

0.20, India would be judged to have been neither an accumulator nor a decumulator. At the more reasonable figure of 0.15 for β^* , India would be recorded as a country that had disinvested.

Johnson observes that human capital formation was underestimated by Hamilton and Clemens, inasmuch as the time spent by parents in educating their children was not included. On the other hand, the estimates did not include the fact that people take their human capital away with them when they die. There are errors in both directions in the Hamilton–Clemens estimates. For a discussion of ways in which genuine investment could be better measured, see Arrow, Dasgupta, and Mäler (2001).

8 Krishnan (2001) has found evidence of imitative behavior in fertility decisions in regional data from India.

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First Impressions from the 2000 Census of China

WILLIAM LAVELY

THE 2000 CENSUS of China is most obviously notable for its grand scale. Requiring 10,000 tons of paper for questionnaires, 5 million enumerators, and a million supervisors, it was at the very least an impressive logistical feat. But this census is notable, too, for its design and expanded content, reflecting China's increasing complexity and the rising demand for social data to inform policy. And it is notable for the unprecedented challenges it encountered in the field, conducted as it was under an administrative regime that is, in several respects, inimical to accurate census reporting. The preliminary results mirror China's continued social progress and apparent success in the drive to curtail population growth. This report briefly describes the 2000 census, sketches the highlights of preliminary tabulations, and discusses issues related to data quality.

Census innovations

Several innovations distinguish China's fifth census from the four previous ones. For the first time, a confidentiality statement appears on the census form; the census forms are designed for optical character scanning instead of manual data entry; and census documents can be accessed from an official census Web site. There are also changes to the census questionnaire. There is both a short and a long form, with the latter administered to a 10 percent sample of households in most provinces (to 10 percent of enumeration districts in remote areas). The long form provides unprecedented scope for data collection on issues of concern to China's policymakers, including housing, migration, and employment.

The short form contains standard items collected for every person in every household, including age, sex, nationality, registration status, and edu-

cational level. Items on migrant status, occupation, and marital status that were collected from all households in the 1990 census were in the 2000 census relegated to the long form. Every household is queried about births in the previous year, permitting a crude birth rate and sex ratio of births to be calculated for the entire population, but more detailed fertility questions are contained on the long form. Two items about housing—number of rooms and floor area—also appear on the short form, but the long form explores housing in considerable detail.

Housing is a pressing concern in China. In the past decade, housing reform policies have substantially privatized urban housing and opened a narrowly circumscribed housing market. A shift to an open market in housing, although not imminent, is on the horizon. The census long form contains no fewer than 15 items of direct relevance to this delicate transition, including questions on the use and age of dwelling, construction materials, fuel use, source of water, type of sanitary facilities, housing tenure, and the value or monthly rent of the property.

Migration is another phenomenon with far-reaching social implications. China's economic reforms have brought a rising tide of rural-to-urban migration. But the policies that liberated peasants from the land and created an urban labor market coexist uneasily with remnant structures of the socialist state. Chinese citizens are still officially divided between those with urban and those with rural household registrations. The former constitute a virtual hereditary caste entitled to educational benefits, jobs, and other welfare guarantees, in contrast to migrants in cities who have no perquisites and whose urban foothold is generally precarious. In other countries, schooling facilitates the assimilation of urban migrants. In China, where urban politics bear no responsibility for migrants, the "floating population" has emerged as a large and growing urban underclass, variously estimated at between 100 million and 200 million persons. The unwinding of this system, now cautiously advanced in policies that seek to dilute urban privilege, will take at least a generation. In the meantime, understanding population movement is a high administrative priority.

The census short form contains four questions on household members absent for less than six months, which should yield an estimate of the number and provenance of the short-term floating population. The census long form contains nine items related to household registration and migration, items that classify migrants with considerable spatial and temporal precision. The 1990 census only distinguished mobility across county boundaries; the 2000 census can chart mobility across township and street committee boundaries. The 1990 census could only detect movement in the five years before the census; the 2000 census records place of birth as well as location of last residence, and it records the year of last move. As precise as they are, these items will not permit a full assessment of the floating

population because the census is a quasi-de jure enumeration that counts people at their legal residence if they have been away from that residence for less than six months. But this six-month reference period is itself an important revision of the one-year reference period of previous censuses. By placing more migrants at their current abode rather than at their place of registration, it gives the census a more de facto cast. Yet by counting those away from home for less than six months at their legal instead of actual residence, it will still undercount migrants and understate their influence on urban populations.

Concern with the floating population explains another break with census tradition, the reference date. Each of China's four previous censuses set 1 July as the reference date; the fifth census set 1 November. The inadvisability of census work in hot summer weather is cited as one reason for this change, but the crucial rationale concerns the effort to enumerate migrants at their actual rather than legal residence (Zhang 1998: 7). Sojourning migrants generally return to their natal home for Spring Festival, which in the year 2000 occurred in February. Coming within six months of Spring Festival, a 1 July reference date would have placed many migrants at their legal rather than at their actual residence and thus would seriously have undermined the estimate of the number of long-term urban migrants.

Unemployment was foreign to the vocabulary of socialism, if not the reality, but in the reform era it has emerged as a volatile problem. Decollectivization in the early 1980s cast underemployed peasants into the labor market, while market forces continue to produce layoffs and forced early retirements in moribund state-owned enterprises. Yet despite its salience, unemployment has been virtually unmeasurable because of the variety of guises under which unemployed workers are categorized (Solinger 2001). The census long form contains items on employment that address this problem. In addition to the occupation and industry items that also appear in previous censuses, there are questions in 2000 about work for pay in the week preceding the census. Another item classifies the unemployed and inquires about their source of support. These items should permit a basic but systematic appraisal of economic activity and unemployment.

Preliminary data

As of this writing, the main source of published data from the census is *Major Figures on 2000 Population Census of China* (Population Census Office 2001). A series of provincial census communiqués are also available from the official census Web site (<http://www.p2000.gov.cn/p2000/index.htm>). The data released thus far provide the basis for only very general and tentative conclusions.

National trends

With some 1.265 billion persons as of 1 November 2000, the People's Republic of China accounts for approximately 21 percent of the human population. China is still the most populous nation, a title it will not cede to India for another four to five decades (Dyson 2001). China's population grew more slowly in the past decade than in any other decade since the founding of the People's Republic. The four intercensal periods shown in Table 1 neatly frame the stages of demographic transition, with average annual growth peaking at 2.07 percent in the years 1964–82, dropping to 1.47 percent in the years 1982–90, and dropping further to 1.07 percent in the past decade. One may infer that reductions in fertility account for the decline in population growth, as there is no evidence that the crude death rate rose in the decade. A possible alternative explanation would be a substantial undercount of population in 2000 relative to the undercount in 1990. The problem of data quality will be considered in the following section.

China's population is composed of a Han majority and 55 officially designated minority nationalities: minorities represented 8.4 percent of the total in 2000. Decelerating growth of the minority population in the past decade has set Han and minority growth rates on a path toward convergence. Between 1982 and 1990 the minority population grew at an annual average rate of 3.83 percent, an exceptionally rapid rate produced by high natural increase as well as reclassification of persons from Han to minority status (Poston 1993). In the 1990s minorities grew at 1.51 percent per year, less than half the rate of the previous decade. This trend likely reflects the successful extension of birth planning policies into previously exempt minority areas.

China has certainly urbanized in the past two decades, but frequent changes in the definition of the urban population have made the measurement of urbanization one of the more treacherous areas of Chinese demography. The measurement problem can be observed in Table 2, which shows

TABLE 1 China: Population and population growth over five censuses, 1953–2000

Year	Population (thousands)	Absolute intercensal increase (thousands)	Average annual intercensal growth rate (percent)
1953	582,603	—	—
1964	694,582	111,979	1.60
1982	1,008,180	313,598	2.07
1990	1,133,709	125,529	1.47
2000	1,265,830	132,121	1.07

NOTES: Population totals include the People's Liberation Army but do not include Taiwan, Hong Kong, Macao, or Overseas Chinese. Population totals refer to 1 July, except for 2000, which refers to 1 November.

SOURCES: 1953 and 1964: State Statistical Bureau 1986a and 1986b, respectively. 1982, 1990, and 2000: Population Census Office 1985, 1993, 2001, respectively.

the urban population and percent urban as measured in recent censuses and reports of the State Statistical Bureau (SSB). The 1982 census put China’s urban proportion at 20.6 percent, roughly where it had stood for the previous 20 years. This rose to 26.2 percent in 1990, to 28.6 percent in the 1995 one percent sample census, and to 30.9 percent in the SSB estimate for 1999 (see Table 2). The 2000 census reporting 36.1 percent as urban is obviously inconsistent with the earlier estimates.

The discontinuity involves two separate measurement issues. The first concerns the definition of an urban place. The 1990 and 2000 censuses define urban places according to different criteria. In 1990, and through the ensuing decade, the urban population was defined by administrative categories: residence committees were uniformly counted as urban, and village committees were counted as rural unless they were administered under cities or towns at the prefectural level or above (Chan 1994). The urban definition used in the 2000 census departs from the strict administrative definition by promoting some village committees to urban status on the basis of density measures and other criteria.¹ This revised definition more faithfully reflects the urban reality of densely populated industrialized settlements that are still under village administration.

The other, and no doubt more important, reason for the inconsistency is the revised method for determining residence at the time of the census—the six-month reference period alluded to above. Under 1990 census rules, rural migrants residing in a city for less than one year were counted at their place of registration. Under rules guiding the 2000 census, those residing in the city for six months or more were counted at their urban abode.

Comparing the 2000 census estimate with that for 1999, it is apparent that the revised measure captures approximately 67 million more urban residents. Although still an underestimate of the urban proportion by the standards of a purely de facto census, the 2000 census moves the estimate

TABLE 2 China: Estimates of the urban population and percent urban, 1982–2000

Year	Source	Urban population (thousands)	Percent of population urban
1982	Census	206,309	20.55
1990	Census	296,145	26.20
1995	Sample survey	353,396	28.58
1999	Sample survey	388,920	30.90
2000	Census	455,940	36.09

NOTE: Three definitions of urban population are used here: one for 1982, one for 1990–99, and one for 2000. In 1982, the urban population was defined as the population of cities, urban districts under cities, and towns, defined as settlements of minimum population size and percent of population “nonagricultural” (Chan 1994). For 1990 and 2000, see text.
 SOURCES: 1982, 1990, and 2000: Population Census Office 1985, 1993, 2001, respectively. 1995: National Population Sample Survey Office 1997. 1999: National Bureau of Statistics 2000.

closer to the actual figure. The urban trend is real, and represents a profound social revolution that, should it continue, will transform China into a mainly urban society in less than two decades.

Although China's population has "aged" over the past decade, it is still relatively young. In labor force terms, China has high productive potential, with 70 percent of the population in the 15–64-year age range (see Table 3). The proportion of the population age 65 and older rose from 5.6 percent in 1990 to 7.0 percent in 2000. An even more rapid rise in the proportion over age 65 will occur over the next few decades.

Urban–rural differences suggest the forces that have shaped the age structure. Urban China, where the one-child policy has been successfully enforced for two decades, has a relatively low proportion under age 15, and a larger proportion aged 15–64. In the 2000 census 75.3 percent of urban Chinese were in the 15–64-year age range, implying a dependency ratio that is extraordinarily low. Selective migratory flows in the 1990s probably reinforced this age pattern. As may be seen in Table 3, in 1990 rural and urban China had roughly equal proportions aged 65 and older. By 2000 the proportion of rural elderly exceeded that of urban, a counterintuitive result that can be explained by the flow of working-age persons from rural to urban areas.

China's citizens were better educated at the end of the 1990s than at the beginning. Growth of the population with secondary and higher education was particularly rapid. Middle-school graduates rose from about 23 for every hundred persons in the total population in 1990 to 34 in 2000. Those with any higher education remain a tiny but fast-growing elite. Those attaining junior college or above jumped from 1.4 percent to 3.6 percent of the total population in the past decade. The illiterate population also continued its secular decline. In 1964 nearly 33 percent of Chinese aged 15 or older were illiterate or semiliterate, a number reduced to 15.9 percent in 1990. By 2000 this had fallen to 6.7 percent. Of course, rising educational attainment would be expected even if rates of schooling did not change in the decade, because younger, better-educated cohorts are replacing older,

TABLE 3 China: Percent distribution of rural and urban population by age group, 1990 and 2000

Age	Total		Rural		Urban	
	1990	2000	1990	2000	1990	2000
0–14	27.7	22.9	29.6	25.5	25.9	18.4
15–64	66.7	70.1	64.8	67.2	68.6	75.3
65+	5.6	7.0	5.6	7.3	5.5	6.3
Total	100.0	100.0	100.0	100.0	100.0	100.0

SOURCES: Population Census Office 1993, 2001.

TABLE 4 Population and percent urban in the 2000 census, and measures of change 1990–2000: China total and provinces

Province	Population (1,000 persons)	Average annual rate of growth 1990–2000	Percent urban	Absolute increase in percent urban 1990–2000
China total	1,265,830	1.07	36.1	9.9
Beijing	13,820	2.40	77.5	4.5
Tianjin	10,010	1.27	72.0	3.3
Hebei	67,440	0.96	26.1	7.0
Shanxi	32,970	1.33	34.9	6.2
Inner Mongolia	23,760	0.99	42.7	6.6
Liaoning	42,380	0.69	54.2	3.4
Jilin	27,280	0.98	49.7	7.0
Heilongjiang	36,890	0.45	51.5	4.4
Shanghai	16,740	2.22	88.3	22.1
Jiangsu	74,380	1.01	41.5	20.3
Zhejiang	46,770	1.18	48.7	15.9
Anhui	59,860	0.62	27.8	9.9
Fujian	34,710	1.41	41.6	20.2
Jiangxi	41,400	0.91	27.7	7.3
Shandong	90,790	0.71	38.0	10.7
Henan	92,560	0.77	23.2	7.7
Hubei	60,280	1.08	40.2	11.3
Hunan	64,400	0.58	29.8	11.5
Guangdong	86,420	3.13	55.0	18.2
Guangxi	44,890	0.59	28.2	13.1
Hainan	7,870	1.78	40.1	16.1
Chongqing	30,900	0.66	33.1	15.7
Sichuan	83,290	0.59	26.7	5.4
Guizhou	35,250	0.82	23.9	4.9
Yunnan	42,880	1.44	26.7	8.6
Tibet	2,620	1.72	23.9	6.3
Shaanxi	36,050	0.89	23.3	10.8
Gansu	25,620	1.32	24.0	2.0
Qinghai	5,180	1.47	34.8	7.4
Ningxia	5,620	1.84	32.4	6.7
Xinjiang	19,250	2.34	33.8	1.9

NOTE: Absolute population figures are rounded to the nearest 10,000. The urban population was defined differently in 2000 than in 1990. See text.

SOURCE: Population Census Office 2001.

Tibet, which draw many Han migrants from the interior and where minority fertility is under looser constraints. Growth rates tended to be lower in impoverished areas of the interior that have experienced large outflows of economic migrants. In this category, Hunan, Sichuan, Chongqing, and Henan are notable.

Urbanization in the 1990s (as measured by the two noncomparable definitions discussed above) was closely tied to economic growth. The urban percentage grew the most on the east coast, seen in Figure 1 as a dark crescent running from Jiangsu in the north to Hainan in the south, a region favored by investors from Taiwan, Hong Kong, and elsewhere. Jiangsu and Fujian each went from 21 percent to 41 percent urban in the decade. The growth of the urban proportion was less but still substantial in a second tier of provinces in the middle reaches of the Yangzi River, including Anhui, Hubei, and Hunan. The only rapidly urbanizing province in the interior was the newly created Chongqing Municipality. Every provincial-level unit experienced some urban growth in the decade. The slowest urban growth occurred in the far west and in the northeast, the latter already highly urbanized and saddled with many failing state-owned enterprises.

Data quality

How good are the data? Given the environment in which the 2000 census operations took place, this is no routine question. China's social and political conditions once favored a high-quality enumeration, but no longer. Two decades ago China had a relatively immobile population under the surveillance of a powerful and pervasive bureaucracy. There were, moreover, few disincentives to full and accurate reporting, for citizens or for officials. Under these circumstances China was able to produce census and survey data of remarkable accuracy. But two important phenomena arose, beginning in the 1980s, to alter this favorable situation. The first is a tide of rural-to-urban migrants, people who are difficult to track and whom officials of cities would prefer not to have on their books. The second is the tightening of birth planning administration, which has given both families and local officials substantial disincentives to report out-of-plan births.

Results of a postenumeration survey suggest that census data quality is good. A survey of 602 enumeration districts found a net undercount of 1.81 percent of the population (SSB 2001). Although this is noticeably higher than the .06 percent undercount reported for the 1990 census, it is considered reasonable by international standards (Walfish 2001). A complete report of the postenumeration survey, however, is not yet available. It would be surprising if counts of births, deaths, migrants, and children were as complete as the overall population count. And, although there is as yet no basis on which to evaluate the postenumeration survey, there are reasons to expect that the survey could itself fall substantially short of a complete enumeration. To understand why, it is necessary to consider the confluence of forces that affected the recent round of census work.

The high quality of previous Chinese censuses was due in considerable measure to the existence of the household registration system and to the direct linkages between the registers and the census. The household

registers include information about every household and constituent individual in an administrative area. The register should reflect every change in the composition of a household brought about by birth, death, marriage, or migration, and it should reflect the sum of official knowledge about residents of the local administrative area.

The linkage between household registration and census work is direct and explicit. In the months immediately before a census, the household registers are updated and verified in a process called “rectification” (*zhengdun*). By the time of the census the registers should reflect all information available to local officials about households in the enumeration area. The enumerator then draws upon the household register to construct a list of households in the enumeration district. This listing (the *hukou xingming dice*) includes the name and address of the household head, the number of registered household members, the number of births and deaths in the household in the past year, and the number of registered persons who are absent for less than and for more than six months. Thus at the time of the household interview, the enumerator already has in hand a list of basic information that can be checked against that provided by the household informant. Census procedures anticipate cross-checking between sources, including “local cadres, activists, and other informed people” who are interviewed in a postenumeration meeting.² Census procedures emphasize that the household interview is mandatory, but make it equally clear that the interview is not the sole source of information. It is thus theoretically possible for Chinese census data to be more accurate than the information provided by the census respondents themselves.

But the household registers—and local population statistics in general—are not as good as they used to be, and this creates problems for the census. Because household registration work is now seriously distorted by other policy imperatives, even the “rectified” registers probably misrepresent the actual status of local populations. Register errors accumulate between censuses. In the 2000 census, pre-census rectification revealed that Wenzhou city had 50,000 deceased persons still on the registers, and Chongqing Municipality had 130,000. These errors were corrected, of course, but it is far easier to purge registers of the dead than it is to add the living. Children born outside the birth plan are often (despite central government regulations) excluded from the register, at least until substantial fines are paid. In Chongqing, rectification work uncovered 68,000 such cases,³ but more could go undetected because both citizens and officials have reason to hide them. Parents conceal excess births to avoid fines, while officials keep them off the books because cadre job evaluations are based primarily on birth planning performance. To shield census work from these unfavorable incentives, census officials directed that all unregistered out-of-plan children be registered, that no fines be collected from parents of these children, and that officials be granted amnesty for previous birth planning falsifications

as long as they were truthfully reported in the census.⁴ But the census is a passing event, while birth planning is a perennial problem. It would be only natural for local officials, fearing a “squaring of accounts after the autumn harvest,” to err on the side of discretion.

Other administrative considerations militate against the counting of urban migrants. The floating population is not easy to count in the best of circumstances, analogous in difficulty to counting undocumented aliens or the homeless in the United States. For this reason, census operations included unusual efforts to enumerate urban migrants, including an extensive pre-enumeration of sojourners sleeping outdoors and in public places. But local officials, who for various reasons wish to minimize the population of their administrative domain, may be less interested in a complete count. Low rates of population growth reflect success in population control, and small population totals boost per capita measures of income and productivity. Urban cadres in particular may be wary of moves by the central government to regularize the status of temporary migrants, as this could lead to additional claims for urban services such as schooling. Thus even as census workers valiantly sought out the floating population by scouring construction sites and searching under bridges, local officials in some places subjected migrants to “census fees,” “security fees,” and “temporary residence fees” in an attempt to discourage them from being counted.⁵

The 2000 census was dogged by rumors of vast undercounts. Some provinces, such as Henan, Hunan, and Shaanxi, were said in newspaper accounts to have counted millions fewer people than expected. These reports were probably exaggerated and possibly were based on misunderstandings of the way out-of-province migrants would be counted. Nonetheless, the potential for error in this census is considerable. Users should proceed with caution until the data have been carefully evaluated.

Open questions

The preliminary data release is also interesting for its omissions. Unlike the preliminary census publications that followed the 1982 and 1990 censuses, *Major Figures* for 2000 does not report on birth rates or on the sex ratio of births. It is thus silent on two of the most pressing issues in Chinese demography. This is assuredly not by chance and may well be indicative of yet other unseen problems.

There is great uncertainty about the level of China’s fertility, reflecting declining confidence in the quality of birth reporting over the past decade. Although *Major Figures* did not report fertility for 2000, six provincial census bureaus reported crude birth rates (CBRs). These are shown in Table 5, along with corresponding data from the 1995 one percent sample survey and from the SSB 1999 sample survey on population change, also a one percent population sample.

TABLE 5 Crude birth rate (births per 1,000 population) for China total and for provinces reporting birth rates based on the 2000 census, 1995–2000

	1995	1999	2000
China total	17.1	15.2	—
Beijing	7.9	6.5	6.0
Shanghai	5.8	5.4	5.5
Zhejiang	12.7	10.6	10.3
Guangxi	17.5	15.0	13.6
Chongqing	—	11.9	9.7
Guizhou	21.9	21.9	20.6

NOTES: 1995 refers to 1 October 1994 – 30 September 1995. 2000 refers to 1 November 1999 – 31 October 2000.

SOURCES: 1995: National Population Sample Survey Office 1997. 1999: National Bureau of Statistics 2000. 2000: Provincial communiqués «<http://www.p2000.gov.cn/p2000/index.htm>»

The provincial CBRs suggest that fertility has declined since 1995, as each of the six provinces reports a lower CBR in 2000 than in 1995 or in 1999. This trend is difficult to interpret because it involves the changing age composition of the population as well as the underlying age-specific fertility rates. Another layer of complexity is added by the possibility that the reported rates have been adjusted by SSB demographers. Rates derived from fertility surveys are generally rejected as unrealistically low. The report of the 1995 one percent sample survey, for example, put the crude birth rate at 17.1 per 1,000, but this figure is apparently the result of a substantial upward adjustment.⁶ We do not yet know whether 2000 census fertility rates have been or will be adjusted. This information is obviously crucial for evaluating the census fertility data.

The sex ratio at birth (SRB) is also conspicuously absent from *Major Figures*. The ratio has risen steadily since 1980, a trend that has ominous implications for female welfare and for future nuptiality patterns, and bleak ramifications for poor families whose life plans depend crucially on the recruitment of a daughter-in-law. The 1982 census recorded (for 1981) an SRB of 108.5. The 1995 one percent sample survey found an SRB of 115.6, and the State Statistical Bureau announced an SRB of 117 for 1999. Sex ratios of the population aged 0–4 years have risen correspondingly (see Table 6). The proximate causes of this trend must be some combination of underreporting of surviving females and excess female mortality, whether *in utero* or after birth. The 2000 census should provide valuable data for evaluating the trend.

The foregoing discussion leads to a final question: how will the census be published and distributed to users? Census data for 1990 were published in printed volumes, but no public-use micro sample has yet been released.

TABLE 6 China: Sex ratio of births and sex ratio of the population aged 0–4 years, 1953–99

Year of census or survey	Sex ratio of births (males per 100 females)	Sex ratio of the population aged 0–4
1953	—	107.0
1964	—	105.7
1982	108.5	107.1
1990	111.4	110.2
1995	115.6	118.4
1999	117	119.5

NOTE: 1982 census sex ratio at birth (SRB) refers to births in 1981; 1990 census SRB refers to births in 1 January 1989 – 30 June 1990; 1995 sample census SRB refers to births in 1 October 1994 – 30 September 1995. The 1999 SRB refers to births in 1 October 1998 – 30 September 1999; this figure was given as 117 without a decimal point.

SOURCES: 1953 and 1964: State Statistical Bureau 1986a and 1986b, respectively. 1982 and 1990: Population Census Office 1985 and 1993, respectively. 1995: National Population Sample Survey Office 1997. 1999: National Bureau of Statistics 2000 (for sex ratio of population) and (for sex ratio of births) 28 March, 2001 news conference of National Bureau of Statistics Director Zhu Zhixin. The latter is presumably from the SSB Population Change Survey of 1999.

Demographers in China and abroad have had to rely on micro sample datasets purveyed through the back door (Mason and Lavelly 2001). China's top census officials clearly envision a census that is widely used and provided in forms appropriate for computerized data analysis (Zhang 1998: 11–12). Whether they are able to realize this vision remains to be seen.

Notes

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1 The revised urban definition is described in State Statistical Bureau Document 1999, No. 114 (*Guanyu tongjishang huafen chengxiang de guiding (shixing)*) [Regulation concerning statistical differentiation of urban and rural (provisional)]. I am grateful to Kam Wing Chan for providing a copy.

2 These procedures are described in the census enumerator handbook (Population Census Office 2000b). In the postenumeration phase a range of documents may be consulted, as illustrated by this report from the New China News Agency:

In response to the problem of the non-reporting of the excess birth population, Henan demands that every place implement “five matches” of data from the 1998, 1999, and 2000 village (neighborhood) committee birth

planning monthly reports, household register *zhengdun* materials, figures held by the village (neighborhood) committees, the *hukou xingming dice*, and the census enumeration form, comparing each with each and verifying no errors. (*Xinhuashe* 2000)

3 Chongqing figures are from *Yangzi Wanbao*, 5 November 2000 (“Chongqing renkou pucha xiayitiao—13 wan siwangren hukou wei chuxiao” [Chongqing census shock: household registrations of 130,000 dead persons not cancelled]). Wenzhou figures are from *Xinmin Wanbao*, 11 November 2000 (“Wenzhou renkou pucha faxian wu wan duo siren ‘huozhe’” [Wenzhou census discovers over 50,000 dead persons “alive”]).

4 These points were contained in a circular published by the State Council Fifth National Population Census Leading Small Group, entitled “Guanyu zai diwuci quanguo renkou pucha zhong zhen zuohao renkou pucha

dengji gongzuo fangzhi manbao loubao de tongzhi" [Doing good enumeration work and preventing falsification and underreporting in the fifth national population census], reported in *Yangcheng Wanbao*, 26 October 2000 ("Renkou pucha ziliao bude zuowei jisheng yiju" [Census data should not be used as a basis for birth planning]).

5 Many news reports at the time of the census described and warned against such conduct. One noted that:

...in some places the census is used as a pretext to levy illegitimate fees, so that the migrant population doesn't dare to be enumerated; there are also some places where the census is a pretext to do birth planning investigations and levy heavy fines on excess births, to the extent that even some households that have already paid fines and registered their excess child are again fined, so that excess birth households do not dare to make a truthful report. *Huashangbao*, 10 November 2000. ("Shaanxi renkou pucha cunzai yanzhong wenti: ying dengji renkou shaole 200 wan" [Shaanxi census has a serious prob-

lem: enumerated population two million less than expected])

For other examples see *Renmin Ribao*, 19 November 2000, dateline Changsha ("Hunan sheng renkou pucha gongzuo wending zhashi, moxie meiti 'loudeng qianwanren' de chuanwen bu shi" [Hunan province census work is steady and solid; the rumor of "ten million undercounted" carried in some media is false]); *Xinhuashe*, 22 November 2000, dateline Hefei ("Anhui yanjin jie renkou pucha luan shoufei luan fakuan" [Anhui strictly prohibits using the census as a pretext to collect illicit fees and fines]); *Sichuan Ribao*, 17 November 2000 ("Sichuan jinji dian ling gedi jianjue zhizhi zai renkou pucha zhong luan shoufei" [Sichuan urgently telegraphs order to resolutely curb illicit fee collection during the census]).

6 The State Statistical Bureau uses an adjustment factor to adjust for undercount of births in register and survey data. According to Judith Banister (personal communication), the multipliers used rose from 1.072 in 1991 to 1.123 in 1995.

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The Impact of HIV/AIDS on Adult Mortality in Zimbabwe

GRIFFITH FEENEY

IN THE COUNTRIES most severely affected by HIV/AIDS, deficiencies in national statistics make it difficult to gauge the demographic impact of the epidemic. This report analyzes national data on the level and trend of adult mortality in Zimbabwe over the past two decades. It assesses data quality and presents estimates of the rise in adult mortality risks. Although the estimates are far from perfect, comparative analysis of data from numerous sources provides persuasive evidence of the impact of the epidemic.

The UNAIDS “Durban Report” (UNAIDS 2000) shows 25 percent of adults in Zimbabwe living with HIV/AIDS, making it one of the countries most affected by the epidemic. Evidence of the social and economic consequences of the epidemic is extensive and indicates serious impacts on the macroeconomy, on households and school enrollment, and on agriculture and business (African Development Forum 2000).

Materials and methods

Four sources of data are available for the estimation of adult mortality in Zimbabwe.

Vital registration

Vital registration data provide deaths classified by age and sex for 1982 (United Nations 1986: Table 26), 1986 (United Nations 1995: Table 19), and unpublished figures for 1990–92 and 1995. Completeness of registration was estimated as described below, using, in addition to the vital registration data, the distributions of the population by age and sex from the 1982 (United Nations 1990: Table 7) and 1992 (Zimbabwe Central Statistical Office; no date: Table A1.2) population censuses and from a 1997 population survey.

Completeness of death registration during the 1982–92 intercensal period was estimated using the method pioneered by Brass (1979). The criticism of this method by Trussell and Menken (1979) is addressed by iteratively adjusting the second census age distribution for relative completeness of enumeration.

The Brass method requires total registered deaths in each age-sex group during the intercensal period. “Registered” deaths for 1983–85, 1987–89, and 1993–94, for which vital registration data are not available, were estimated as the average of registered deaths in surrounding years. Registered deaths for the intercensal period are then estimated by summing deaths for each year, with the first and last years prorated according to the date of the censuses. Age-specific death rates for males and females are then calculated, with denominators estimated by exponential interpolation between the 1982 and 1992 census age-sex distributions and between the 1992 census and 1997 survey age-sex distributions.

The resulting death rates for ages 10–14 years show substantial increases for both males and females between 1986 and 1995. Since little impact of HIV/AIDS on mortality risks for this age group is expected during this period, these increases suggest that completeness of death reporting improved during the intercensal period. The trend of registration completeness was estimated by assuming that death rates for the 10–14-year age group were constant during 1986–92. The level of registration completeness for 1982–92 is set using the adjusted number of deaths for the intercensal period as a whole. Completeness of registration for 1995 was estimated by extrapolating the trend of improvement between 1986 and 1992.

The resulting estimates of registration completeness were used to adjust registered deaths for incomplete reporting. Age-specific death rates were then recalculated using adjusted numbers of deaths. Life tables beginning at age 5 years were constructed from these death rates. Finally, probabilities of death ${}_{45}q_{15}$ (that is, the conditional probability of death by age 60 given survival to age 15), ${}_{35}q_{15}$, and ${}_{35}q_{30}$ are computed from these life tables. The first of these statistics is computed because it has become a standard indicator of adult mortality (Boerma, Nunn, and Whitworth 1998; Timæus 1998). The second two statistics are computed for comparison with estimates derived from survival of parents and siblings.

Estimates from household deaths data

No question on household deaths was included in the 1982 census of Zimbabwe, but data are available from the 1992 census and the 1997 intercensal survey. Underreporting of deaths is common with such questions, but overreporting may occur as well.

Estimates from parental survival data

The 1982 and 1992 censuses of Zimbabwe included questions on the survivorship of parents (Zimbabwe Central Statistical Office 1985; no date: Table 26), as did the 1997 survey. The method described in Timæus (1992) is applied to yield estimates of female conditional probabilities of death from age 25 to ages 35, 40, ..., 80 and estimates of male conditional probabilities of death from age 35 to ages 45, 50, ..., 75.

The method requires estimates of the mean age at which females and males experience births during any given year. The mean age at which females experience births, 26.7 years, is calculated from 1992 census data on births occurring during the year prior to the census (Trussell and Menken 1979). The mean age for males, 33.7 years, is estimated as the female mean plus the difference between the mean age of fathers at birth and the mean age of mothers at birth, estimated as the difference between mean age at marriage for males and females calculated from 1992 census data on marital status (Trussell and Menken 1979).

The dating procedure of Brass and Bamgboye (1981; United Nations 1983) is used to assign each estimated conditional probability of death to the point in time to which it applies, and all probabilities are translated to ${}_{35}q_{30}$ using the Brass General model life table (United Nations 1983: Table 2). A two-census variant of the estimation procedure that produces a single estimated adult mortality level (Zlotnik and Hill 1981) was also applied to the data for 1982–92 and 1992–97.

Estimates from sibling survival data

Adult mortality may also be estimated from reports on the survivorship of brothers and sisters. The 1994 Demographic and Health Survey (DHS) of Zimbabwe included questions on the survivorship of siblings (Zimbabwe Central Statistical Office 1995). No sibling survival data were published in the DHS report, but special tabulations are available. The sibling survival data may be used to estimate probabilities of death in the same way as the parental survival data were used, with dating and model life table translation yielding trends (Timæus, Zaba, and Ali 2001).

Results

Relative to the 1982 census, registered deaths during the 1982–92 intercensal period are estimated to be 44.2 percent complete for females and 63.2 percent complete for males. The 1992 census is estimated to have enumerated females 2.6 percent more completely, and males 2.3 percent more completely, than the 1982 census. Substantial improvements in completeness

of death reporting are estimated to have occurred between 1982 and 1995 (see Table 1, rows labeled "Completeness").

TABLE 1 Estimated age-specific death rates (ASDRs: deaths per 1,000 person-years lived), probabilities of death (${}_{35}q_{15}$, ${}_{45}q_{15}$, ${}_{35}q_{30}$), and completeness of death registration (percent) for Zimbabwe, selected years, 1982–97

ASDR	Based on vital registration adjusted for completeness of reporting						Based on household deaths	
	Year						Year	
	1982	1986	1990	1991	1992	1995	1992	1997
Females								
15–19	1.3	1.2	1.8	1.7	1.8	1.7	2.5	2.4
20–24	1.9	1.9	2.9	3.3	4.0	5.9	4.5	5.1
25–29	2.7	2.6	4.3	5.0	5.9	10.8	6.5	12.8
30–34	3.1	3.1	5.0	5.4	7.3	14.3	6.6	20.3
35–39	3.6	3.8	5.5	6.2	8.2	14.2	7.5	15.4
40–44	4.9	5.5	6.7	7.7	8.9	14.0	8.1	23.1
45–49	4.9	6.2	7.6	8.3	8.8	14.5	9.7	20.2
50–54	9.9	8.8	9.9	9.3	9.6	10.9	10.2	15.6
55–59	9.7	12.3	12.1	13.2	11.6	14.7	12.8	12.7
60–65	18.1	17.8	20.2	19.6	19.2	19.0	18.2	16.3
${}_{35}q_{15}$	107	117	159	177	208	331	210	421
${}_{45}q_{15}$	195	210	252	270	292	417	301	503
${}_{35}q_{30}$	249	263	300	311	324	422	322	494
Completeness	39.8	39.8	45.8	49.5	54.3	59.1	NA	NA
Males								
15–19	1.3	1.1	1.2	1.1	1.0	0.9	2.1	2.4
20–24	3.2	2.0	2.5	2.6	2.9	3.4	4.1	5.2
25–29	4.2	3.3	5.0	5.6	5.9	10.6	7.9	11.2
30–34	4.9	4.6	7.5	8.4	9.6	16.2	10.6	29.0
35–39	5.3	4.9	8.4	9.9	11.6	21.5	11.4	23.4
40–44	7.7	6.5	9.4	10.5	12.4	22.9	13.0	24.3
45–49	9.3	9.2	11.0	11.7	13.2	21.3	14.4	25.2
50–54	17.0	13.5	15.4	15.5	17.5	22.1	15.7	28.2
55–59	16.9	20.6	21.4	20.6	19.8	25.5	19.4	33.7
60–65	30.0	26.2	27.6	27.2	30.1	28.9	25.3	37.7
${}_{35}q_{15}$	169	150	209	230	259	414	285	496
${}_{45}q_{15}$	310	295	355	369	397	553	411	651
${}_{35}q_{30}$	395	374	426	435	469	593	452	695
Completeness	57.1	57.1	65.7	71.0	77.9	84.9	NA	NA

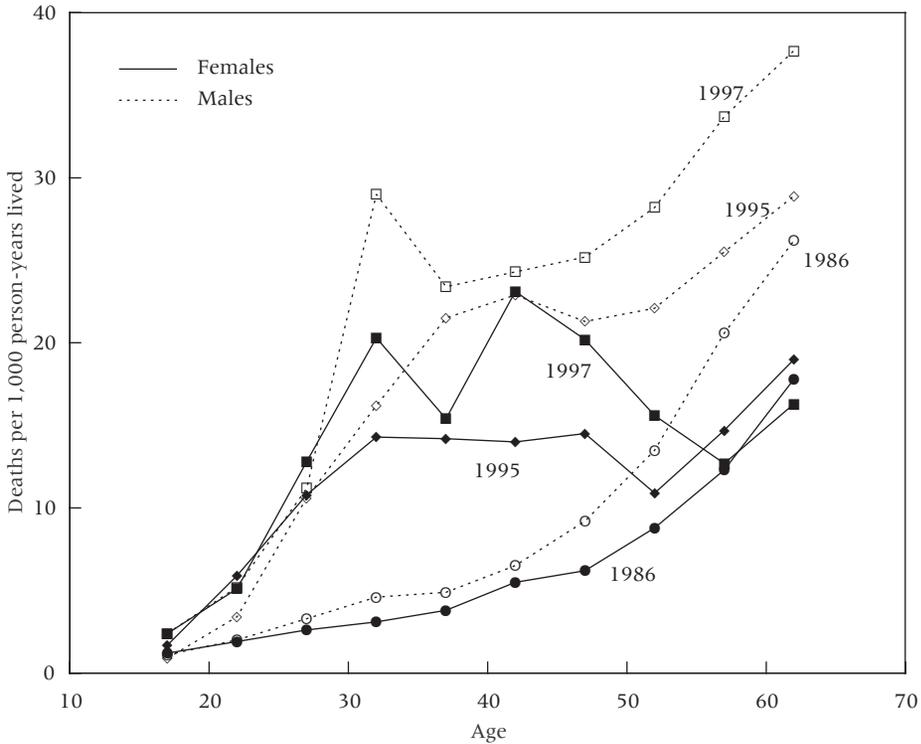
NOTE: Rates from vital registration calculated for all available years. See text for explanation of procedure for adjusting for incomplete reporting. Household deaths are for the year preceding the census or survey, rather than for calendar year. Household deaths are as reported (no adjustment) in the 1992 population census and the 1997 national population survey.

Age-specific death rates in prime adult ages rose by 200–300 percent between 1986 and 1997 (Table 1 and Figure 1). The conditional probabilities of death calculated from adjusted registered deaths are remarkably consistent with those calculated from household deaths (Figures 2 and 3). The latter estimates also indicate that probabilities of death rose sharply during the mid-1990s.

Conditional probabilities of death estimated from survival of sisters and brothers are shown in Table 2. In Figure 2 these probabilities are compared with the death registration and household deaths estimates. Taken together, the estimates indicate a gradual increase in the probability of dying between age 15 and age 50 from the early 1980s to the early 1990s and a rapid increase thereafter.

Conditional probabilities of death estimated from survival of mothers and fathers are shown in Table 2. Figure 3 compares these three sets of estimates with the death registration and household deaths estimates (for 1982, 1992, and 1997). The estimates based on survival of mothers and fa-

FIGURE 1 Age-specific death rates (ages 15–19 to ages 60–65), by sex: Zimbabwe, selected years



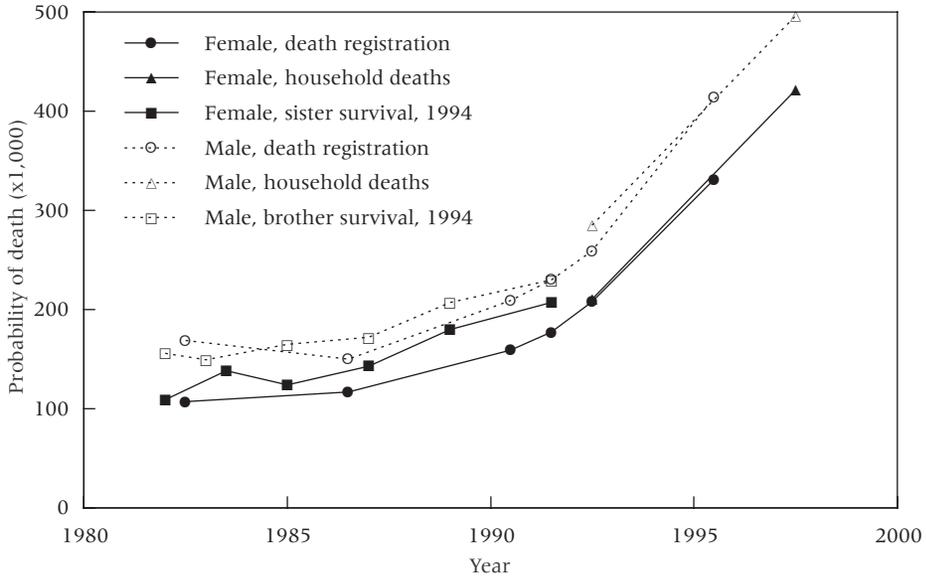
NOTE: Rates for 1986 and 1995 are calculated from vital registration data corrected for underreporting. Rates for 1997 are from 1997 population survey, unadjusted. See text for explanation of vital registration correction procedure.
SOURCE: Table 1.

TABLE 2 Male and female probabilities of death ${}_{35}q_{30}$ and ${}_{35}q_{15}$ estimated from survival of mothers, fathers, sisters, and brothers: Zimbabwe, 1982 and 1992 censuses, 1997 intercensal survey (ICS), and 1994 Demographic and Health Survey (DHS)

Females								
Survival of mothers							Survival of sisters	
1982 census		1992 census		1997 ICS		1994 DHS		
Year	${}_{35}q_{30}$	Year	${}_{35}q_{30}$	Year	${}_{35}q_{30}$	Year	${}_{35}q_{15}$	
	1969.3	216	1979.9	184	1982.8	292	1981.8	109
	1969.0	253	1979.6	208	1983.3	309	1983.3	138
	1969.6	258	1980.0	206	1983.9	331	1985.1	124
	1970.5	250	1980.7	216	1985.0	335	1987.0	143
	1971.7	239	1981.7	219	1986.3	331	1989.2	180
	1973.1	219	1983.1	215	1987.9	339	1991.4	207
	1974.9	184	1984.9	195	1989.7	339		
	1977.0	173	1987.0	174	1991.8	389		
	1979.2	184	1989.2	176	1994.2	428		
Median	1971.7	219	1981.7	206	1986.3	335	1986.1	140
1982-92		1992-97						
	1987.6	191	1995.1	511				
Males								
Survival of fathers							Survival of brothers	
1982 census		1992 census		1997 ICS		1994 DHS		
Year	${}_{35}q_{30}$	Year	${}_{35}q_{30}$	Year	${}_{35}q_{30}$	Year	${}_{35}q_{15}$	
	1969.8	408	1980.3	331	1984.2	423	1981.6	156
	1970.4	418	1980.7	335	1984.9	426	1983.2	149
	1971.3	418	1981.5	341	1985.9	431	1985.0	165
	1972.6	404	1982.6	341	1987.2	435	1987.0	172
	1974.1	362	1984.1	328	1988.8	436	1989.1	207
	1975.8	355	1985.8	278	1990.6	489	1991.4	230
	1977.7	428	1987.8	307	1992.6	555		
Median	1972.6	408	1982.6	331	1987.2	435	1986.0	169
1982-92		1992-97						
	1987.6	259	1995.1	549				

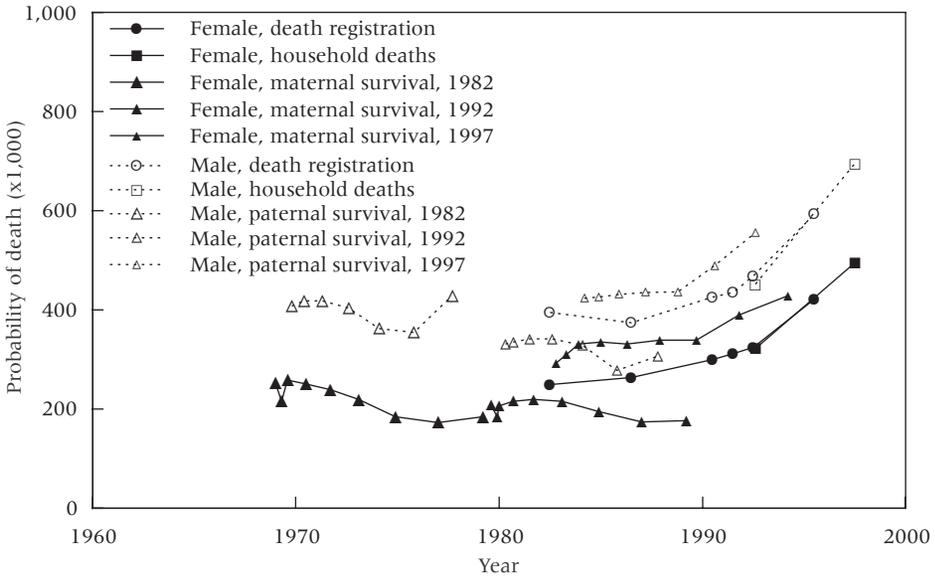
NOTE: ${}_{35}q_{30}$ denotes conditional probability of death by age 65 given survival to age 30. ${}_{35}q_{15}$ denotes conditional probability of death by age 50 given survival to age 15. The estimated probabilities of death refer to points in time, rather than, as is more usual, to calendar months or years. The ${}_{35}q_{30}$ estimate of 216 at upper left, for example, refers to time 1969.3, i.e., point in time 3/10ths of a year through calendar year 1969. See text for explanation of estimation procedures.

FIGURE 2 Male and female probabilities of death $_{35}q_{15}$: Estimates from various sources, Zimbabwe, 1982-97



SOURCE: Tables 1 and 2.

FIGURE 3 Male and female probabilities of death $_{35}q_{30}$: Estimates from various sources, Zimbabwe, 1969-97



SOURCE: Tables 1 and 2.

thers as reported in the 1982 census are broadly consistent with the death registration estimates for the mid-1980s. The estimates based on survival of mothers and fathers in the 1992 census are substantially lower, however, and the estimates based on survival of mothers and fathers in the 1997 survey are substantially higher.

Discussion

Estimated completeness of death registration rises rapidly between 1986 and 1992 (Table 1). This increase reflects the assumption that death rates for the 10–14-year age group were constant. If these death rates were in fact rising, completeness of death registration in the later years is overestimated and death rates are underestimated. This possibility suggests that the estimated increase in probabilities of death is conservative, that is, that the rate of increase indicated in Table 1 is a lower bound.

Nevertheless, the consistency of the trend derived from registered deaths (adjusted for underreporting) and the trend derived from (unadjusted) household deaths in the 1992 census and 1997 survey is surprisingly good. Since reporting errors in the household deaths data and errors in estimating changing registration completeness are largely independent, the consistency suggests that the estimated trend is approximately correct. It should be noted in this connection that the procedure for estimating the changing levels of completeness of death registration was worked out independently of the calculation of the estimates derived from the household deaths data, that is, the procedure was not varied to attain the consistency displayed in the figure.

The pattern of the increase in age-specific death rates between 1986 and 1995–97 (Figure 1) is as important as the magnitude. The sharp rise and fall of death rates between ages 20 and 60 departs radically from the normal age pattern of human mortality in showing extremely high mortality for young and middle-aged adults. The relatively erratic age pattern of rates from the 1997 survey suggests that age reporting in the survey is less accurate than age reporting on death certificates.

The age-specific death probabilities reported in Gregson et al. (1997) are for a small part of Zimbabwe and so are not comparable to the age-specific death rates in Table 1, but the magnitudes and the age pattern are broadly comparable.

The conditional probabilities of death estimated from survival of siblings agree well with, but tend to be slightly higher than, the estimates from vital registration (Figure 2). The male and female estimates both confirm the rising trend. Death rates for the seven-year period prior to the 1994 DHS derived from sibling histories are given in Bicego (1997). Interpolation and averaging death rates in Table 1 to give rates for the same period yields

comparable rates. The rates given in Bicego are about 20 percent lower than the corresponding rates derived from Table 1.

The discrepancy between the parental survival estimates from the 1992 census and the 1997 survey is striking. The two series overlap for nearly a decade, during which the 1997 estimates greatly exceed the 1992 estimates and rise rather than fall. The difference in level between the two sets of estimates must be due in substantial part to differences in the quality of reporting of survivorship of parents in the 1992 census and the 1997 survey. The discrepancy might also reflect a lack of robustness in the dating procedure in the face of rapidly rising mortality.

Zimbabwe experienced a massive rise in adult mortality risks from the mid-1980s to the mid-1990s. During the 15-year period 1982–97 the conditional probability of death by age 60 given survival to age 15 rises from 0.20 in 1982 to 0.50 in 1997 for females and from 0.31 to 0.65 for males. The increase accelerates throughout this period, suggesting that probabilities of death will rise further before leveling off and declining. There is essentially no doubt that these increases in adult mortality risks are due to HIV/AIDS. Given the timing, age pattern, and magnitude of the changes, no competing explanation is remotely credible.

Note

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R. F. Harrod on Reviving the Birth Rate

Continuing below-replacement fertility and projected declines in population size are demographic features of many European countries and Japan. They are variously met with complacent acceptance, calls for higher rates of immigration, or—often last and least—proposals for increasing the birth rate. Fertility was also low in the 1930s, and some of the policy debate from that period resonates today. In England and Wales, fertility then had been declining for half a century. Over the decade 1931–40, it averaged 1.8 children per woman—moreover, with net emigration. Worries over this situation and its likely consequences led to the setting up in 1944 of a Royal Commission on Population, charged with considering “what measures, if any, should be taken in the national interest to influence the future trend in population.” In a memorandum submitted to the Commission in that year, the economist R. F. Harrod set out a detailed proposal to encourage childbearing through a scheme of family endowments. Part of the introductory section of Harrod’s submission, arguing the case for state intervention and for material rather than “spiritual” measures, is reproduced below.

An evident problem in offering economic incentives for childbearing is that, to induce a given behavioral change, well-off families would require much larger incentives than the poor. Hence child endowments that aspire to effectiveness across the income distribution have to be skewed toward the upper end. Harrod argues that this is as it should be, that policy should establish neutrality between large and small family sizes, and that this is a conceptually separate issue from poverty alleviation. “We should seek a re-distribution of national income favourable to the parents of larger families and the plan should be put into effect whether or not another re-distribution as between rich and poor is proceeding at the same time.” He remarks on the implausibility of the government’s being able to “talk up” fertility—thereby generating some kind of costless ideational change, a “spiritual aufklärung.”

Later parts of the submission not reprinted here cover the specific details of the proposal. The proposed annual benefit per child (intended for every child after the

second, with half-rates payable for the second child) is paid for 18 years. It is substantial and increases with the child's age—at ages 13–18, for most of the income range it amounts to 20–30 percent of the father's income (or mother's, if hers is higher). Harrod also discusses further the rationale for making the endowments (and the compulsory contributions—a flat 5 percent of income—that finance them) proportional to income. To make his case Harrod draws on the dysgenic and population-quality arguments popular at the time: worry about “race decline” and “a general lowering of standards and of efficiency if the parents who are best equipped in experience, knowledge and culture are relatively infertile.”

In the event, the Commission recommended a flat schedule of family allowances, together with tax exemptions for dependent children calculated to provide some income-based benefit. These were justified on population as well as equity and welfare grounds, “since the handicaps of parenthood have played a large part in the fall of average family size below replacement level.” Population quality issues—the subject of several other submissions—were sidestepped by calling for further research. By the time the Commission's report was finally published, in 1949, the baby boom was well underway: average fertility over 1946–50 was 2.4.

Roy Forbes Harrod (1900–78) was one of the foremost economists of his day. His career was largely spent at Christ Church College, Oxford. A student and sometime colleague of Keynes, his best-known early work was centered on identifying a dynamic equilibrium growth path for the economy—building on Keynes's static equilibrium analysis. As stylized (by others), this came to be called the Harrod–Domar growth model, a formulation basic to growth theory. Harrod was editor of the *Economic Journal* for the period 1945–66. He was active in politics and as an economic adviser to both Labour and Conservative governments. He was knighted in 1959.

The extract is reprinted from volume 5 of the Papers of the Royal Commission on Population (London: His Majesty's Stationery Office, 1950), pp. 80–85.

We cannot afford delay

5. If we are to avoid great losses in numbers the rise in the net reproduction rate must come quickly. We have leeway to make up, since it has been below the survival level for some twenty years. The comparative stationariness that has been predicted for our population during the next thirty years, should reproduction remain at the same level and the downward trend in mortality be continued, masks a decline of about one-quarter in the under-45 section of the population which is due to occur on those hypotheses. Thus even if the net reproduction rate rose suddenly to unity at the end of thirty years a loss of many millions would occur thereafter. But it is not likely to rise suddenly to unity in a given year and so the subsequent losses would probably be greater still. If we are to avoid large losses, it must reach unity in much less than one generation from now, and it is hardly likely to do that unless the rise begins immediately. These

propositions take no account of the loss of young manhood in the present war, which will seriously aggravate the situation. In these circumstances we cannot afford to allow ourselves a further period in which to wait and see.

6. Secondly, the proposal I put forward may be recommended by its inherent justice as well as its tendency to encourage reproduction. Consequently even if we could be sure, as we certainly cannot be, that the alleged change of feeling would be sufficiently potent to secure survival, there would still be a case for adopting the plan. The fact of the matter is that parents as a class and mothers in particular have had less than justice in our economic arrangements. Parents of larger families have been under greater economic pressure than other members of the community. This has always been unjust in itself, but justice often has to give way to expediency, and in this case the tendency of the race to reproduce itself too rapidly, especially after the decline in mortality in the late eighteenth century, had to be kept in check to prevent over-population. Now that the tendency no longer exists we may redress the balance without peril in favour of parenthood.

Return to old birth-rate not desirable

7. It may be worth stating at the outset that we cannot contemplate a return to the old level of gross reproduction. Malthus was quite right in supposing that it is impossible to combine the low mortality rates which obtain in civilised conditions, with the birth-rate which would result from giving free rein to reproductive instincts. The geometric increase would soon lead to grotesque over-crowding. Birth control in its widest sense is a pre-condition of continued civilisation. Thus it would be wrong as well as fruitless to tilt against the modern idea of deliberately planning the size of the family. It is not required that normal parents should think in terms of a family of six or eight, but rather of four or five, although the more philoprogenitive parents may be encouraged to contribute more than their fair share to the next generation, subject to humane regard for the health of the mother.

Two governing principles

8. In devising remedies to meet the serious situation I submit that two principles should be applied, namely, (i) they should be adequate in scale and (ii) they should be national in scope.

(i) Remedy must be adequate in scale

9. If we take a broad survey of history and pre-history, we find a great variety of social and economic arrangements some of which were favourable, others unfavourable to reproduction. It is very probable that there

was a causal connexion between the type of social system in operation and the contemporary tendency to over- or under-population; Malthus' work failed to stimulate as much enquiry into these problems, as their interest and importance deserve; there is an almost virgin field here for the anthropologist with a biological and mathematical training. Examples are infanticide, and the system of serfdom, under which children were unable to escape from the hereditary plot of land, so that, it being impossible to enlarge the productive resources available to the family, there was a strong prudential motive to limit its size. In historic times it may well be that the Christian condemnation of infanticide was responsible for the increase of serfdom which followed it and provided an alternative check to the undue growth of numbers. On the other hand systems have from time to time prevailed which were especially favourable to reproduction. Thus where there was a strong clan nexus and flexible territorial arrangements, the more reproductive families would by their superior power be able to gain not merely elbow-room for their larger numbers but a larger quantity of land per head. It is against the background of these great variations in fundamental institutions affecting reproduction that one should think of the proper scale of remedy to meet the present situation.

10. It is most improbable that these various customs and institutions were consciously devised in order to stimulate or retard population growth. They probably came about by natural or social selection in reaction to a change in the environment favourable or unfavourable to population growth. Such changes might be climatic or bacteriological or due to new methods of cultivation, providing greater means of subsistence—or smaller means, when disturbing the ecological balance—or change might be due to the mere lapse of time as when a group having increased for a number of generations began for the first time to press upon the means of subsistence. In the new situation if one community had a variant in its social system, favourable to larger or smaller numbers as the case required, it might survive and replace its neighbours by ordinary natural selection, or through its superior power and prestige causing its neighbours to copy the favourable variant. It is possible that human survival will once again be secured by one or other of these selective processes. But this is not a solution that we can view with satisfaction, if the more favourable arrangements originate elsewhere. Consequently the task before us is a new one, namely, to bring about with conscious intent and by deliberate planning the kind of fundamental social adjustment that has probably hitherto in human history been brought about by a selective process. It may seem a formidable task, but is also, I submit, an inspiring one, and must fill us with a supreme sense of responsibility.

11. The idea that it would not be satisfactory to wait for other peoples to give the lead in reviving births does not rest only on the narrow nationalist prejudice. Not that we should altogether despise such prejudice—

the events of 1940 have demonstrated the strength and revived the prestige of nationalism in this country, in the sense of a determination to remain masters in our own house. But there is an argument to meet even those who refuse to be impressed with the importance of the mere maintenance of British stock and independence. New institutions favourable to reproduction may well come to birth not in isolation but as part of a wider ideology. For example, it is by no means proved that the Nazi ideology is favourable to reproduction in the long run. But if it is favourable, and if free countries do not of their own volition adapt their social system to the need for higher reproduction, then, despite its defeat in this war, Nazi ideology, or something similar, will triumph in the end. It would be a sad story if our mighty efforts in war were rendered of no avail by the mere failure to reproduce our kind. The free democratic system relies largely on the power of reason and discussion to secure necessary reforms. This population question is the acid test of its power. Unless the free societies which seek to solve their problems by reason are able to maintain their stock, they will in due course be effaced or dominated by others. This might mean another dark interlude in human history.

(ii) Remedy must be national in scope

12. The incentive to larger families should be national in scope. We need a revival among all British men and women whatever their party, creed or walk in life. Our plans should be of universal application, and neutral as between conflicting views about the ideal economic organization of society. Thus we should seek a re-distribution of national income favourable to the parents of larger families and the plan should be put into effect whether or not another re-distribution as between rich and poor is proceeding at the same time. The issues are quite distinct in idea and should be kept distinct in practice. Sir William Beveridge's plan for an allowance of 8s. a week for each child in excess of one is excellent in itself, but is not a solution of our problem. It will not appeal to the ordinary man as adequate in scale when assessed against the background of ideas set out in paras. 9–11. And since it was conceived in accordance with his terms of reference as a protection against "want", it is unlikely to solve the problems of those who would not in its absence experience want; thus its operative effect would be sectional, not national, in scope.

Two points concerning causes of low reproduction

13. I do not propose to enter into the very deep question of the causes of the fall in the birth-rate, save to make two points, one negative and one positive, which are immediately relevant to my proposal.

(i) Fall in reproduction not due to hard conditions

14. It is often urged that parents have been caused to reduce the size of their families by the *facts* of insufficient livelihood, social insecurity and the fear of war. This is not a plausible explanation. Whether we compare birth-rates in different countries, different periods or different income strata, it does not appear that a hard lot in life is inimical to reproduction. Rather the other way. This is a very important point. If one accepted the hard luck story, one might build one's hopes for a revival in births on the general improvement in living standards, employment prospects and international security which we are determined to achieve when this war is over. But if an easier and more secure lot in life is not in itself favourable to reproduction, then we must resolutely discard the hope that an improvement in general conditions will encourage births and we must seek other means. This conclusion is not inconsistent with the view that the *ideas* of insufficient livelihood and insecurity are coming to have a more important influence on parents. The fact of the matter seems to be that as man's actual condition improves, his idea of what is due to him and necessary to his existence rises more rapidly; his ambition always tends to outstrip his achievement. It is a paradox, but well borne out in everyday experience, that the better off people become the more they complain of not having enough money. In regard to the fear of war, it should be observed that this was not very widespread in Great Britain either before 1914 or between 1919 and 1933, the periods of great decline. It is no good bemoaning the tendency of man's requirements ever to outstrip his means of satisfying them. We are not likely to succeed in extirpating this trait in human nature, nor should we wish to do so, for it is the motive power of progress. If our people ever became content with their lot, that would be a sure symptom of imminent decadence.

A widespread fallacy

15. At this point notice must be taken of a widespread fallacy. It is often argued that if being better off does not induce people to have more children, family endowment will not do so. The fallacy is crude but prevalent. It is perfectly consistent to hold that an improvement in the average *absolute* standard of living will not encourage births and at the same time to argue that an improvement in the *relative* condition of parents of larger families will do so. If it is seen that the parents of larger families are more prosperous and more secure than the less reproductive members of the community, that will be an inducement to people to achieve that privileged status.

(ii) “Social security” favourable to spread of low reproduction

16. There is one change in social conditions to which we may safely—in so far as any statement in this field is safe—ascrcribe the wide diffusion of a low reproduction rate, namely, the public provision of “social security”. Among the mass of people in the past children have been the principal or sole means of insurance against want. For instance, if there were six children, two might fail to make good in life, another two might be stony-hearted and indifferent to their parents, but it would be very bad luck if two out of the six were not found with means and affection sufficient to keep their parents out of the work-house. The parents of but two children would be in a much more precarious position. As one cause of want after another, industrial accident, old age, sickness, unemployment, have come under the care of the public, the need to insure oneself by having children has faded out. Not only were parents of large families in fact better placed to face the vicissitudes of life, but also were seen to be, and thus became objects of envy to their neighbours. Now so much is the situation changed that they have become objects of pity and even sometimes, it is to be feared, of contempt, for being simpletons.

17. The argument that “social security” is a cause of infertility is not designed to cast any slur upon social security. On the contrary, the case for that reform is unshakable and it would be a slur on any nation, which had achieved the general standard of living of our own, if it did not provide in full measure for the needy. After all, insurance by having children was a rough and ready method which left those who could not have large families in an exposed position. We have reached a level of well-being at which we must regard such a method as intolerable. But the point is that if one introduces a very far-reaching change (like social security) in the balance of forces which maintain society in being, one must be on the look-out for the necessity of compensating changes. The physiologist well knows what a delicate balance of forces is the human organism; the ecologist understands the intimate inter-connexion of animal life, plant life, methods of cultivation and the water-table and that a disturbance in the balance may have most far-reaching consequences; it would be foolish to suppose that the balance of forces in the social organism is any less delicate, but unhappily sociology has not yet found its feet as a scientific study. Still, we may venture as far as the elementary proposition that if the primary self-regarding motive for having children is removed, there is danger to survival if a substitute is not found.

18. In touching on the causes of the fall in reproduction, I have confined myself to two points, one negative and one positive, which seem to

bear directly on the plan which I have to submit. The general question of the causation is a vast subject and to enter on it would prolong this introduction beyond reasonable limits. But while I have not referred to numerous other causal factors, it must not be supposed that I have not had them in mind nor given them weight in considering remedies.

Spiritual versus material remedy

19. The question is sometimes raised whether we should put more emphasis on a spiritual remedy or a material remedy for the present state of affairs. Those who stress the spiritual side sometimes argue that without a prior change of heart the material remedy will be of no avail and therefore should not be pursued or at least should be postponed until we see the effect of a spiritual change. I do not share this conclusion for the following reasons. In the first place the intrinsic desire for children must be so deeply-rooted in the constitution of our species and has played such an important role through countless vicissitudes, that it must surely be regarded as an abiding element. Some hold that the fall in reproduction is due to the growth of selfishness and the decline of parental love. On biological principles it is most unlikely that there has been a loss of strength in the fundamental parental instincts in the last two generations. Self-righteous persons in every age have railed against the growth of selfishness in their own; but their testimony is unreliable and taken in the aggregate self-contradictory. The fall in reproduction may be accounted for by the great changes in the environment which have occurred, and there is no need to resort to the improbable hypothesis of a change in human nature. But if it is needlessly cynical to suppose a decline in parental love, it is unscientifically optimistic to imagine that it can be whipped up above its normal level by spiritual exhortation. Parental love is a force of great but not infinite potency. If excessive material obstacles are put in its way it may be overcome. What we need, to secure survival, is that parental love and more self-regarding motives should work together in harmony to encourage parenthood, as they have done in the past. It is wrong to put too much strain on the altruistic elements in human nature; society should be so arranged that self-regarding motives play some part in getting fulfilment of the two basic tasks of doing an honest job of work for society and providing the next generation. The better that is arranged, the more can the fund of altruism be released for works of supererogation, kindness to the needy and suffering, the pursuit of new ideas and new adventures, and all the other good works on which we depend for the progressive amelioration and refinement of the human race.

20. Secondly even if a spiritual aufklärung were capable of having a marked effect on reproduction, it would not be within the power of the

public authority to bring it about. Official propaganda would be of little use: indeed it is probable that most British citizens would greatly resent any official exhortation on these very personal matters. It is true that a man of genius, a Dickens or a Mr. H. G. Wells, may have a profound influence on the way in which people regard their own private affairs, and their attitude to life. But we do not know if there are such men of genius among the coming generation and it is most unlikely that, if there are, they would be willing to employ that genius to a given purpose on an official suggestion.

21. Thirdly, I suggest that far the most effective method open to the state to bring about a spiritual change is by applying a material remedy. The average citizen will not be impressed by propaganda, but he will be impressed by action. If a Royal Commission recommended and a British Parliament enacted a far-reaching measure to provide a basis for family life, that would be an event likely to strike the imagination. It would lead to heart searching and intimate discussion. Once serious thought is provoked we may rely on the conscience and reason of each individual to secure some revision of our scale of values. Whatever his religious convictions or moral code, the case for handing on the torch is so strong that he will recognize it, if he has any principles at all. I submit that the natural order of events is as follows. Public discussion, such as is now taking place, is necessary to get sufficient public opinion behind a Parliamentary measure; but this discussion is likely to have little influence on private conduct. The primary purpose of the measure must be to provide a material remedy, but if the measure provokes thought at a deeper level it may have some influence on the individual's notion of his duty. Whether this spiritual bye-product will be important or not, it is difficult to judge. I submit that it is the only way in which the state could bring about a spiritual change, and, if this is so, it means that the need for a change of heart reinforces the case for early Parliamentary action on the material plane.

How Long Can We Live? A Review Essay*

JOHN R. WILMOTH

The quest for a longer, healthier life is the subject of a recent book by Jay Olshansky and Bruce Carnes. Written for both popular and professional devotees of the subject, the book is lucid and eminently readable. Personal anecdotes illustrate key points, adding warmth and charm to the discussion and spicing up topics that might otherwise deter all but the most serious reader. I enjoyed reading this book and learned a great deal from it. It does a great service by debunking the plethora of claims about potions, treatments, lifestyles, and even biomedical discoveries that promise to halt aging and make us all young again. In spite of its many good qualities, however, I did differ with some of the main points and was surprised by the authors' lopsided portrayal of demographic research on mortality decline and human aging.

During the past decade or longer, the authors and their colleagues have influenced the popular and professional discussion about human aging and mortality trends in various ways. One positive contribution has been educational, as they have explained key technical concepts to nonspecialists in clear terms. In this book as well, they offer helpful discussions of evolutionary theories of aging, related biomedical issues (antioxidants, Darwinian medicine, etc.), and historical trends in mortality and health. Chapter 1 also includes an informative overview of the history of ideas about aging, disease, and death. Over the years another contribution has been the publicity Olshansky and Carnes have brought to this discussion by offering a conservative view about prospects for further increases in human life expectancy. Although some demographers may disagree with aspects of their technical

*Review of S. Jay Olshansky and Bruce A. Carnes, *The Quest for Immortality: Science at the Frontiers of Aging*. New York: W.W. Norton, 2001. 254 p. \$25.95; \$14.95 (pbk.).

arguments or their conclusions, most would probably share the authors' skepticism with regard to commercial products claimed to retard aging and thus significantly extend human longevity.

The core argument

The book covers many topics, but in this review I focus on the issue seemingly most relevant to demographers—namely, prospects for future mortality decline. In this regard, the core argument advanced in this book (and in the technical papers on which it is based) can be summarized roughly as follows. Natural selection has shaped the mortality patterns of humans and other species, resulting in a universal age pattern of “intrinsic” mortality (what remains, theoretically, after all environmental and behavioral influences are removed). This evolutionary legacy of intrinsic mortality imposes strict limits on the amount of mortality reduction that can be achieved merely by manipulating the environment in which humans live or by regulating their personal behaviors. Therefore, further reductions in death rates beyond these “natural” limits can come only through high-tech biomedical interventions that “manufacture survival time.” Even if we push beyond these biological limits, however, the rise of life expectancy must slow down in the future because of a force known as “entropy in the life table.”

The theoretical and empirical pillars of these arguments are the evolutionary theory of senescence developed by eminent biologists (such as Weismann, Medawar, Williams, and Kirkwood), the authors' own analyses of mortality data for humans and other species, and fundamental mathematical relationships linking death rates and life expectancy (i.e., entropy). In several parts of the book, these concepts are accompanied by analogies drawn from everyday life (race cars, sports records, warranty periods), which give intuitive appeal to the arguments but offer no real scientific support.

Evolutionary theory

In reviewing the evolutionary theory of aging in Chapter 2, the authors have summarized an important body of literature. Later they invoke evolutionary arguments to explain why “sickness, aging, and death make perfect sense for living machines with body designs that were never intended to be tested in the laboratory of extended life” (p. 129). They take the argument a step further, attempting to link theories of the evolution of aging to the diseases that shape modern mortality patterns. “This genetic legacy of biological responses to the hostile environments of our ancestors is directly responsible for the diseases we experience today, and the ages at which they appear” (p. 76). Thus, according to the theory, we are susceptible to late-life disease and mortality because the blind filter of evolution does not

care about postreproductive survival. But what does this conclusion about a *pattern* of increasing age-related susceptibility tell us about *levels* of human morbidity and mortality?

Consider cardiovascular disease, the number-one killer in most industrialized nations. This disease is extremely rare among elderly survivors in hunter-gatherer populations, because they do not follow the high-fat diet and sedentary lifestyle common in highly developed countries today (Polunin 1967; Howell 1979). Thus, even if heart disease and stroke become more common with age because of increased susceptibility, their elevated prevalence in modern populations is by no means the inevitable result of our “genetic legacy.” Similar arguments can be made for other major diseases, such as cancer. Although I agree with the authors that aging and associated diseases “are not an exclusive by-product of decadent lifestyles” (p. 78), their emphasis on the evolutionary and genetic origins of such conditions goes much too far to the other extreme. Evolution may have shaped the *age pattern* of human mortality, but current conditions and an individual’s life history heavily influence its *level*, even in old age.

Laws of mortality

Evolutionary arguments are augmented in Chapter 3 by the authors’ own work on the demography of humans and other species. As always, one must applaud any effort to answer fundamental questions about the origins and character of human mortality patterns. Indeed, if one could document a universal mortality pattern across all species (or even within the mammalian class), this would be a finding of the utmost significance. The authors are convinced that such a pattern exists, if we focus exclusively on intrinsic causes of death (Carnes, Olshansky, and Grahn 1996). This is an interesting theory that bears consideration, but it is presented in the book as though it were an established scientific fact. For example, we learn about these ideas in a section of Chapter 3 titled “The law of mortality.”

This quaint terminology is often associated with the mortality curve proposed by the nineteenth-century English actuary, Benjamin Gompertz, or with related curves offered by his intellectual successors. By analogy to similar terms from the physical realm (e.g., the “law of gravity”), it suggests that organisms die according to rules that are fixed and unchanging. Indeed, the book claims that the implications of a law of mortality are “staggering,” since its mere existence “implies that there are limits to how long humans and other organisms can live, and biological reasons for why such limits exist” (p. 89). Such a statement is utterly naive, however, because the parameters of the Gompertz curve (or of any other so-called law of mortality) can be manipulated to produce any level of life expectancy (or other summary index of mortality). Again, the existence of a general *pattern* says nothing about the specific *level* of mortality.

Entropy in the life table

In addition to discussions of evolutionary theory and a hypothetical law of intrinsic mortality, the book offers a mathematical justification for the contention that the rise in life expectancy must slow down. Even if we succeed in “manufacturing” survival time beyond the limits imposed by human biology, the authors claim that future gains in life expectancy will be smaller and smaller because of “the inescapable reality of entropy in the life table” (p. 88). The book states that “[g]ains in life expectancy are already slowing down and entropy in the life table ensures that future progress will be even slower” (p. 87). The second half of this statement is incorrect. Although entropy was closely associated with the deceleration in the rise of life expectancy during the twentieth century, it has little relevance for future trends.

Briefly, the entropy of any physical system is a measure of its uncertainty. In a life table, high entropy means that deaths are spread over a broad age range. An important result from formal demography is that mortality decline in the presence of high entropy is associated with a very rapid rise in life expectancy, since a high proportion of the deaths averted would have occurred among infants and children. Because mortality risks in adolescence and young adulthood are relatively low, mortality decline at the youngest ages yields a much higher return (in terms of life expectancy at birth) than mortality decline at older ages. In situations of low entropy, deaths are concentrated at older ages and the return from an equivalent proportional mortality reduction is considerably less.

The historic rise in life expectancy was accompanied by a large reduction in the degree of entropy in the life table. A critical point, however, is that it was not entropy *per se*, but rather the *decline* of entropy that produced a slowdown in the rise of life expectancy at birth. As entropy fell precipitously during the twentieth century, the return on reductions in death rates decreased in tandem, and thus the increasing trend in life expectancy decelerated. During recent decades this entropy decline has slowed down, making entropy a less significant factor in life expectancy trends during the late twentieth century than earlier. In the future entropy is expected to stabilize at very low levels,¹ and thus the relationship between mortality decline and gains in life expectancy should stabilize as well. For this reason it is incorrect to claim that entropy in the life table implies a continuing slowdown in the rise of life expectancy (see also Vaupel 1986).

Another important point is that the effect of changing entropy on trends in life expectancy is implicit in any forecast based on extrapolations of age-specific mortality rates, which is the standard method used by demographers. The authors seem to misunderstand the very rudiments of demographic forecasting when they suggest that a “commonly used” technique assumes a linear increase in life expectancy itself (p. 96). This misleading

depiction of their colleagues' work opens the door for them to ask indignantly, "Why would scientists who know about entropy in the life table generate such overly optimistic estimates for the future course of life expectancy?" (p. 97).

Indeed, the authors' confusion about the concept of entropy in the life table clouds their entire discussion of mortality forecasts. For example, in discussing an extrapolative forecast that assumes a 2 percent rate of mortality decline across all ages, Olshansky and Carnes assert incorrectly that "entropy in the life table poses a nearly insurmountable obstacle to the 2 percent assumption" (p. 98). As explained above, entropy measures the increase in life expectancy associated with a given decline in death rates. It says nothing about whether an assumed level of mortality decline is achievable or not.

Aside from these conceptual errors, the trend in entropy is only one piece of the trend in life expectancy. The other factor that matters is the rate of mortality decline. Regrettably, the book overlooks a very significant event in the mortality history of highly developed countries during the last decades of the twentieth century: the accelerated pace of mortality decline at older ages, owing to lifestyle changes and medical progress (Kannisto et al. 1994). This more rapid decrease in death rates, combined with a slower decline in entropy, has led to a stable increase in life expectancy at birth, albeit at a slower pace than during the early twentieth century.

The evils of "prolongevity"

The authors use the label "prolongevist" to refer to anyone who is more optimistic about the future of human longevity than they are. Their definition of the term itself is uncontroversial, and it is hard to believe that the authors do not count themselves among those who "believe that aging and death are amenable to modification, and that longevity can be extended through human intervention" (p. 35). However, as used in context it is clearly intended as a derisive term associated in the past with "alchemists" and "hucksters." In modern guise, prolongevists include vitamin vendors, spiritual healers, and overly enthusiastic biomedical researchers, who tempt us with promises of 150- or even 200-year life spans. The authors distinguish three branches of "prolongevity" and note that it takes on extreme and moderate forms. But these distinctions are blurred through most of the discussion, and the book often reads like a holy crusade against the evils of longevity in all its manifestations.

One such manifestation, apparently, is modern demographic research. Olshansky and Carnes seem to include almost any demographer who has ever written anything about this topic in the camp of prolongevists. For example, demographers who reject the existence of "biologically imposed upper limits" on the human life span are called "the most zealous advocates

of prolongevity" (p. 134). The authors also take aim at demographers who "have developed mathematical models to make forecasts of life expectancy in the future," claiming that such methods "have led some researchers to predict that life expectancy will soon rise to 100 years or higher" (p. 93). Which demographers have made such claims? The book contains no bibliographic references, and the text generally does not cite the names of individuals whose work or ideas are criticized. Therefore, it is worth examining the references that accompany a similar statement in a recent article by the authors in the journal *Science* (Olshansky, Carnes, and Désesquelles 2001).²

In short, the authors have created a straw man by misquoting four teams of demographic researchers: Manton, Stallard, and Tolley (1991), Vaupel and Gowan (1986), Wilmoth et al. (2000), and Lee and Carter (1992).³ *None* of these four articles predicts that life expectancy at birth will exceed 100 years anytime in the future.⁴ For example, the most widely cited of these works among specialists in mortality forecasting is undoubtedly the article by Lee and Carter. It anticipates a life expectancy at birth of 86 years in 2065 (for the US) but explicitly rejects higher estimates, noting that "for life expectancy to rise to such a high value as 100 by 2065 would require a radical break in historical trends" (Lee and Carter 1992: 668). Adopting their own tone of indignation, one might ask: Why would scientists who have followed the debate about human life expectancy so closely make such patently false statements about the work of their colleagues?

Mortality limits

In addition to questions about the future of life expectancy, the debate about possible "limits" to the human life span also figures prominently in the book. Here, a difference of perspectives derives partly from divergent definitions of what is meant by a mortality limit. Such a limit usually means either the maximum or the average length of life (in a large human population) that can be achieved by all possible means but never surpassed. These "possible means" include medical breakthroughs as well as other factors, although some medical interventions might be excluded from the definition of a limit if they involve fundamental alterations of the genetic code that defines us as a species. This is the classic definition of a mortality limit, but whether or not such a maximum exists is a different issue (Wilmoth 1997).

The definition of a mortality limit used in this book (and elsewhere by the authors) is quite different. Most notably, the definition is conditional: a limit is "how high life expectancy can climb *without some sort of medical intervention*" (p. 92; emphasis added). To some people, this may seem like a limit that is not really a limit. But even accepting the concept of a movable limit, confusion reigns supreme in the book's discussion of this topic. On the one hand, the authors assert that their conditional limit is 85 years (p. 117, p. 181), but that future advances in biomedical sciences may take life expect-

ancy beyond this level (p. 181). On the other hand, they claim that a biological limit to life expectancy “*has already effectively been surpassed in some populations*” (p. 134; emphasis in original). These statements cannot refer to the same limit, however, because no country has thus far exceeded a life expectancy of 85 years for the total population.

Of course, Olshansky and Carnes can propose any definition they like, but there is a fundamental problem with their current approach. The authors strongly criticize other demographers while ignoring fundamental differences in definitions. They claim that demographic forecasts do not reflect “biological constraints that limit the duration of life” (p. 99), but in making such statements they assume an absence of medical innovation. Forecasts by other demographers assume implicitly that medical progress will continue into the future. Perhaps both approaches have merit, but this difference should be acknowledged explicitly as a source of disagreement. In reality, the difference of opinion between the authors and other demographers about likely future trends in life expectancy is much smaller than they suggest in their book.

Policy implications

Overall, this is a thought-provoking book, even if one does not accept its main arguments. Furthermore, its policy implications are nontrivial: If our goal is to extend human longevity further, then biomedical research should concentrate on re-engineering human bodies and rewriting the human genome (see also Olshansky, Carnes, and Butler 2001). An individual’s health and well-being may benefit from a good diet, regular exercise, low stress, and other lifestyle factors (Chapter 10). However, because of entropy in the life table and the immovable wall of intrinsic mortality, life expectancy for society as a whole can gain little from further advances in public health, personal behaviors, or improved nutrition. We have already gone beyond the limits imposed by our biological heritage thanks to survival time that has been “manufactured” by medical science. Further gains may come from “[u]nderstanding gene structure, function, and regulation, and learning how to modify and manipulate these genetic attributes in order to preserve and enhance health and extend life” (p. 152).

I was struck by the genetic and biomedical reductionism that dominates the book’s depiction of human mortality patterns. The discussion of the role of medicine in mortality reduction emphasizes invasive procedures and heroic interventions (for example, pp. 119–124), and there is no mention of the hypothesis that disease reduction and improved nutrition at younger ages many decades ago may have contributed to improved health and lower mortality at older ages in recent years (Manton, Stallard, and Corder 1997; Barker 1998; Costa 2000). Furthermore, because of their belief in genetically determined intrinsic mortality, the authors have excluded

public health and behavioral modification from future mortality reductions: “Short of medical interventions that manufacture survival time, there is very little you can do as an individual to extend the latent potential for longevity that was present at your conception” (p. 236).

On the one hand, I agree with the authors that much (perhaps most) of the gain in life expectancy since around 1970 in developed countries is attributable to modern medical care. On the other hand, it is important to recall that some of the most effective medical interventions occur before the onset of life-threatening disease—for example, the preventive control of hypertension and high cholesterol via prescription drugs and medically informed dietary modification. From my perspective, these are extensions of a long-term process of changing human habits. In such cases nothing has been “manufactured” by “medical miracle-makers”—we are merely learning how to care better for our bodies and thus to avoid (or postpone) disease. These sorts of changes will no doubt continue, and they will contribute (along with numerous other factors) to a continued rise in life expectancy.

Conclusion

I think that Olshansky and Carnes underestimate the plasticity of the human organism in response to changing environmental and behavioral conditions. Their bold conclusions about the immutability of intrinsic mortality are not well justified by either the theoretical literature they cite or the data analyses they have performed. Their arguments about entropy in the life table are simply wrong, and their portrayal of the research of other demographers is inexplicably negative. Finally, I find their habit of separating medical interventions from other manipulations when talking about mortality limits to be unjustified and confusing semantic gymnastics. In spite of these criticisms, the book is well written and entertaining, and offers a broad coverage of a complex field. I commend the authors for this achievement.

Notes

The reviewer thanks Shiro Horiuchi of Rockefeller University for helpful comments on an earlier draft of this essay.

1 Using mortality forecasts for the United States from the Social Security Administration, my calculations show that the decline in life table entropy will reach very low levels (less than 0.5 percent per year) after 2008.

2 Olshansky, Carnes, and Désesquelles (2001) state that “[t]wo methods have been used in recent years to predict that life expectancy at birth will reach 100 years in the 21st

century.” As in the book, they mention both a risk-factor model (developed by Kenneth Manton and his colleagues) and extrapolative mortality forecasts (a traditional technique in demography). To say that life expectancy will exceed 100 years sometime “in the 21st century” is more plausible than to suggest that it will “soon rise” to this level. Whichever phrasing is used, however, the articles cited by the authors do not in fact make such claims.

3 Even without bibliographic references, the book contains unmistakable references to

these and similar publications by Manton and colleagues (pp. 93–95) and Vaupel and colleagues (pp. 97–98).

4 The study by Manton and colleagues (1991) considers different methods of computing limits to human life expectancy. One of these involves a theoretical model linking mortality with various risk factors. The analysis concludes that “a life expectancy of 95 to 100 years (with a standard deviation of about 10 years) might be achieved by ‘optimal’ risk-factor interventions” (p. 628). Their last paragraph notes also that “a life expectancy of 100 years has implications for the Social Security and Medicare Trust Funds, private pension systems, health insurance, and the health care system” (p. 631). Thus, although the article describes the risk-factor profile that would be needed to achieve a life expectancy of just under 100 years, and although it considers the social implications of such a development, it does not predict that life expectancy in any population will exceed 100 years at any time in the future. The article by Vaupel and Gowan (1986)

explores the impact of different mortality trends on population age structure. The main results consist of population distributions under various scenarios, whose implications for life expectancy are mentioned only briefly at the end of the article. The article contains a section discussing the difference between “insight, prediction and projection,” and the authors state that “the purpose of the calculations was neither prediction nor projection, but insight” (p. 432). The article by Wilmoth et al. (2000) documents an increase in the maximum age at death in Sweden from 1861 to 1999, but it does not even contain the words “life expectancy.” In its concluding paragraph, it suggests that a continuation of this upward trend seems likely in the future, but it contains no prediction or projection involving specific dates or levels. The mortality forecasts by Lee and Carter (1992) yield a life expectancy at birth of 86 years in 2065 (for the United States) with a confidence band from (approximately) 80 to 90 years.

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BJØRN LOMBORG

The Skeptical Environmentalist: Measuring the Real State of the World

Cambridge: Cambridge University Press, 2001. xxiii + 515 p. \$69.95; \$27.95 (pbk.).

Numerous books by environmentalists assert the parlous condition of the natural world under its continuing human onslaught. Those looking at the bright side, or questioning the dire interpretations and conclusions, are fewer but heavier. In 1984, Julian Simon and Herman Kahn responded to the Carter Administration's downbeat *Global 2000 Report* with their volume *The Resourceful Earth* at 585 pages and its successor, Simon's *The State of Humanity* (1995), at 694 pages; Gregg Easterbrook's *A Moment on the Earth* (1995) made breezier reading, but outdid both of them in length. Now, Bjørn Lomborg also weighs in at over 500 pages—150 of which are notes and bibliography. The book appeared in Danish in 1998. This is a revised and updated version, "partially translated" by Hugh Matthews. It has a lucid style and is attractively presented. It has many excellent charts, picking out simple but illuminating trends. In a novel touch the endnotes are numbered consecutively, 1 to 2930—at least making for ease in finding them.

Lomborg is a professor of statistics at the University of Aarhus, Denmark. Not yet 40, he was once, he says, "an old left-wing Greenpeace member" (p. xix). He is now what might be termed a recovered environmentalist, in the same way that Simon was a recovered family planner. His book has reached best-seller status. Described by *The Economist* as "one of the most valuable books on public policy—not merely on environmental policy—written for the intelligent general reader in the past ten years" and by the World Resources Institute (in a press advisory to journalists) as "pseudo-scholarship," it has, to put it mildly, caused a stir. Lomborg does not claim scientific expertise in the topics he reviews but brings to them statistical competence, skills in forensic analysis and synthesis, and no compunction in taking issue with prominent figures and conventional wisdom. He objects not to most of the quantitative information describing environmental trends collected by UN agencies, the World Bank, and other official bodies but to the interpretations made of them by those in the business, as he would see it, of generating alarm—presumably in order to stimulate remedial action. There is "an increasing fusion," he writes, "of truth and good intentions" (p. 32). He takes evident enjoyment in putting people right.

Specifically charged with organizational culpability are Greenpeace, the World-wide Fund for Nature, and, most of all, the Worldwatch Institute (often, not too unreasonably, identified with its former president, Lester Brown). The book's subtitle is a reference to the well-known and widely read annual Worldwatch publication *The State of the World*. The UN *Human Development Reports* are belabored on the issue of increasing global inequality (dangerous ground even for a skilled statistician); the recent UN Environment Programme report, *GEO 2000*, is castigated for fanciful estimates of soil erosion and child deaths from pollution. Prominent environmentalists like Paul Ehrlich, E. O. Wilson, and David Pimentel are singled out for criticism on particular issues. (Lomborg can of course be reproached for his own treatment of evidence: see, for instance, the criticisms assembled at www.anti-lomborg.com. A series of critical comments on the book will appear in the January 2002 issue of *Scientific American*, preceded by an unusual denouncement of it

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by the journal's editor in chief that is headed "Misleading math about the Earth" and "Science defends itself against *The Skeptical Environmentalist*." Lomborg's rebuttals are found on his own website, www.lomborg.org.)

The collection of alarmist views on the environment are merged into a single broad and soft target, which Lomborg calls the Litany—a word he is inordinately fond of. His mode of operation is to take a strong statement by a prominent person or organization about some existing or forecast environmental damage and demonstrate, generally using the same data, how ill-founded it was or how mistaken it has turned out to be. The choices are mostly obvious ones, like Paul Ehrlich's 1970s-vintage forecasts of a looming food crisis or Norman Myers's extravagant species extinction rates or Lester Brown on almost anything. In the debate over the interpretation of statistical data, Lomborg's voice is often compelling: well-reasoned and uncompromising. He sees his task and achievement to "free us of our unproductive worries" (p. 351). While telling us he "does not mean that we should make no effort towards improving the environment"—indeed, the reader is admonished "not to abandon action entirely" (p. 5)—the clear message is to relax: most of the environmental news is good.

The picture he draws is almost uniformly rosy. Food problems are in process of being solved and the proportion of the world's population "starving" has been halved since 1970, despite the 2 billion more mouths (p. 87). Both renewable and nonrenewable resources are abundant. "We could produce the entire energy consumption of the world with present-day solar cell technology placed on just 2.6 percent of the Sahara Desert" (p. 159). "Our forests are not under threat"—a third of the planet's land is forested, and even tropical forests are declining only at 0.5 percent per year (p. 117). There are no fresh water shortages, just water management problems—requiring, for example, "a movement away from self-reliance in food production in the arid parts of the world" and "adequate pricing" (p. 157). Extinction rates have been grossly overstated (Lomborg's figure for animals is 0.7 percent extinctions per 50 years—p. 255), and anyway refer mainly to beetles. (Simon also took particular exception to biodiversity alarmists.)

In several cases, he notes, major putative environmental problems have been disproven by scientific research, although the green literature is slow to acknowledge the fact. The damaging effects of acid rain on forests turned out to be largely illusory; synthetic estrogens in effluents are apparently not, as once was feared, a cause of breast cancer or low sperm counts; the case that global warming will increase the frequency of extreme weather events is weak. Other of Lomborg's arguments are based on the minor scale of the effect in comparison to background levels or the high cost of remedy in comparison to the returns from other uses of that money.

The lengthy account of global warming research is the centerpiece of the book. Lomborg is disdainful of most of the scenarios of the Intergovernmental Panel on Climate Change. He argues for adaptation to warming rather than prevention: stabilizing global CO₂ emissions is "a poor use of resources" (p. 316). The atmospheric concentrations will plateau anyway as we gradually shift to renewable energy sources, probably with less than a 2°C warming by 2100 (p. 286). Full compliance with the Kyoto Protocol, he reports, would gain us only a six-year delay in that

rise (p. 302). If we are worried enough about such prospects as an abrupt reversal of the Gulf Stream, with its dramatic chilling effect on Western Europe, to want to spend 2 percent of gross world product on greenhouse limitation measures (the reason, he suggests, that the European Union is so supportive of Kyoto), we should be even more prepared to spend equivalently on diversion of incoming meteors—considering their far more catastrophic potential (p. 316).

Population growth, at the center of environmentalist concerns a few decades ago, is dismissed by Lomborg (in this, he is far from alone) as no longer a problem. The rate of increase has fallen markedly: only a few billion more people will be added to the world this century. Moreover, rather than trampling the rural environment, the additional people will be in cities, where they will be healthier and wealthier than had they lived in villages. This pleasing prospect skirts over such matters as the large ecological footprint city-dwellers impose on the environment elsewhere, the dismal living conditions that poor and rapidly growing cities actually offer—their vaunted healthiness often an unkept promise—and the political challenges of absorbing increasing numbers of young adults into faltering economies. It is unhelpful to say, as he does, that these are problems of poverty.

The implicit hubris in Lomborg's subtitle equals (and mirrors) that of *Worldwatch*. Lomborg states, contra the alarmists but with equal lack of qualification, that "There is no ecological catastrophe looming around the corner to punish us" (p. 348). He is a true successor of Julian Simon not only in his serious attention to sources and statistics but also in his celebratory stance. For Simon, more people were a moral good in themselves as well as an instrumental good for what they could make of the world. In a rhetorical echo of Simon, Lomborg writes: "this is the really fantastic point about the real state of the world: that mankind's lot has vastly improved in every significant measurable field and that it is likely to continue to do so" (p. 351).

Such language is revealing. It seems to indicate that there is more going on here than debate over scientific facts or, narrower still, statistical inference. Indeed Lomborg in places acknowledges this, speaking of the protagonists as having wholly different agendas: "the global warming discussion sounds like the clash of two religions"—it is "not just a question of choosing the optimal economic path for humanity, but has much deeper, political roots as to what kind of future society we would like" (pp. 319, 321). Choosing that optimal economic path, he implies, is what it should be about, with all of nature in play. On the most basic issue, anthropocentrism, Lomborg stands with most social scientists: that it is simply the only sensible position. That given, the main tool he relies upon is benefit-cost analysis—pure reason in one sense but hedged about with contestable and contested assumptions. One such assumption, typically hidden, is the belief in the smoothness of environmental change. There are no nasty surprises in the wings: *natura non facit saltum*. Thus he is disdainful of the precautionary principle, that staple of environmental argument, seeing it as being used to unfairly advantage the case for costly environmental expenditures. The principle is "actually all about making *worse* decisions than we need to" (p. 350). Another assumption is that a substantial rate of time discounting is appropriate: he plumps for 4–6 percent per year (pp. 313–315), which implies weighing the interests of the next generation at

from one-third to one-fifth of those of the present generation. A third is the device of contingent valuation—ascertaining willingness to pay as a measure of environmental value. This standard, says the Global Commons Institute (1995), “embodies the ethics of the protection racketeer. It has no place in a civilised debate.” On all kinds of issues, human societies choose to remove certain areas of action from benefit–cost calculation. We tie ourselves to the mast: that, basically, is what constitutions and treaties do.

Thus Lomborg does considerable damage to the environmentalist cause but not through wholly legitimate means. The divergence in conclusions between him and his opponents is only in part a scientific disagreement. For example, environmental realities may not be adequately captured by the statistics he chooses to start with: no doubt that is a problem, but it is one shared with his opponents. Misunderstandings of the data—something that is alleged by Lomborg’s *Scientific American* critics—would be another matter for technical debate, but repairing any such problems, I suspect, would not go far to narrow the broader differences. The larger source of divergence is that the broad conclusions in dispute are not the immediate products of some objective statistical procedure but derive from marshaled argument. The debates over them take place in a realm above the data. The expert witnesses have had their say, leaving the case in the hands of the counsel for the prosecution and the defense. The protagonists take advantage of the flexibility of ordinary language, wielding the tools of rhetoric to cajole and persuade (see the discussion of the process in the sphere of economics by McCloskey 1998). In Lomborg’s implied depiction the debate is between rational beings like himself and woolly, parti-pris thinkers. A better characterization would be less unbalanced: Howard Margolis (1987) speaks of this kind of situation as one of “rival gestalts.” What is at issue is indeed contrasting views of what kind of society—and world—we would like.

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ANDREW MASON AND GEORGES TAPINOS (EDS.)

Sharing the Wealth: Demographic Change and Economic Transfers between Generations

Oxford: Oxford University Press, 2000. xvi + 408 p. \$85.00.

Intergenerational transfers make the economics of population aging much more challenging than the field would otherwise be. Without these transfers, we would have only a standard neoclassical story of wages being driven up and the rate of profit being driven down, both factors subject to some speculative arguments about technological progress. Add intergenerational transfers and not only does the distributional problem become much more complicated but the issues of political gridlock and social dysfunction become pertinent.

This volume of papers derives from a 1995 seminar organized by the International Union for the Scientific Study of Population (IUSSP) through its Economic Demography Committee and the East-West Center. It is divided into three parts, the first having to do with intergenerational accounting, the second with pension systems, and the third with intergenerational transfers within the family. Thus the volume succeeds in covering the entire variety of intergenerational transfers and a wide range of countries as well, including France, India, Korea, Pakistan, Peru, Taiwan, and the United States.

Of all the misconceptions that should be banished from discussions of aging, the most persistent and egregious is that in some simpler and more virtuous age, children supported their parents. Ronald Lee, in an international survey, dismisses the idea that wealth flows from children to parents in the poorest countries; in fact, it flows from parents to children even in very primitive settings, in part because resource consumption in old age is so meager. Even in wealthy countries the flow remains downward at the family level, but the emergence of public programs for the elderly switches the total wealth flow around. Using the example of Taiwan, Andrew Mason and Tim Miller demonstrate that even during periods of extremely rapid economic growth, intrahousehold transfers can prevent the emergence of dislocating intergenerational income differences. The most important mechanism, the authors find, is coresidence. The importance of coresidence re-emerges in the very different context of Peru, where Sumon Bhaumik and Jeffrey Nugent find that elderly persons with a coresident child are far more likely to receive significant transfers from their children. But coresidence is not invariably at the center of intergenerational transfers: in South Korea, Yean-Ju Lee finds that transfers from children to parents occur even when children live far away, and Joseph Altonji and colleagues find that, whereas time transfers in the United States decline sharply with distance from parents, money transfers do not.

In the section on government-funded pensions, Jorge Bravo applies actuarial approaches to estimate the rate of return to unfunded pensions as a function of the parameters of the system, such as the retirement age. I am always troubled by the concept of a "rate of return" to unfunded pensions, but this is a quibble. Salvador Valdés-Prieto describes the clientism that was at the heart of Chile's pre-Pinochet pension system and manages to make the international social security establishment's initial (and continuing) suspicion of that reform look rather mis-

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guided. But, lest Chile be thought the wave of the future, in the closing pages Valdés-Prieto anticipates the debacle now being observed in Argentina, where the government has essentially confiscated the assets of the fully funded pension to satisfy its fiscal needs. Rafael Rofman presents a careful accounting of the costs of transition in Argentina, but the recent crisis has swept all these calculations aside. Shripad Tuljapurkar and Ronald Lee's paper on uncertainty forecasts of the US Social Security system will be familiar material to many readers of this journal, as will Jagadeesh Gokhale's intergenerational accounting analysis of the United States.

Didier Blanchet and Jean-Alain Monfort's essay on the French pension system is notable, partly because of the ingenious graphical presentation and partly because, alone among the models employed, theirs endogenizes wages and the rate of return to capital. While not representing transfers per se, surely effects of population aging on the wage rate and the rate of return to capital will be among the most important in terms of ultimate impacts on the intergenerational distribution of income and wealth. The authors are diffident about this model, implying that it is not up to the standard set by current Overlapping Generations (OLG) models. I would have preferred to see them be more incisive, pointing out, for example, that the restrictive assumptions necessary to solve OLG models are patently absurd (model-consistent forward-looking expectations extending decades if not hundreds of years into the future) and that the life-cycle theory of consumption, which is at their heart, is in disarray. By contrast their own approach, they note, is "not necessarily far removed from real behaviour." Quite.

What I found missing in this book was more consideration of why, in the first place, we give our parents resources that we could consume ourselves or give to our own children. It is difficult to understand, from an evolutionary point of view, why we should not simply let our elders shift for themselves. Are we trying to buy bequests? None of the contributions in this book strongly supports the strategic altruism model, but none strongly supports the pure altruism model either. Are we providing a demonstration for our children of how they ought to act toward us when we are aged? More to the point, are we afraid to renege on the intergenerational contract, even if our parents cannot retaliate, for fear that our children will imitate our behavior? One gentle theory has it that elders are a repository of embodied wisdom, which is no doubt true, but they are a repository of entrenched senility too. All in all, the pervasiveness of upward intergenerational transfers in countries once they pass a relatively modest development threshold, and the sheer size of these transfers in the most developed countries, remains a puzzle.

In closing, all of the essays in this book are informative, they hang together well, and the editors' introduction provides a valuable synthesis; the main drawback of the book is the five-year delay between the conference and publication. The field of economic demography has suffered a tragic loss in the untimely death of Georges Tapinos.

NANCY FONER, RUBÉN G. RUMBAUT, AND STEVEN J. GOLD (EDS.)

Immigration Research for a New Century: Multidisciplinary Perspectives

New York: Russell Sage Foundation, 2000. xiv + 491 p. \$49.95.

Apart from its first 90-odd pages, this volume showcases the work of nearly 20 young scholars who were funded in 1996 and 1997 with predoctoral or postdoctoral research fellowships through the International Migration Program of the Social Science Research Council (SSRC). Earlier versions of many of the papers were presented at a June 1998 SSRC conference titled "Transformations: Immigration and immigration research in the United States." A few were previously published in a special issue of the *American Behavioral Scientist* (June–July 1999).

The collection opens with an overview and summary of the contents by the three editors. Here the authors state that the volume's purpose is "to provide a glimpse of these dual transformations" (p. 1)—that is, the ways in which immigration is shaping not only the demographic makeup of the US population but also the scientific study of immigration itself.

Part I, "Studying immigration: Disciplinary perspectives and future research needs," begins with an interesting essay in which Rubén Rumbaut surveys the characteristics of immigration researchers. Data come from the National Survey of Immigration Scholars (NASIS), collected in 1998 from 753 researchers who were sampled primarily from membership lists of professional associations that have subsections devoted to migration. Thirty percent of these individuals are foreign born, and another 18 percent have at least one foreign-born parent. Sociology (33 percent) and history (28 percent) dominate the fields in which they work. Major ethnicities and national origins include Asian (21 percent), Latin American/Caribbean (22 percent), and European (excluding Irish and Jewish) (17 percent). Trends gauged by the date of receipt of the Ph.D. indicate the rise of first-generation researchers and those of Asian and Latin origin, offset by declines in Jewish and other European (including "American" and white) participation. Rumbaut concludes that "there is value in making immigration research itself the object of systematic and reflexive scrutiny and analyzing it from the vantage of the sociology of knowledge.... Simply put, immigration is producing many of the scholars who study it and who will tell its story" (p. 40).

The second chapter reflects on disciplinary perspectives regarding immigration. It includes reflections by prominent figures in several fields, including Mary Waters (sociology), Nancy Foner (anthropology), George Sánchez (history), Aristide Zolberg (political science), and Josh DeWind (on the work of the SSRC). Taken together, these reflections illustrate the varied methods and paradigms used by each discipline, the extent to which immigration research has been or now is viewed as "mainstream," and the degree to which each discipline is self-conscious about its particular role in helping to understand immigration behaviors.

In the concluding chapter in Part I Herbert Gans identifies six research gaps in need of filling. These fall generally into the area of adaptation by immigrants and their descendants: (1) How are immigrants selected or self-selected for immigration to and emigration from the United States? (2) Why do we not know more about the immigrants themselves, especially those in the first and second genera-

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tions? (3) How do macro-level factors in the American economy, polity, and society affect not just the adaptation processes experienced by immigrants and their immediate descendants but also the opposition by native-born white Americans to immigrants and their children? (4) How relevant is Robert Merton's "insider-outsider" dichotomy to immigration research, and what issues does it raise about the backgrounds of researchers whose work gets funded and about the legitimacy of their findings? (5) Who and what determine the immigration research agenda, including which topics and groups are studied and what methodologies are most valued? (6) What role do funding agencies play, including the source of their funds, what topics are most likely to be funded (and not funded), and which researchers appear to be most favored (and out of favor)?

Part II, accounting for the remaining 400 pages, features the work of a new generation of immigration scholars. The 17 chapters are grouped under three broad headings: Political economy, membership, and the state; Migration, economic incorporation, and the market; and Ethnicity, race, gender, and community. The chapters rely heavily on qualitative data; most contain no tables or figures, and only a handful report regression results. Likewise, very few use nationally representative immigrant samples. Most authors focus on a particular immigrant group, destination point, or labor market niche. To cite a few examples, the researchers have studied Filipino nurses; Mexicans in California; Dominicans in New York City; Mexican migrants in US agriculture; skilled migrants in Silicon Valley; Jewish, Korean, and African-American entrepreneurs; 60 domestic workers in New York City and Philadelphia; 26 women from a sending community in western Mexico; a Sudanese refugee community in the Midwest; southern Italians in the United States; and Japanese Peruvians. In combination, these chapters offer a potpourri of recent scholarship; as a group, however, they do not convey an overarching theme, ask a specific research question, or contain a well-defined hypothesis.

The title of this volume leads readers to expect something the book does not deliver. "Immigration research for a new century" suggests that a broad-scale research agenda will be sketched, providing a map and a compass for researchers to come. The title would be more apt if "researchers" were substituted for "research." Further, with one or two exceptions, the chapters focus on the United States, despite the fact that immigration concerns around the world have burgeoned in the past five years. And while Herbert Gans points to several areas where additional research would be highly valuable, limiting his prescriptions to immigrant adaptation is surely too narrow. What about effects on the host society, adjustments the receiving societies make to the increased presence of immigrants, policy aspects including national security concerns, and demographic methods—to name a few other high-priority topics?

Moreover, economists will surely be disappointed by this book. Despite the volume's subtitle—"Multidisciplinary perspectives"—economists are left almost entirely out of the picture. Indeed, the disciplinary perspectives provided in Part I by senior scholars conspicuously omit economists. Of the 27 named contributors, just one (an agricultural economist at the US Department of Agriculture) represents the field. By contrast, the list includes 13 sociologists, three anthropologists, and five historians. Economists will also be chagrined to find that the index omits

mention of George Borjas, David Card, Richard Freeman, Mark Rosenzweig, James Smith, Finis Welch, and other distinguished economists doing vital immigration research. What makes these omissions all the more unfortunate is Mary Waters's valid observation that "Immigration is a hot political issue right now, and a great deal of public policy debate is currently being waged. Yet most of the research that is cited in these debates has been conducted by economists" (p. 46).

The editors argue in their Introduction that "These chapters, with their widely different disciplinary styles, theoretical concerns, and research methods, stand in marked contrast to what an earlier era of immigration studies produced" (p. 18). If one went back far enough, surely this would be true. But the volume does not seem to mark a sharp break with the recent past. Looking, for example, at *The Handbook of International Migration: The American Experience* (another Russell Sage publication from 1999, edited by Charles Hirschman, Philip Kasinitz, and Josh DeWind), one has the sense of evolution rather than discontinuity. The fact that the younger scholars represented here were trained by some of today's most respected immigration researchers also suggests continuity, rather than a sharp break with the past.

Finally, if we return to the issue of immigration research conducted by insiders versus outsiders, Rumbaut concludes his chapter with the comment, "In the access to the new and old immigrant populations that their unique position may afford them...this new generation of scholars in a transformed context of scholarship may bring *both unique advantages and disadvantages* to the social scientific study of immigration" (p. 40; my emphasis). Rumbaut is referring, among other things, to the degree of objectivity and detachment that "insider" researchers are able to bring to their work. This is one of the volume's most provocative comments, and I would have preferred a more thorough discussion of this issue.

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SHORT REVIEWS

by John Bongaarts, Julia Dayton, Susan Greenhalgh, Geoffrey McNicoll,
Michael P. Todaro

LOURDES BENERÍA WITH SAVITRI BISNATH (EDS.)

Gender and Development: Theoretical, Empirical and Practical Approaches

Cheltenham, UK and Northampton, MA: Edward Elgar, 2001. 2 volumes. xxi + 576 p. (Vol. I); xi + 606 pp. (Vol. II). \$400.00.

This massive, two-volume collection of key articles on the economics of gender and development fills a need for a major reference work in this growing field of research and practice. Part of the International Library of Critical Writings in Economics series, now edited by Mark Blaug, this collection is ably constructed and edited by Benería, professor of city and regional planning at Cornell University, and Bisnath, a doctoral candidate at the same university. The selection of items focuses on well-known pieces (this non-economist reviewer, for example, was familiar with roughly half the selections) and draws heavily on such mainstream sources as *World Development* (which supplied 17 of 54 items). The editors organized the selections into ten substantive topics. Volume I covers classic works in economic development and gender; methodological approaches; accounting for women's work; families and households; and project implementation and empowerment strategies. Volume II includes writings on women's access to resources, gender, and poverty; gender, employment, and labor markets; structural adjustment and economic restructuring; gender and markets; and institutional and social change. The broad scope of the selections testifies to the rapid development of feminist economics in recent years. Given that development, one is disappointed to find the editorial commentary on the selections kept to a bare minimum. A more substantive introduction by Benería, a well-known authority in the field, would have been welcome.

From the perspective of feminist research generally, these works in feminist development economics are notable for their near-exclusive focus on inequality along lines of gender, to the exclusion of disparities of race, class, and sexuality—social divisions that have been shown to interact so deeply with gender that gender is rarely studied in isolation from these other dimensions of social inequality. Also perplexing from the vantage point of feminist theory is the senior editor's wholesale dismissal of "postmodern" approaches as overly discursive. She argues that feminist economists' emphasis on development has required them to focus on material issues, as well as on the nuts and bolts questions that must be answered in order to solve problems in women's lives. Yet instead of viewing the discursive and the material as distinct and opposed, feminist development eco-

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nomics might well have seen the two as intertwined, with the discourses on gender profoundly shaping the practices of gender. Yet even without a discursive dimension, the collection will surely serve its intended function as an essential reference work for students and practitioners of gender relations in developing economies. Despite the number of pages devoted to the subject—almost 1,200 in these volumes alone—the reader comes away agreeing with the editor that students of development have much work to do to establish the notion of gender as a relationship between men and women, rather than something having to do with women alone. One hopes that, by helping readers identify the gaps in this growing field, these volumes will inspire others to continue the work of understanding and alleviating the many inequalities produced by development in today's globalizing world. Chapter-end bibliographies, name index.—S.G.

MARGARET JOLLY AND KALPANA RAM (EDS.)

Borders of Being: Citizenship, Fertility, and Sexuality in Asia and the Pacific

Ann Arbor: University of Michigan Press, 2001. xii + 326 pp. \$24.95 (pbk.).

The editors of this provocatively titled, cutting-edge volume draw attention to some fascinating links between the borders of persons and the borders of countries, the creation of human life and the construction of nations. Smudging the boundaries between the humanities and the social sciences, the authors draw perspectives and methods from anthropology, history, cultural studies, and feminist theory to ask new questions about citizenship, sexuality, and fertility in Asia and the Pacific. In her helpful introduction, Jolly, professor at Australian National University's Gender Relations Centre, limns the myriad connections the volume makes between two, now separate bodies of literature: that on the gendering of nationalism and citizenship, and writings on population and fertility control. The contributors, who are mostly based in Australia, explore a rich variety of themes—familial metaphors of the nation, reproductive "choice," women as citizen-mothers, the place of men in the national and global politics of reproduction, and many more—in the Asian states of China, India, Indonesia, the Philippines, and Thailand and the Pacific states of Fiji, Papua New Guinea, the Solomon Islands, and Vanuatu. Although each chapter tells a fascinating story, the signal contribution of this volume lies in the domain of theory. The authors not only bring work on fertility and fertility control into conversation with writings on nationalism, they also use the Asian and Pacific cases to interrogate dominant, Eurocentric theories of citizenship. By posing questions and finding some answers in two large corners of the world, the authors have greatly advanced the cultural and political understanding of critical, yet poorly understood issues. Strongly recommended to anthropological, feminist, and other curious students of reproduction. Chapter-end bibliographies, index.—S.G.

JOHN LOXLEY

Interdependence, Disequilibrium and Growth: Reflections on the Political Economy of North–South Relations at the Turn of the Century

New York: St. Martin's Press, 1998. xii + 185 p. \$65.00.

While much has been written about how economic globalization is affecting all regions of the world, the actual extent of globalization has been greatly exaggerated. Some countries in Southeast Asia, for instance, have been active participants, if not wholly beneficiaries. They have engaged in concentrated export expansion, witnessed rapid inflows and outflows of foreign investment, and benefited from large-scale technological transfer. Others, particularly in sub-Saharan Africa, have been largely bypassed by globalization. Their commodity exports have deteriorated in value, they have received declining shares of private foreign investments and foreign aid, and their debt problems continue to burden their domestic economies. In this fascinating and unconventional analysis of the evolution of North–South relations over the past two decades, John Loxley examines how globalization has differentially affected various regions and countries of the developing world. He argues correctly that distinguishing between different groups of countries is vital for a more accurate understanding of how developed-country policies and the international institutions they control (principally the World Bank and International Monetary Fund) influence developing-country populations in areas of trade, debt, and aid.

The principal focus of this book, which is part of a larger project of six studies on North–South relations sponsored by Canada's International Development Research Centre, is on four contemporary developments with potentially serious implications for rich–poor country interactions: how the United States deals with its balance-of-payments deficits; the emergence of regional trading blocs in North America, Europe, and the Pacific Rim; implementation of the Uruguay Round of GATT; and the collapse of the Soviet Union and its satellite regimes. On a more philosophical level the author raises serious questions about the desirability and feasibility of basing economic policy in the South on the growth imperative that has permeated economic policy in the North. He argues that "the generalization of the northern hegemonic consumption model to the rest of the world is not compatible with ecological balance and the law of entropy." In this context, Loxley advances several unconventional alternative approaches to development that could inevitably influence economic relations between rich and poor countries in terms of trade, financial flows, and international migration. According to the author, "These approaches assume radical changes in the locus of political power, in the distribution of wealth and in the morality underlying economic and social behaviour. They challenge the need for and desirability of economic growth, our patriarchal, competitive and inequitable approach to economic organization, and the wisdom of prevailing views on a whole range of issues from development, poverty, the nature of the informal sector, the use of land and resources and free trade."

Clearly this book does not represent mainstream economic thinking. It harkens back to some of the dependency literature of the 1960s and 1970s while maintaining a less confrontational approach to North–South relations. Yet, this kind of bold and unconventional approach is missing from much of the contemporary de-

bate on development. To that end, Loxley's contribution is certainly to be welcomed. Population issues are limited to discussions of the stationary-state economic models of John Stuart Mill and, more recently, Herman Daly. Bibliography, index.—M.P.T.

WILLIAM PETERSEN

From Birth to Death: A Consumer's Guide to Population Studies

New Brunswick, NJ: Transaction Publishers, 2000. 197 p. \$39.95.

Although William Petersen does not explain who the intended consumer is, this short and often entertaining account of the field might well be recommended as introductory reading for a student embarking on the study of demography. Equally, it would be useful for someone with no such ambition but curious to know in nontechnical terms what demographers do. There is a lot about data and measurement, brief discussions of the staples of fertility and mortality, an aside on paleodemography and primitive populations, and chapters on theories, forecasts, and policies. The text is enlivened by Petersen's reflections and anecdotes from a long career, together with sometimes caustic remarks on items or personalities in the demographic canon. He notes how national schools of demography have emerged characterized by the dominant social issue each was concerned with, impeding communication between them. Thus American researchers on ethnicity ignored the large European literature on the subject: "in the American context the problem was long seen as identifying and mitigating impediments to full assimilation, while in Central Europe it was, on the contrary, how minorities could maintain their own languages, religions, and ways of life." Quoting Notestein and Davis approving demography's number-crunching bent, he remarks: "It is strange to find two distinguished members of a discipline commenting with some satisfaction on the paucity of theory, which any philosopher of science would describe as a serious lack." Index.—G.McN.

ZEBA AYESHA SATHAR AND JAMES F. PHILLIPS (EDS.)

Fertility Transition in South Asia

Oxford: Oxford University Press, 2001. xiv + 434 p. \$74.00.

South Asia here refers to the subcontinent, comprising a population of around 1.3 billion. In this conference-based volume in the IUSSP's International Studies in Demography series there are 19 chapters, grouped into three sections: fertility levels and trends; explanations; and the role of policy. Although hampered by serious problems of data inaccuracy, the contributors generally agree that fertility transition is underway even in the lagging countries of the region—Pakistan and Nepal—and has been completed in Sri Lanka. A variety of explanatory variables receive attention: the effectiveness of public administration in establishing human security; the level of women's autonomy—with micro-credit schemes as a contributing factor; son preference, seen as inhibiting fertility transition in major parts

of the region; and family planning programs. Two chapters present contrasting accounts of the sources of Bangladesh's fertility decline—one as program-induced, the other as an outcome of pervasive socioeconomic change that has eroded traditional beliefs and patterns of life. The editors are with the Population Council in Islamabad and New York. Index.

M. A. B. SIDDIQUE (ED.)

International Migration into the 21st Century: Essays in Honour of Reginald Appleyard

Cheltenham, UK; Northampton, MA: Edward Elgar, 2001. xxi + 321 p. \$90.00.

Reginald Appleyard, emeritus professor at the University of Western Australia, has been a major contributor to the literature on international migration. This collection of papers was prepared for a conference in his honor at his university in late 1999. Although the conference focused on a number of major issues related to international migration, participants were permitted broad leeway in preparing their contributions. As such, the book is a diffuse collection of essays with authors focusing on topics in their fields of expertise.

After an introductory paper by Siddique and Appleyard, Tomas Hammar explores the politics of immigration control and the ways in which the issue has been politicized in Europe. Philip Martin examines economic integration between Mexico and the United States in the context of demand pull, supply push, and network linkage that have resulted in the migration of more than 8 million Mexican-born residents to the United States over the past century. Martin reviews the history of this movement, examines how the standard international trade model relates to migration, and assesses the impact of NAFTA and the effectiveness or lack thereof of various US policies designed to regulate the flow. Aderanti Adepoju addresses intraregional migration in sub-Saharan Africa within the context of regional economic groupings. He argues for regional integration as a necessary condition for African economic development but points out that most attempts at such integration have largely failed, primarily owing to weak political support. Among the other ten chapters, five were particularly instructive to this reviewer. Barry Chiswick examines why illegal migrants tend to be low-skilled workers and assesses their impact on the distribution of income in the host economy and the use of income transfers. He discusses the political economy of illegal migration and explains why enforcement usually fails. Although this chapter draws heavily on much of what Chiswick has published before, his analysis is concise and convincing. Ronald Skeldon notes how the term diaspora has been appropriated by the migration literature and examines its relevance to the case of the Chinese, who have created a global network of communities and are now viewed as a modern illustration of diaspora—a scattering of peoples. Also noteworthy are papers by Hania Zlotnik on past trends and future prospects for international migration, by Charles Keely on international personnel movements and the emergence of an international migration regime, and by Sally Findley on the rise of forced migration in sub-Saharan Africa. This volume of papers is among the best to appear in the prolific literature on international migration published during the past two

decades. It should be of value both to the migration specialist and to readers interested in this increasingly critical area of population research. Index.—M.P.T.

EWA TABEAU, ANNEKE VAN DEN BERG JETHS, AND CHRISTOPHER HEATHCOTE (EDS.)

Forecasting Mortality in Developed Countries: Insights from a Statistical, Demographic and Epidemiological Perspective

Dordrecht, Netherlands: Kluwer Academic Publishers, 2001. xxvi + 303 p. \$113.00.

The rapid aging of populations in the more developed countries has led to growing demands on social security and public support programs for the elderly. These trends, in turn, have increased the need for more accurate projections of mortality and of the population of elderly adults. This task has become more urgent because past projections often overestimated mortality, hence underestimated the size and rate of growth of the elderly, a bias that probably still exists in some recent projections. This volume reviews the theory and practice of forecasting mortality in Western European countries, especially in the Netherlands. In these countries mortality is low, extensive historical data are available, and the economic, environmental, epidemiological, and social determinants of mortality are relatively well understood. The first step in a mortality projection is to find a concise statistical description of past trends. This historical information is then used to make a projection that, for the short run, can rely on extrapolation. But for projections exceeding two or three decades simple extrapolation often leads to implausible results and expert judgment then must be consulted. For example, experts may identify a target for life expectancy at birth in a future year, or they may predict the relative improvements in age- and sex-specific mortality. While in the past mortality projections were usually based strictly on mortality trends, they are now increasingly informed by an understanding of relevant behavior (e.g., smoking, diet, exercise) and by information on levels and trends in causes of deaths.

Aimed at a technical audience of demographers, statisticians, and epidemiologists, the contributions in this volume provide a concise state-of-the-art overview of current projection practices. Several promising lines of research are identified that are likely to lead to improved mortality projections. Further research and better data are clearly needed because there is still considerable disagreement about future mortality trends among experts and dissonance among projection methodologies.—J.B.

ARNE TOSTENSEN, INGE TVEDTEN, AND MARIKEN VAA (EDS.)

Associational Life in African Cities: Popular Responses to the Urban Crisis

Uppsala: Nordiska Afrikainstitutet, 2001. 324 p. \$29.95 (pbk.).

This book examines the role of civil society in addressing the demands of urban life in Africa. Since the 1960s, African cities have experienced the most rapid sustained population growth rates in world history, at over 5 percent a year on average. The number of cities with more than one million residents now exceeds 30,

and the proportion of national populations living in the largest cities has increased to over 20 percent. In the next quarter-century, 90 percent of world population growth is expected to occur in urban areas of developing countries. Accompanying Africa's population growth are a shortage of housing and jobs, widespread poverty, severe environmental problems, failing services, and inadequate local government structures.

As a conference compendium, the book includes papers presented at a 1998 conference in Bergen, Norway. An introductory chapter documents the urban environment in Africa, clarifies terms used in the discourse (such as associational life, civil society, and governance), and summarizes the changing role of urban associations. Case studies review specific civic initiatives in various African cities, with a focus on how each started and its achievements and challenges. Contributors suggest two main motivations that bring forth civic organizations: to provide informal insurance and to respond to government shortcomings in the provision of local services and governance. The factors that bring groups together for mutual insurance appear primarily to be religion and place of origin or birth. With respect to the breakdown in governance, sections of the book are devoted to civic efforts in the areas of land tenure, housing, infrastructure, and public services. Conspicuously absent are studies of civic efforts in improving health care or disease prevention. Although alluded to in some chapters, scant discussion is devoted to public-private collaboration in solving urban problems. Also missing is a clear sense of what factors, if any, distinguish civic groups among the poor and marginalized as compared with more well-to-do populations.—J.D.

UNITED NATIONS POPULATION FUND

The State of World Population 2001. Footprints and Milestones: Population and Environmental Change

New York: UNFPA, 2001. 76 p. \$12.50.

The annual population status report of the UNFPA has evolved from simple origins to become a well-written and properly sourced thematic overview of population issues. For 2001 the subject is population and environmental change. The discussion emphasizes problems over achievements and the ill effects of forecast environmental change over any offsetting favorable effects. The supporting references, however, are thorough and well chosen. A section on externalities examines "environmental paybacks" from population-related investments yielding lower fertility. Paybacks include a smaller increase in carbon emissions and the environmental improvements affordable as a result of the more rapid economic growth induced by the so-called demographic bonus of low child dependency. The report concludes with recommendations for action, calling for further progress in implementing the Cairo agenda (these are the milestones of the subtitle) and for a range of environmental measures that lie largely outside UNFPA's bailiwick. A statistical appendix reproduces selected demographic, social, and economic indicators for developing countries, mainly from the UN Population Division, UNESCO, WHO, and the World Bank.—G.McN.

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The US National Intelligence Council on Growing Global Migration

The National Intelligence Council, a body reporting to the Director of Central Intelligence, brings together expertise from inside and outside the US government to engage in strategic thinking on national security issues. Some of its reports, known as National Intelligence Estimates, are now issued in unclassified versions. One of these, published in December 2000, was entitled Global Trends 2015: A Dialogue about the Future with Nongovernment Experts. It discussed what it termed the key drivers of global change and presented a generally bleak set of scenarios of the medium-term future. (See the short review in PDR 27, no. 2, pp. 385–386.) Demographic factors—in particular, mass migration—were seen as one of the drivers. This topic is investigated further in a subsequent NIC report, Growing Global Migration and Its Implications for the United States, issued this year. The initial section of the report, headed Key Judgments, is reprinted below.

The report emphasizes the economic advantages of liberal immigration policies to the advanced economies, “despite some initially higher welfare costs and some downward pressure on wages.” Resistance to liberalization in European countries and Japan is seen as putting them at a competitive disadvantage to the United States. Their levels of illegal immigration, however, will inevitably increase in scale. Expectations for the US are for rises in both legal and illegal immigration. Mentioned as one of the “difficult issues” that are minor offsets to the broad gains offered by immigration is its use as a vehicle for “transnational terrorist, narcotrafficking, and organized crime groups.” The full report is available online at <http://www.cia.gov/nic/pubs/index.htm>.

During the next 15 years, globalization, demographic imbalances between OECD and developing countries, and interstate and civil conflicts will fuel increasing international migration, much of it illegal. Migration will have positive and negative consequences for sending and receiving countries alike. Other countries’ responses to migration issues will affect migration pressures on the United States and a broad range of US economic and security interests.

Growing migration

Today, more than 140 million people live outside their countries of birth and migrants comprise more than 15 percent of the population in over 50 countries. These numbers will grow as demographic “push” and “pull” factors intensify:

—Some 45 million people in developing countries will enter the job market each year through 2015; many will fail to find work and some will emigrate, whether legally or illegally.

—Illegal migration—facilitated increasingly by alien-smuggling syndicates and corrupt government officials—will grow dramatically, matching or exceeding other forms of migration into many countries in Europe and in the more developed countries of Asia, Africa, and Latin America.

—Violent conflicts, economic crises, and natural disasters in developing countries will often trigger mass migrations.

—By contrast, Europe and Japan face rapidly aging populations and shrinking labor forces, threatening the solvency of pension systems and constraining economic growth in the absence of greater migration or other compensatory measures such as pension reform and increases in productivity.

—A wide range of constraints—many of them resulting from recent globalization and democratization trends—will limit most countries' willingness and ability to control migration flows across their borders.

Migration's mixed impact

For sending countries, emigration will relieve pressures from their unemployed youth, generate substantial remittances, and often provide them with leverage on receiving countries. Returning immigrants often will be agents of economic modernization and political liberalization. But emigration also will result in the loss of skilled personnel—especially in Sub-Saharan Africa, South and East Asia, and Russia—while ethnic diasporas will sometimes be agents of extremism or separatism, as in the Balkans.

For most receiving countries, immigration will provide demographic and economic vitality for those with aging populations—even as it raises complex political and social integration challenges.

—Migration will ameliorate labor force and military manpower shortfalls and expand tax and consumer bases in developed countries.

—Immigrants' initial strain on social, educational, and health services and their differing languages, cultures, and religious practices will evoke discrimination and hamper their further assimilation.

—The negative impact of large illegal or mass migration will be greatest in less developed receiving countries, straining local infrastructures, contributing to the spread of infec-

tious diseases, and sometimes upsetting ethnic balances and contributing to conflict or violent regime change.

Key regional trends

Americas. Legal and illegal migration to the United States and within the region will continue to rise. Despite declining population growth and strong economic prospects in Mexico, persistent poverty and large wage differentials will further fuel large-scale emigration to the United States. Central America will remain the second-largest source of illegal migrants and its large alien-smuggling infrastructure also will make it a gateway for other US-bound immigrants, especially from South America and Asia. Political instability or economic decay in Cuba or Haiti could again lead to mass migration to the United States. Growing illegal migration within Latin America also will become a more contentious issue among governments.

In *Russia and other Eurasian states*, weak immigration control regimes, fitful economic development, ethnic conflicts, and discrimination against minorities will sustain migration pressures that already have produced more than 10 million—mostly Russian—migrants since the Soviet Union's breakup. Migration into Russia will partially compensate for but not offset labor force shortfalls and declining population. It also will add to welfare costs and may generate friction with other FSU states and with China over illegal immigration into the Russian Far East.

The *Middle East and Sub-Saharan Africa* are the principal sources and places of refuge to some 7 million refugees—including Palestinians, Afghans, and Rwandan Hutus—who will remain a source of instability and political polarization throughout these regions. The status of Palestinian refugees will continue to confound efforts toward a peace settlement. Intra-regional migration will play a key role in the economies of more developed states, but these regions also will be a major source of migration to developed countries.

In the *European Union*, countries will attempt to reconcile protection of national borders and cultural identity with the need to relieve growing demographic and labor market imbalances. Most EU countries are unlikely to

opt for large numbers of new immigrants, while legal constraints against discrimination and laws favoring family reunification also preclude a “fortress” approach. Instead, most are likely to opt for “targeted migration” in an effort to meet labor shortages in selected sectors while not unduly burdening national health and welfare systems or provoking a political backlash.

In *Asia*, populous countries such as China and India will be the source of growing regional and global migration flows. The advanced countries in the region—with the exception of Australia and New Zealand—will strongly resist integrating migrants socially and politically. Japan, which faces the greatest demographic imbalances, nonetheless will attempt to retain its current, highly cautious approach to immigration. Japan’s premium on ethnic homogeneity, few legal constraints against discrimination, high population density, and geographic insularity will reinforce this approach absent a sustained economic recovery. Should a recovery take hold, however, labor shortfalls may become so acute that Japan may shift eventually to a more open, targeted migration approach.

Hesitant Japanese and EU Responses Portend Problems. Because temporary workers are likely to find ways to remain indefinitely, shifting to the “targeted migration” course may not significantly curtail social and political tensions. Nor would it substantially expand European and Japanese labor forces or tax bases or provide relief for shrinking military recruitment pools, since temporary workers are excluded from military service and many illegal workers evade taxes.

Implications of Global Migration Pressures for the United States. Foreign born residents now comprise nearly 11 percent of the US population, up from 6 percent in 1980, and immigration will continue to climb during the next 15 years. It will be driven by the attraction of the strong US economy and political and economic difficulties in many developing and former

Communist countries. Most experts believe that migration will continue to contribute significantly to noninflationary economic growth and demographic balance, despite some initially higher welfare costs and some downward pressure on wages in relevant sectors. Difficult issues will nonetheless arise:

—Manmade humanitarian emergencies and natural disasters, especially in Latin America, will challenge US immigration control efforts.

—The United States will remain vulnerable to attempts by some foreign governments to use the threat of mass migration as leverage in bilateral relations or to relieve domestic pressures.

—Attempts by other countries to prevent or sharply limit immigration will tend to channel migration pressures toward the United States.

—Transnational terrorist, narcotrafficking, and organized crime groups will seek to blend into and recruit among coethnic immigrant communities and exploit gaps in migration control efforts to ply their trades.

On a broader level, the reluctance of key US economic partners and allies to substantially liberalize migration policies will place them at a competitive disadvantage with the United States in strategically important sectors such as information technology. According to the OECD, differing demographic trends are a major factor in Europe’s and Japan’s weaker economic outlook in comparison with the United States during the next five years.

—Restrictive migration policies, by limiting economic growth in Europe and Japan, may undermine efforts to overcome the imbalances among the advanced economies.

—Such policies also may skew the “guns versus butter” debate in these countries toward maintaining social expenditures at the expense of defense spending, weakening burden-sharing and the alliance system.

Testing Evolutionary Hypotheses with Demographic Data

ALICE L. CLARKE
BOBBI S. LOW

An ecological evolutionary viewpoint offers new perspectives on contemporary demographic problems in general and on population–environment issues in particular. In turn, rich and detailed human demographic data can help solve problems of interest in evolutionary theory. Such data have been analyzed in greatest detail in studies of traditional and historical societies. Evolutionary approaches using historical data go beyond small-sample anthropological studies to the application of the evolutionary approach to large datasets, and illuminate important similarities between small-scale traditional societies and large modern populations living in evolutionarily novel environments. This article provides a concise update of the breadth of questions and hypotheses of likely interest to demographers and others that evolutionary theorists address using a variety of traditional and historical datasets. It suggests opportunities for additional collaborative work between evolutionary theorists and historical demographers and highlights topics relevant to modern demography.

Death at the Border: Efficacy and Unintended Consequences of US Immigration Control Policy

WAYNE A. CORNELIUS

This article assesses the efficacy of the strategy of immigration control implemented by the US government since 1993 in reducing illegal entry attempts, and documents some of the unintended consequences of this strategy, especially a sharp increase in mortality among unauthorized migrants along certain segments of the Mexico–US border. The available data suggest that the current strategy of border enforcement has resulted in rechanneling flows of unauthorized migrants

to more hazardous areas, raising fees charged by people-smugglers, and discouraging unauthorized migrants already in the US from returning to their places of origin. However, there is no evidence that the strategy is deterring or preventing significant numbers of new illegal entries, particularly given the absence of a serious effort to curtail employment of unauthorized migrants through worksite enforcement. An expanded temporary worker program, selective legalization of unauthorized Mexican workers residing in the United States, and other proposals under consideration by the US and Mexican governments are unlikely to reduce migrant deaths resulting from the current strategy of border enforcement.

Women's Autonomy in India and Pakistan: The Influence of Religion and Region

SHIREEN J. JEJEEBHOY
ZEB A. SATHAR

This article compares the lives of women and explores dimensions of their autonomy in different regions of South Asia—Punjab in Pakistan, and Uttar Pradesh in north India and Tamil Nadu in south India. It explores the contextual factors underlying observed differences and assesses the extent to which these differences could be attributed to religion, nationality, or north–south cultural distinctions. Findings suggest that while women's autonomy—in terms of decision-making, mobility, freedom from threatening relations with husband, and access to and control over economic resources—is constrained in all three settings, women in Tamil Nadu fare considerably better than other women, irrespective of religion. Findings lend little support to the suggestion that women in Pakistan have less autonomy or control over their lives than do Indian women. Nor do Muslim women—be they Indian or Pakistani—exercise less autonomy in their own lives than do Hindu women in the subcontinent. Rather, findings suggest

that in the patriarchal and gender-stratified structures governing the northern portion of the subcontinent, women's control over their lives is more constrained than in the southern region.

Spatial Patterns of Fertility Transition in Indian Districts

CHRISTOPHE Z. GUILMOTO
S. IRUDAYA RAJAN

The article explores the dynamics of Indian fertility at the district level using a child-woman index developed from the four Indian censuses, 1961 to 1991. It employs statistical and geostatistical techniques to assess fertility change across districts and periods. Fertility decline is evident in every region, but sizable regional differentials exist. A cluster analysis of fertility profiles indicates that a clear spatial pattern of fertility in India has emerged and the pattern intensified because of the process of fertility decline.

First Impressions from the 2000 Census of China

WILLIAM LAVELY

The 2000 census of China has several notable innovations, including a sample long form containing detailed items on migration, housing, and employment. Preliminary data indicate rapid urbanization and continued

rapid social change in the 1990s, and apparent success in the government's drive to curtail population growth. Although a post-enumeration survey indicates that overall data quality is good, the rise of a mobile "floating population" and pressures of the birth planning program caused problems for the enumeration of migrants and infants. Data released to date have been silent on two important issues, fertility and rising sex ratios.

The Impact of HIV/AIDS on Adult Mortality in Zimbabwe

GRIFFITH FEENEY

In June 2000, an estimated 25 percent of adults in Zimbabwe were living with HIV/AIDS. Statistical data on the impact of the epidemic, though problematic in many ways, are better for Zimbabwe than for many other countries in sub-Saharan Africa. This analysis presents estimates of adult mortality in Zimbabwe based on multiple sources, including registered deaths adjusted for incomplete reporting, estimated at approximately 50 percent. Comparison of estimates from different data sources shows that they are subject to substantial errors. At the same time, the estimates leave no doubt that adult mortality risks in Zimbabwe more than doubled between 1982 and 1997. The evidence that this rise is due to AIDS deaths is circumstantial, but very strong; there is no credible competing explanation.

Vérifier les hypothèses évolutionnistes à l'aide de données démographiques

ALICE L. CLARKE
BOBBI S. LOW

Un point de vue d'évolution écologique ouvre de nouvelles perspectives sur les problèmes démographiques contemporains en général et sur les questions d'ordre démographique et environnemental en particulier.

Or, des données démographiques détaillées peuvent contribuer à régler des problèmes intéressants dans le domaine de la théorie évolutionniste. Ces données ont été analysées de façon approfondie à la lueur d'études des sociétés traditionnelles et historiques. Les méthodes évolutionnistes se fondant sur des données historiques vont au-delà des études anthropologiques utilisant l'échantillonnage de faible effectif; elles utilisent de grands ensembles de données et soulignent les similarités importantes entre les sociétés

traditionnelles de petites dimensions et les grandes populations modernes vivant dans les nouveaux environnements évolutionnistes. Le présent article fournit une mise à jour concise de l'étendue des questions et des hypothèses susceptibles d'intéresser les démographes, ainsi que d'autres points abordés par les théoriciens évolutionnistes, à l'aide d'ensembles de données historiques et traditionnelles variées. Il propose d'autres possibilités de collaboration entre théoriciens évolutionnistes et démographes historiques et fait ressortir les thèmes se rapportant à la démographie moderne.

Mourir à la frontière : Efficacité et conséquences non intentionnelles de la politique américaine sur le contrôle de l'immigration

WAYNE A. CORNELIUS

Le présent article évalue l'efficacité de la stratégie du contrôle de l'immigration aux États-Unis, en vigueur depuis 1993, et qui vise à réduire les tentatives d'entrée clandestine. Il documente également certaines conséquences non intentionnelles de cette stratégie, notamment l'augmentation marquée de la mortalité chez les migrants clandestins le long de certaines sections de la frontière entre le Mexique et les États-Unis. Les données disponibles suggèrent que les mesures coercitives à la frontière ont eu comme résultat de réacheminer le flux migratoire clandestin vers des endroits plus dangereux, de faire monter les tarifs chargés par les passeurs de clandestins et de dissuader les migrants vivant illégalement aux États-Unis de retourner à leur lieu d'origine. Toutefois, il n'y a aucune évidence que la stratégie décourage ou empêche un nombre appréciable de nouvelles entrées clandestines, surtout en l'absence d'un effort sérieux pour freiner l'embauche de migrants clandestins en faisant respecter la loi dans les lieux de travail. Il est peu probable qu'un programme élargi de travail temporaire, la légalisation sélective de travailleurs mexicains clandestins résidant aux États-Unis, et d'autres propositions à l'étude par les gouvernements américain et mexicain réduisent le nombre de décès de migrants clandestins attribuables aux mesures coercitives à la frontière.

L'autonomie des femmes en Inde et au Pakistan : L'influence de la religion et de la région

SHIREEN J. JEJEEBHOY
ZIBA A. SATHAR

Le présent article compare la vie des femmes et examine leur degré d'autonomie dans deux régions de l'Asie méridionale : le Punjab au Pakistan, et l'Uttar Pradesh en Inde septentrionale ainsi que le Tamil Nadu en Inde méridionale. Il examine les facteurs contextuels qui sous-tendent les différences observées et le degré d'influence qu'exercent la religion, la nationalité ou les distinctions culturelles nord-sud sur ces différences. Les résultats suggèrent que même si l'autonomie de la femme (évaluée en fonction de la capacité de prise de décision, la mobilité, la possibilité de résister aux menaces du mari, l'accès aux ressources financières ainsi que leur contrôle) est assujettie à certaines restrictions dans les trois domaines, les femmes de Tamil Nadu sont de beaucoup mieux nanties que les femmes des autres régions, abstraction faite de la religion. Ces résultats apportent peu de crédibilité à la suggestion que les femmes au Pakistan jouissent de moins d'autonomie ou de contrôle sur leur vie que les femmes en Inde, ou que les femmes musulmanes (Indiennes ou Pakistanaïses) jouissent de moins d'autonomie dans leur vie que les femmes hindoues du sous-continent. Les résultats suggèrent plutôt que dans les structures patriarcales et stratifiées selon le sexe, que l'on retrouve dans la portion septentrionale du sous-continent, le contrôle qu'exercent les femmes sur leur propre vie est plus limité que dans la région méridionale.

Structuration spatiale de la transition de la fécondité dans les districts indiens

CHRISTOPHE Z. GUILMOTO
S. IRUDAYA RAJAN

Cet article explore les dynamiques de la fécondité à l'échelle du district en utilisant un indice enfants-femmes calculé à partir des quatre recensements indiens de 1961 à 1991. Il utilise des méthodes statistiques et géostatistiques pour évaluer les changements

de fécondité à travers les districts et les périodes. La baisse se fait sentir partout en Inde, mais d'importants différentiels régionaux existent. Une analyse par cluster des profils de fécondité indique qu'une forte structuration spatiale de la fécondité a émergé en Inde et s'est intensifiée du fait des processus de baisse de fécondité.

Premières impressions sur le recensement de 2000 en Chine

WILLIAM LAVELY

Le recensement de 2000 en Chine a été marqué par plusieurs innovations remarquables, y compris un spécimen de formulaire détaillé sur la migration, l'habitat et l'emploi. Des données préliminaires indiquent une urbanisation et un changement social rapides dans les années 1990, et un succès manifeste de la motivation du gouvernement à freiner la croissance démographique. Bien qu'une enquête postcensitaire indique que la qualité de l'ensemble des données est fiable, l'émergence d'une « population flottante » mobile et les pressions exercées par le programme de planification familiale ont entraîné des problèmes dans le dénombrement des migrants et des enfants en bas âge. Les données publiées à ce jour n'ont rien révélé sur deux questions importantes : la fécondité et le rapport mâles-femelles à la hausse.

L'incidence du VIH/SIDA sur la mortalité adulte au Zimbabwe

GRIFFITH FEENEY

En juin 2000, il a été estimé qu'au Zimbabwe, 25 pour cent des adultes étaient atteints du VIH/SIDA. Des données statistiques sur l'incidence de l'épidémie, bien que défectueuses à bien des égards, sont meilleures pour le Zimbabwe que pour bien d'autres pays en Afrique subsaharienne. La présente analyse fournit des prévisions sur la mortalité adulte au Zimbabwe, prévisions fondées sur des sources multiples, y compris les décès enregistrés et ajustés pour cause de rapport incomplet, estimé à environ 50 pour cent. La comparaison des prévisions provenant de différentes sources de données révèle qu'elles sont susceptibles de contenir des erreurs importantes. Néanmoins, les prévisions ne laissent planer aucun doute, à savoir que les risques de mortalité adulte au Zimbabwe ont plus que doublé entre 1982 et 1997. L'évidence que cette hausse soit attribuable aux décès causés par le SIDA est circonstancielle, mais tout de même très probable : il n'existe pas d'autre explication concurrente crédible.

Somatiendo a prueba hipótesis evolucionarias con datos demográficos

ALICE L. CLARKE
BOBBI S. LOW

Un punto de vista evolucionario ecológico ofrece nuevas perspectivas sobre los problemas demográficos contemporáneos en general y sobre aspectos de población-medio ambiente en particular. A su vez, abundantes y detallados datos demográficos humanos pue-

den ayudar a resolver problemas que son de interés para la teoría evolucionaria. En los estudios de sociedades tradicionales e históricas tales datos han sido analizados muy detalladamente. Los enfoques evolucionarios que usan datos históricos van más allá de estudios antropológicos de muestra pequeña, a la aplicación de un enfoque evolucionario a compilaciones de gran tamaño de datos, e ilustran parecidos importantes entre sociedades tradicionales de pequeña escala y poblaciones modernas extensas que viven en ambientes evolucionarios novedosos. Este

artículo provee información sucinta y actualizada sobre una gama de interrogantes e hipótesis de posible interés para los demógrafos y otros a que los teóricos evolucionarios se dirigen usando una variedad de compilaciones de datos tradicionales e históricos. Se sugieren aquí oportunidades para trabajos de colaboración entre teóricos evolucionarios y demógrafos históricos y se destacan tópicos relevantes para la demografía moderna.

Muerte en la frontera: Eficacia y consecuencias no intencionadas de la política estadounidense de control de la inmigración

WAYNE A. CORNELIUS

Se examina en este artículo la eficacia de la estrategia de control de la inmigración—puesta en práctica por el gobierno de los Estados Unidos desde 1993—en reducir las tentativas ilegales de entrada, y se documentan algunas de las consecuencias no intencionadas de esta estrategia, especialmente un aumento marcado de mortalidad de migrantes no autorizados a lo largo de ciertos segmentos de la frontera de Estados Unidos-México. Los datos disponibles sugieren que la actual estrategia de hacer cumplir la ley de frontera ha resultado en un encauzamiento de los flujos de migrantes no autorizados hacia áreas de mayor riesgo, ha aumentado las cuotas de pago que cobran los contrabandistas de gente, y ha desalentado al migrante no autorizado que ya está en los Estados Unidos de volver a su lugar de origen. Sin embargo, no hay evidencia que la estrategia disuade o impide un número importante de nuevas entradas ilegales, en particular dada la ausencia de un real esfuerzo de reducir el empleo de migrantes no autorizados aplicando la ley en el lugar de trabajo. Una expansión del programa de trabajadores temporales, una legalización selectiva de trabajadores mexicanos no autorizados que residen en los Estados Unidos, y otras propuestas en consideración por los gobiernos de los Estados Unidos y México, probablemente no reduzcan la mortalidad de migrantes que resulta de la estrategia actual de hacer cumplir la ley de frontera.

Autonomía de la mujer en India y Pakistán : Influencia de religión y región

SHIREEN J. JEJEEBHOY
ZEB A. SATHAR

Se comparan en este artículo las vidas de mujeres y se examinan las dimensiones de su autonomía en dos regiones de Asia meridional—Punjab en Pakistán y Uttar Pradesh en la India Septentrional, y Tamil Nadu en la India Meridional. Al hacerlo, se examinan los factores contextuales que subyacen las diferencias observadas y se evalúa hasta que punto pueden atribuirse estas diferencias a la religión, nacionalidad, o distinciones culturales de norte-sur. Los hallazgos sugieren que aunque la autonomía de la mujer—en cuanto la toma de decisiones, movilidad, estar libre de una relación amenazadora con el marido, y acceso a y control de recursos económicos—está restringida dentro de los tres contextos, la mujer en Tamil Nadu se encuentra considerablemente mejor que las otras mujeres, desconsiderando la religión. Los hallazgos dan poco apoyo a la sugerencia que la mujer en Pakistán tiene menos autonomía o control sobre su vida que la mujer en India. Tampoco ejerce menos autonomía la mujer musulmana—ya sea India o Pakistana—en cuanto a su propia vida que la mujer hindú en el subcontinente. Más bien, los hallazgos sugieren que en las estructuras patriarcales y estratificadas por género que gobiernan la porción septentrional del subcontinente, el control de la mujer sobre su vida es más restringido que en la región meridional.

Patrones espaciales de la transición de la fecundidad en distritos de India

CHRISTOPHE Z. GUILMOTO
S. IRUDAYA RAJAN

La dinámica de la fecundidad de India a nivel de distrito es examinada en el artículo usando un índice niño-mujer desarrollado de los cuatro censos de India de 1961 a 1991. Se emplean técnicas estadísticas y geoestadísticas para evaluar el cambio de fecundidad a través de distritos y períodos. Se

evidencia en cada región un descenso de fecundidad, pero sí existen considerables diferenciales regionales. Un análisis por conglomerados de perfiles de fecundidad indica que ha surgido en India un claro patrón de fecundidad espacial, y el patrón se ha intensificado debido al proceso de descenso de la fecundidad.

Primeras impresiones del censo de 2000 de China

WILLIAM LAVELY

El censo de 2000 de China tiene varias innovaciones notables, incluyendo un largo formulario muestral conteniendo puntos detallados sobre migración, vivienda, y empleo. Datos preliminares son indicadores de rápida urbanización y continuación de rápidos cambios sociales en los años de la década 1990, y un manifiesto éxito del esfuerzo del gobierno de reducir el crecimiento de la población. Aunque una encuesta pos-enumeración indica que en general la calidad de los datos es buena, el alza en la "población flotante" móvil y las presiones del programa de planeamiento de nacimientos causaron problemas para la enumeración de migrantes y niños pequeños. En dos aspectos importan-

tes los datos que hasta la fecha han sido liberados se han mantenido mudos: la fecundidad y el aumento en la razón de sexo.

El impacto del VIH/SIDA sobre la mortalidad adulta en Zimbabwe

GRIFFITH FEENEY

En junio de 2000, un estimado 25 por ciento de adultos vivían con VIH/SIDA en Zimbabwe. Datos estadísticos sobre el impacto de la epidemia, aunque problemáticos en muchas formas, son mejores para Zimbabwe que para muchos otros países de África al Sur del Sahara. Este análisis presenta estimaciones de mortalidad adulta en Zimbabwe basadas en múltiples fuentes, incluyendo muertes registradas ajustadas por declaraciones incompletas, estimadas en aproximadamente un 50 por ciento. Una comparación de estimaciones de diferentes fuentes de datos muestran que están sujetas a errores considerables. Al mismo tiempo, las estimaciones no dejan dudas que el riesgo de mortalidad adulta en Zimbabwe más que dobló entre 1982 y 1997. La evidencia que este aumento se debe a muertes de SIDA es circunstancial, pero muy firme; no existe otra explicación competitiva creíble

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