Forum: On the demography of South Asian famines

This issue of Forum is devoted to a discussion of Tim Dyson's articles 'On the demography of South Asian famines' which appeared in *Population Studies* 45 last year.

Age-patterns of famine-related mortality increase: implications for long-term population growth

Jane Menken and Cameron Campbell

Population Studies Center, University of Pennsylvania, 3718 Locust Walk, Philadelphia, PA 19104–6298 USA

Abstract

In this paper, we reaffirm Watkins and Menken's (1985) conclusion that there is 'little likelihood that famines will be a major determinant of population growth in the future, any more than ... in the past'. We find that age and sex-specific patterns of famine mortality change that have markedly different proportional change in group-specific mortality can nevertheless lead to similar trajectories for population size. Subsequent growth is related more to relative reductions in survival by age than to relative increases in mortality. We also comment on the use of Dyson's conception index as a measure of the onset of famine.

On the basis of careful analysis of three nineteenth-century famines in the Indian subcontinent, the 1943–1944 Bengal famine and the 1974–1975 Bangladesh famine, Dyson (1991a,b) challenges accepted thinking on the short-term effects of famine in South Asia and argues that many notions about the consequences of famine may be incorrect. His work illuminates our pictures of the evolution of the response of birth and death rates to famine conditions, of the interaction of famine conditions and epidemics, and of the age and sex composition of famine deaths. In this paper, we comment on his claim that conceptions provide a sensitive index of the development of famine and conclude that they do so only in retrospect. We next discuss the controversy over age and sex differentials in the impact of famine and attempt to recast the issue in terms of the impact first on survival rather than deaths, and then on resumption of population growth. We use a computer-simulation model to provide illustrations of the effects of different age and sex patterns of mortality change during famines on subsequent population growth.

Conception rates

Dyson (1991a:22) contends that the fertility of a population was 'affected at a far earlier state in the build-up to famine than was its mortality' and that the 'level of conceptions...constitutes a reasonably sensitive index of the development of famine'. We are puzzled by these statements for several reasons. First, in these populations, conceptions were not observable until the birth occurred nine months later, by which time death rates were, in most cases, increasing. For the 1943–44 famine in Bengal, the increases in his conception index appear to precede price increases (Dyson, 1991b, Figure

6). As he discusses, conditions other than food shortage may have led to the early change in the conception index.

An alternative explanation for the observed patterns of birth and death rates is that both the fertility and the health of these populations were affected by changes in the availability of food. In both cases, the effects were not observable until some time later, in that deaths do not occur immediately, but rather follow cumulative decline in health and may await the renewal of an infectious disease, as Dyson suggests. The difference is that poor health can be reversible, while a conception that failed to occur is always reflected in the births nine months later. Like death rates, birth rates may also reflect cumulative effects of food shortage since, according to Pebley et al. (1985), intrauterine mortality is affected by poor nutrition.

Therefore, we agree only to a limited extent with Dyson's claim that conceptions can provide a sensitive index of the evolution of famine. In the past, they were not observable while the famine was developing, so could not be used to predict famine. This situation is likely to hold today. In addition, without an increase in mortality, it is doubtful that a crisis would be classified as famine. Therefore, the usefulness seems to be circumscribed: when there is a famine, the change in health and fertility conditions can be traced back by shifting birth rates back nine months to serve as a proxy for live-birth conceptions.

We are far more persuaded by his description of the difference in concentration of the effects. Live-birth conceptions may be affected over an extended period of rising concern about food conditions, even before the mortality crisis has begun. The peak in deaths, however, is likely to be much more sharply delineated if most deaths occur when, after an extended period of deteriorating health, infection strikes. It may, as Dyson suggests, even coincide with improvement in food intake.

A final set of comments returns to Dyson's use of the birth rate as a proxy for the conception rate nine months earlier. By so doing, he implicitly assumes, first, that intra-uterine mortality is not affected by famine conditions, secondly, that registration includes all live-born infants, even those who die soon after birth; and thirdly, that if births are under-registered, the level of underregistration is constant through time, even during famine. While the assumptions regarding birth registration may hold for the data that Dyson uses, they are less plausible when applied to other historical populations, for example China's, so that conception indices may be of limited utility in different circumstances. Where many births were never registered, especially if the child died while still very young, reliable rates of birth and conception are difficult to estimate, especially on a monthly basis as required by Dyson's method. Fluctuations in the conception rate would be impossible to distinguish from the effects of variations in infant and child mortality in later months.

Even in the data that Dyson employs, conception rates during the famine may be underestimated by birth rates displaced by nine months. Pebley et al. (1985) show that intrauterine mortality in Matlab increased as the nutritional status of the mother deteriorated, especially when nutritional status was measured by weight gain in the previous month, but also when measured by pre-conception weight. What may appear to be fluctuation in the conception rate nine months previously could in part reflect variation in intrauterine mortality in the intervening months.

Age and sex pattern of famine mortality change

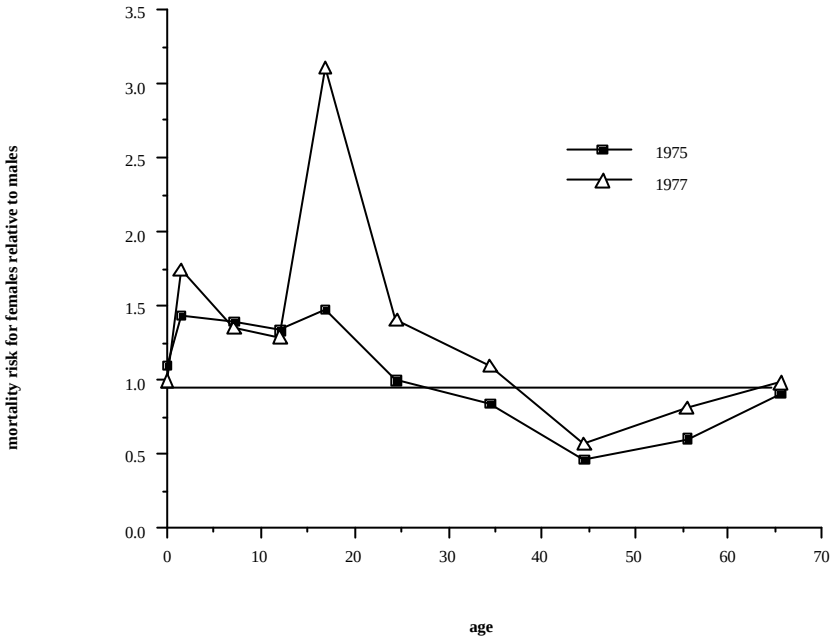
According to Dyson (1991b), use of flawed or incomplete data has been the major culprit leading to faulty conclusions about the mortality impact of famine. Analyses of the effects of the 1943–1944 Bengal famine by Sen (1981) and Greenough (1982) are both problematic because they relied on data for West Bengal alone, which at the time of the famine included only one-third of the population of Bengal. Dyson found that the data were questionable because they were adjusted before they were published, and the assumptions built into the adjustment procedure were later treated as findings. He

employs new data on this famine that cover the entire country and are available by month rather than by year. Dyson also presents evidence that Chen and Chowdhury's (1977) analysis of the 1974–1975 Bangladesh famine was flawed. Some age-specific death rates used in the analysis seem to have been incorrectly taken from years other than the ones listed in the tabulation. In addition, in order to calculate the longest possible time series, they relied exclusively on data from the 132 villages that were part of the Demographic Surveillance System from its inception. In many age groups, rates were based on only a few deaths, and may vary considerably from year to year. These data have been widely used to represent the age and sex pattern of the mortality impact of famine (see for example Watkins and Menken 1985, Caldwell and Caldwell 1989, Menken and Phillips 1990). In this new analysis, data from 228 villages are used, so that the number of individuals in the sample is almost doubled.

Dyson (1991b:289) somewhat surprisingly suggests that the change in mortality for males in Matlab has been 'obscured by a general concern with excess mortality of females'. Yet as early as 1978, Ruzicka and Chowdhury (1978:5) commented on the fact that male mortality for 1975 exceeded female mortality from age 25 on and Watkins and Menken (1985:656) discuss the mortality advantage of women during famines. Figure 1, which uses Dyson's (1991b, Table 9) data, shows the relative risk of dying for females compared to males in 1975 and 1977. The famine in 1975 sharply reduced the female disadvantage for teenagers and reduced the age at which women began to have lower mortality than men. Based on similar data, Watkins and Menken (1985:656) concluded that women are favoured in famine situations. Therefore, we find no disagreement between Dyson and a number of earlier analysts on this issue.Dyson's conclusion that relative mortality increases during famine were highest in the age range which normally enjoyed the lowest mortality, older childhood and young adulthood, contradicts the findings in Chen and Chowdhury (1977). Dyson shows that their claim that relative increases were highest in the age groups that already had high mortality, that is among children and the elderly, was based on faulty data.

In their review of information on the age pattern of mortality response to famine, Watkins and Menken (1985) did not find a universal pattern of mortality increase that characterized all societies. Variations in response to famine could reflect the effect of social and cultural norms governing the allocation of resources during crisis, as well as region-specific age patterns of cause of death. While Dyson's findings may be valid for South Asia, it seems quite possible that other age patterns could characterize the response of other historical populations.

Figure 1
Age-specific relative risk of dying for females compared to males: Matlab, 1975 and 1977



Even for South Asia, it may be too soon to reach some of these conclusions. Dyson's results about the most recent Bangladesh famine depend heavily on his decision to compare the average death rates of 1975 and 1976–1977. Razzaque et al. (1990) compared three cohorts of children born during the famine, conceived during the famine, and conceived after the famine in villages that were on the riverside, and therefore expected to be vulnerable to flooding. They found that children born during the famine experienced higher mortality through the second year of life, while those conceived during the famine had higher infant mortality. The 1976 rates in the youngest age groups are therefore influenced by the famine-related risks – whether they came from measles, as Dyson speculates, or are carryover effects from the famine experience. Dyson used the average rates for 1976 and 1977, so his results are surely influenced by this problem.

The inconsistency between Dyson's results and the results in Chen and Chowdhury (1977) in fact point to the need for recalculation of the Matlab time series of vital rates, using consistent geographical areas, time periods, and computer programs. The original rates were estimated when computer facilities were less than ideal; it is not surprising that errors are now being found in these data. Since all records are still available, it should be possible to correct the time series. This task is not a simple one, however, since there are at least three periods of varying geographic coverage.

In 1966, the Demographic Surveillance System was established and operated in the 132 villages used in the Chen and Chowdhury (1977) analysis. In 1968, 101 villages were added, a few of which were merged or dropped; Dyson uses figures that come from the remaining 228 villages. In 1978, data

collection ended in 79 villages, leaving 149 villages for which more recent information is available. Thus, one important need is for three, or possibly four, parallel series: (1) the 132 villages from 1966 either to the present or until 1978 if some of them were dropped; (2) the 228 villages from 1968 until the present or until 1978 if some of them were dropped; (3) the current 149 villages from 1968 until the present, and (4) that subset of the current 149 villages that has been continuously observed since 1966.

Even these series may not resolve the problem of estimating the age and sex differentials in the impact of the famine. It may be necessary to use time periods other than calendar years. Razzaque et al. (1990) claim that the famine ran from July 1974 to June 1975. For this reason, years that begin in May, such as Chen and Chowdhury used, may reflect the famine conditions better than the calendar year. There is, however, a cautionary note: D'Souza and Chen (1980) suggest that rates for adult males may be biased upward because able-bodied men 'migrated to cities in search of food and work' leaving the less healthy in Matlab.

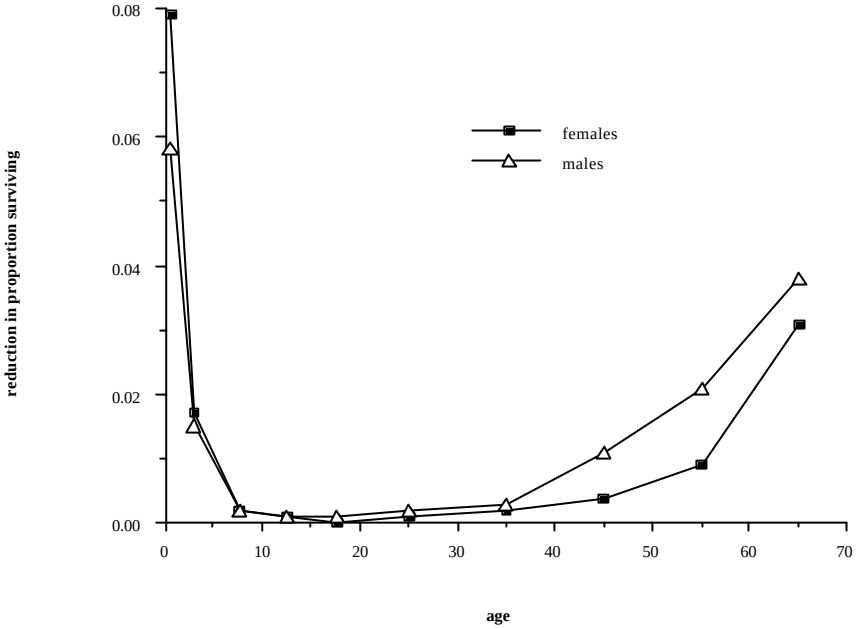
We strongly suggest that these recalculations be carried out, along with an examination of migration so that consistent time series are available on which to base firmer conclusions about age and sex differentials in famine impact.

Why are we interested in *proportional* increase in mortality?

We want to raise this question as a serious issue. Dyson, and many previous investigators including the senior author of this note, have treated the proportional increase in mortality as the significant indicator of the effect of famine: the greater the proportional increase in mortality at a specific age, the greater the effect of famine, regardless of the base or 'normal' mortality risk for that age. Dyson explicitly criticizes some analysts for using the absolute increase as their measure. If, however, we are interested in how a population recovers, in size or along other dimensions, from famine, we may well be concerned with the change in the survival rates, especially in those age groups that will contribute to future generations. Figure 2 gives the reduction in 1975 in the proportion surviving one year when the 1975 mortality rates are compared with the 1977 figures (taken from Dyson 1991b, Table 9). (The shape of this curve changes little when the average of the 1976 and 1977 rates is used as the non-famine baseline.) The reduction in survival is greatest for those age groups that were most at risk under normal circumstances.

Coale (1973), in a classic paper, considered the effects of mortality change on the age structure of a population. He found that age distribution was preserved if each age group experienced the same *absolute* change (positive or negative) in mortality rate, that is, if for each person, regardless of age, the added risk of dying during a famine was the same, so that change in risk was *age-neutral*. Under this regime, each age group contains fewer individuals in the following year than it would have without the mortality change, and the proportional reduction is exactly the same in every age group. That is, if every age group is, say, ten per cent smaller than it would have been, then so is the total number, so that age structure is, therefore, maintained.

Figure 2
Age-specific reduction in survival rates for males and females: Matlab, 1975 compared to 1977



In contrast, if every age group experiences the same *proportional* change in mortality, it is the age distribution of deaths that is preserved. The number of deaths at each age is multiplied by a constant, so that the proportion of the total deaths that occur at age *a* is unaltered. In this case, the population age distribution moves toward greater concentrations in those ages where mortality is usually low, since the age groups with the highest usual mortality rates have the greatest decrease in survival.

Finally, the effect of *disproportional* mortality change depends upon whether or not the highest increases occur in age groups which normally have high mortality. If so, the concentration of the population into low-mortality ages is even greater than in the case of proportional-mortality change. The reverse occurs (increased proportions left in high-mortality age groups) if there is a negative relationship between normal mortality level and proportional increase during the crisis.

From this discussion, we can consider the short-term effect of the different types of mortality change on the population age distribution, assuming that the usual J-shaped relationship between age and death rates holds. We consider three cases:

1. If *mortality increases at all ages by a constant proportion,* the survival probabilities of infants and the older population decline the most. The surviving population is then concentrated more in the reproductive ages.

2. If *mortality increases disproportionately at the older ages*, then the survivors have a younger mean age and are even more concentrated in the reproductive ages than under Case 1.

3. An *age-neutral mortality increase* does not change the age distribution of the population and the same proportion is in the reproductive ages after the famine as before.

If famine affects fertility, the effect is similar to the impact of an increase in infant mortality: the population is more concentrated in the reproductive ages.

The short-term effects on the birth rate after the crisis will differ according to which of these types of famine occurred. If we assume that age-specific birth rates return to their original levels, a population that experiences the age-neutral famine will immediately return to its previous crude birth rate, death rate, and growth rate. A population with proportional mortality change will have a higher crude birth rate, because more are in the reproductive ages. It will, in the short term, grow more rapidly than when famine was age-neutral. A population with disproportionate mortality change will have even higher birth rates right after the crisis and will regain its original size more quickly.

The long-term impact of famine mortality

The earlier work by Watkins and Menken (1985) examined the moderate to long-term effects under two assumptions about the age-specific impact of famine, namely, that there was no age selectivity, and that the age pattern of increase followed roughly that of the Chen and Chowdhury data. There was no difference in the effect of famine on males and females. Simulating famines of different duration (two years and five years) in populations in which the underlying rates of growth ranged from zero to one per cent, Watkins and Menken found that recovery time[1] was faster and the population was larger 90 years after the end of the famine, when the reproductive age group was less affected by famine than in an age-neutral famine.

The age patterns reported by Chen and Chowdhury (1977) and by Dyson (1991b) both affect survival of the non-reproductive age groups disproportionately, leading to population age distributions that favour fertility and, therefore, to shorter recovery times.

To examine the effects of the different assumptions, we rewrote the simulation model to incorporate sex differences in mortality change and simulated the three types of famine, with two versions of non-proportional change. Table 1 shows the assumptions about the relative shape of age-specific mortality change that were used; only case 2b incorporates sex-specific impact. These proportions were adjusted so that the death rate in the first year of the famine was either 110 per cent or 150 per cent of the stable level. In all cases, fertility dropped by one-third during the famine. Immediately post-famine, age-specific fertility and mortality rates returned to their original levels. The original population was based on a West-model life table with expectation of life for females of 27.5 (Coale and Demeny 1983).

1 The number of years from the start of the famine until the population regained its pre-famine size.

Table 1
Age pattern of mortality change due to famine

	Change in age-specific mortality				
	Proportional				Additive
Case	1	2a	2b		3
Age Group	Constant	Chen and Chowdhury	Dyson Male	Female	Age Neutral
0	c	1.35c	1.45c	1.64c	+k
1	c	1.27c	1.44c	1.40c	+k
5	c	1.40c	1.23c	1.28c	+k
10	c	1.03c	1.36c	1.43c	+k
15	c	1.18c	1.90c	1.04c	+k
20	c	1.18c	1.81c	1.36c	+k
30	c	1.18c	1.91c	1.60c	+k
40	c	1.18c	2.32c	1.79c	+k
45	c	1.43c	2.32c	1.79c	+k
50	c	1.43c	2.10c	1.63c	+k
60	c	1.43c	1.56c	1.44c	+k
65+	c	1.43c	1.56c	1.44c	+k

Table 2 gives the two measures of the impact of these famines: the recovery time and the population size 90 years after the famine ends. The pattern is clear, and whether we follow Chen and Chowdhury or Dyson makes little difference. Under both assumptions (cases 2a and 2b), the recovery time is slightly shorter and the population size 90 years later is slightly larger than when all mortality rates change by the same proportion (case 1). When famine is age-neutral (case 3), the population takes longer to regain its original size and is smaller 90 years later than under the other assumptions.

One *caveat* is in order. These results are based on the assumptions that women return to their original fertility rates at the end of the famine. If fertility is affected for a long time after the crisis because male mortality widowed women or made it more difficult for them to find a spouse, then the effects may differ from those shown here. An approach to examining the impact of famine in future studies might be through consideration of changes in reproductive value under these circumstances.

Table 2
Effect on recovery time and long-term population size of different age-specific patterns of famine-related mortality increase

Stable population growth rate		0.0%		0.25%		0.5%		1.0%	
% inc. in CDR	Duration of famine	Recovery time	Pop. size after 90 yrs	Recovery time	Pop. size after 90 yrs	Recovery time	Pop. size after 90 yrs	Recovery time	Pop. size after 90 yrs
No change in death rates									
	2 years		1000		1258		1582		2498
	5 years		1000		1268		1606		2574
Case 1. Proportional increase in each age group									
110%	2 years	***	912	35	1147	16	1442	9	2273
	5 years	***	802	89	1016	44	1285	19	2052
150%	2 years	***	891	46	1120	20	1407	11	2217
	5 years	***	759	95+	960	56	1214	27	1937
Case 2a. Chen and Chowdhury									
110%	2 years	***	917	33	1152	15	1448	9	2282
	5 years	***	812	84	1027	41	1298	18	2071
150%	2 years	***	896	43	1126	19	1414	11	2227
	5 years	***	770	95+	973	53	1230	25	1959
Case 2b. Dyson									
110%	2 years	***	917	32	1153	15	1449	9	2285
	5 years	***	813	83	1029	40	1302	18	2079
150%	2 years	***	897	42	1127	19	1416	11	2231
	5 years	***	772	95+	977	52	1235	25	1969
Case 3. Uniform increase in each age group									
110%	2 years	***	897	43	1128	21	1418	11	2235
	5 years	***	762	95+	965	55	1228	28	1948
150%	2 years	***	870	57	1094	28	1374	14	2164
	5 years	***	705	95+	892	71	1128	36	1798

Discussion

While we have disagreed with some of Dyson's conclusions, we want to comment on substantive aspects of his analysis that merit particular attention. First, it would appear from inspection of his time series (Dyson 1991a,b) that conceptions were more closely tied to price levels during the nineteenth-century famines than during the twentieth-century famines. In all three of the nineteenth-century famines, the conception index tracks the price index quite closely. In the twentieth-century famines, though, the conception index seemed to move more independently, responding only to dramatic shifts in the price level. That the response of conceptions to price differed over time suggests that there was a change in the way individuals decided whether or not to have children or in the reserves available in hard times.

Dyson's results also suggest the possibility that population responses to famine may have been region-specific. In the nineteenth-century South Asian famines, mortality did not rise in tandem with prices. Rather, the most severe mortality response was delayed until the arrival of the next monsoon season, at which point rates increased dramatically. The majority of these deaths could be attributed to diseases like malaria, not starvation.

From this perspective, famine could be considered a distal determinant of mortality, operating through a set of proximate determinants that are likely to have varied by climate and society. In the case of South Asia, these proximate determinants were diseases that already dominated mortality there, namely malaria and to a lesser extent diarrhoea.

In a colder region of the world, the proximate determinants might have been respiratory diseases. It is interesting to speculate on what the famine response would look like in a colder climate: perhaps a winter-time peak in deaths, the result of normally mild-respiratory infections that could become acute when poor nutrition weakened the body's immune response.

The possibility that societies vary in the way that they allocate resources during a crisis may also be a determinant of the mortality response to famine. If Dyson's assessment of the age-pattern of famine mortality is correct, it suggests the possibility that societies in South Asia were characterized by a willingness to devote resources to the young and the aged during crisis, at the expense of young adults. It is also possible that, for biological reasons, increases in the incidence of malaria affected young adults more than children or the elderly.

Dyson's investigation greatly adds to our knowledge of the detailed effects of famine. The results of our simulation are complementary, in the sense that they take the results forward in time and examine the moderate to long-term sequelae. As before, we find that the effects of the extreme famines we simulated are rather small. In addition, especially if one is interested in long-term effects, we suggest that the use of survival rates would be preferred over the use of death rates to assess famine impact. Although Dyson has suggested that the age-pattern of famine-mortality change differs from the one used by Watkins and Menken (1985), his new pattern still leads us to the same conclusion: 'Unless famine intensities exceed those upon which our calculations were based, there is little likelihood that famines will be a major determinant of population growth in the future, any more than they appear to have been in the past' (Watkins and Menken 1985:669).

References

Caldwell, J. and P. Caldwell. 1989. Famine et mortalitŽ en Afrique. Pp.361–383 in *MortalitŽ et SociŽte en Afrique*, ed. G. Pison, E. van de Walle and M.D. Sala-Diakanda. Travaux et Documents, Cahier n.124. Paris: INED.

Chen, L. and A.K.M.A. Chowdhury. 1977. The dynamics of contemporary famine. Pp.409–426 in *International Population Conference, Mexico* Vol. 1. Li• ge: International Union for the Scientific Study of Population.

Coale, A. 1973. Age composition in the absence of mortality and in other odd circumstances. *Demography* 10:537–542.

Coale, A. and P. Demeny with B. Vaughan. 1983. *Regional Model Life Tables and Stable Populations*, 2nd edition. New York: Academic Press.

D'Souza, S. and L.C. Chen. 1980. Sex differentials in mortality in rural Bangladesh. *Population and Development Review* 6:257–270.

Dyson, T. 1991a. On the demography of South Asian famines, Part I. *Population Studies* 45:5–25.

Dyson, T. 1991b. On the demography of South Asian famines, Part II. *Population Studies* 45:279–297.

Greenough, P.R. 1982. *Prosperity and Misery in Modern Bengal: The Famine of 1943–1944*. Oxford and New York: Oxford University Press.

Menken, J. and J.F. Phillips. 1990. Population change in a rural area of Bangladesh, 1967–87. *Annals of Political Science* 510:87–101.

Pebley, A.R., S.L. Huffman, A.K.M.A. Chowdhury and P.W. Stupp. 1985. Intra-uterine mortality and maternal nutritional status in rural Bangladesh. *Population Studies* 39:425–440.

Razzaque, A., N. Alam, L. Wai and A. Foster. 1990. Sustained effects of the 1974–75 famine on infant and child mortality in a rural area of Bangladesh. *Population Studies* 44:145–154.

Ruzicka, L.T. and A.K.M.A. Chowdhury. 1978. *Vital Events and Migration – 1975: Demographic Surveillance System – Matlab Vol. 4*., Scientific Report No. 13. Dacca: Cholera Research Laboratory.

Sen, A. 1981. *Poverty and Famines: An Essay on Entitlement and Deprivation*. Oxford: Clarendon Press.

Watkins, S.C. and J. Menken. 1985. Famines in historical perspective. *Population and Development Review* 11:647–675.

Inhibited conception and women's agency: a comment on one aspect of Dyson's 'On the demography of South Asian famines'

Paul Greenough*

Visiting Professor of History, South Asia Institute, Heidelberg University, 6900 Heidelberg 1, Germany.

Tim Dyson's review of the demographic character of five major nineteenth and twentieth-century South Asian famines (Dyson 1991a,b) deepens but also complicates our understanding of the societal crisis that results from widespread household-level food shortages. His rigorous comparison justifies itself by bringing into focus several until-now rather hazy famine processes; the most important of these, in my view, is the fall in conception that invariably precedes the onset of famine mortality and that continues well after famine mortality ceases. Although other scholars have commented in passing on this phenomenon, no one has so clearly shown the depth and duration of inhibited conception, which, in the examples reviewed, was more serious (because earlier and longer-lasting) than previously recognized.

In the course of demonstrating lowered fertility during historical South Asian famines, Dyson introduces a statistic he calls the 'conception index' or CI. The CI compares the conception rates in a famine-affected population to the non-famine conception rates in the same population at an earlier, more stable time. Whether or not Dyson's terminology is adopted – seeing that the index is intended to measure a decline in conception, it might be more apt to call it a *non*-conception index – his method, which projects post-famine birth registration data nine months backward against baseline fertility during famine, will surely be adopted by others who are engaged with famine demography and have access to registration data.

Some historians are committed to the term 'mortality crisis' as a synonym for famine, but Dyson's work implies that inhibited conception is a better diagnostic than crisis mortality because it is more consistently present. Note that the Bombay presidency famine of 1896–97 had only a modest mortality spike compared to other Victorian famines but a very substantial fall in conception (Dyson 1991a:17-

* **I am grateful to the editors of *Health Transition Review*** for the invitation to comment on Tim Dyson's two articles. I also acknowledge the facilities of the Center for International and Comparative Studies of the University of Iowa and the South Asia Institute of Heidelberg University, while preparing my comments.

18), while in more recent famines, for example, the Bihar famine of 1966–67 and the Maharashtra famine of 1970–73, there was presumably the fall-off in conception but virtually no famine mortality. Nowadays there is a marked tendency for famines to be relieved by outside agencies to the point that mortality from starvation and disease is effectively suppressed (the Bangladesh famine of 1974–75 was exceptional in this regard), which raises the question whether modern, relieved, low-mortality famines have much in common with historical, unrelieved, high-mortality famines. Dyson shows that the link between the two types is the signature effect of inhibited conception.[2]

In famines where mortality has occurred, it is not evident from Dyson's paper that the number of infants *not* conceived is regularly more or less than the number dying. That is, the magnitude of fertility losses during famines might profitably be compared with the magnitude of mortality losses. The calculation of hypothetical lives lost or saved is a familiar activity in public-health work. Concretely, the mortality from diseases and injuries for every age up to age 65 is sometimes converted into a statistic called the 'potential years of life lost' (PYLL). That is, every death prior to 65 years (this number is arbitrary) is considered premature, and the cost to society as measured in years of forgone activity is allocated to specific causes. (Causes of infant mortality loom large for obvious reasons in such calculations.) The magnitudes of all the PYLL attributable to each cause of death, for example, infections, injuries, congenital defects, environmental exposures, are then scaled and used to prioritize health interventions. This kind of logic could be applied also to conception losses during famines to demonstrate in fuller fashion, by going beyond simple mortality, the toll to society.

Dyson is somewhat vague about the socio-cultural routes to lowered conception associated with past South Asian famines. Or, phrased positively, he shows restraint in exploring the behaviours that lie behind the fertility effects he reveals. Changes in conception *pari passu* with price movements and with the deterioration of exchange entitlements are acknowledged, but how environmental factors like scarcity and unemployment come to affect biological processes like fertility is sketchy. We are told that

> through a variety of well-known mechanisms (e.g., reduction in coital frequency, deferment of marriages, decreases in fecundity, spousal separation through migration) these populations reduced their conception rates as a direct response to increasing levels of distress (the latter here roughly proxied by food price rises) (Dyson 1991a:22).

The idiom of 'mechanism' with its suggestion of autonomous process is misleading in this context; while some direct effects of hunger or reproductive physiology are likely, human decisions with all their contingencies and calculations are also prominently involved. In any case none of the 'mechanisms' Dyson mentions is really 'well-known'. (Who has investigated or would care to demonstrate 'coital frequency' during a famine?) Nor has any of these responses been proved to be as direct as or more direct than any other. I do not fault Dyson for these opacities which are not demographic in nature. It may be possible, however, that a more fine-grained analysis of vital registration records would allow demographers to glean information about the ages, districts of origin, castes, income and occupation, and family composition information that, taken together, would help to discriminate those women who conceived during famine from those who did not. Even so, demographic methods alone cannot recreate the contexts of choice in which fertility was inhibited.

Interestingly, Dyson portrays the fall in conception that occurs before the height of a famine as a *protective* state that enhances women's survival. Pregnancy and childbirth, always hazardous to a woman's health in rural South Asia, are particularly dangerous during a famine. Thus, if conception is

2 Like Dyson, I presume that famine is not a merely historical category and that continuing economic inequities and administrative incompetences will lead to subsistence crises in the future followed by global interventions to provide minimal relief.

suppressed, women who would otherwise be at risk of fatal complications will escape them. This escape is Dyson's principal explanation for the consistent pattern of lower adult female mortality increases than adult male mortality increases during famines. At one point he refers to the inhibition of conception as 'anticipatory', putting the term in quotation marks to express, I presume, an ambivalence about the degree of conscious agency. Yet, in his conclusion, he suggests that 'we might be wrong to rule out an element of conscious planning in explaining such anticipatory behaviour' (Dyson 1991b:293).[3] This is something to sit up and take notice of: the large-scale demographic effect which Dyson's research has highlighted appears to him plausibly to be the result of deliberate efforts taken by scarcity-affected persons, presumably women, to control fertility. In short, altered reproductive behaviour during famine may be something that women help make happen instead of having forced upon them. Are the 'mechanisms' of inhibited conception, then, substantially voluntarist rather than autonomic in nature? Is inhibited conception during famines even appropriately considered a *loss* (an involuntary forfeit), as assumed in prior paragraphs, or should it be considered instead a *boon* (a desired benefit)?

There is a version of South Asian famine history, to which I personally have contributed, that represents rural women as victims of authoritative, indeed, masterful adult males who appropriate decision making about all matters related to the survival of the lineage, the family and its individual members (Greenough 1982:215–225, 245–253). Specifically, anecdotal and numerical data in my study of the 1943–44 Bengal famine suggested that male heads of households regularly seized whatever resources were locally available to secure their own survival, or, alternatively, that they regularly abandoned their wives and children and sought work and relief alone at a distance. These behaviours occurred on such a large scale that they resulted in many thousands of abandoned women remaining homeless after the famine, women who had to be cared for by charity. A controversial aspect of my argument was the assertion that the adult male appropriation of resources and abandonment of dependants was culturally sanctioned. I found no contradiction in Bengali culture between adult male self-preservation and widespread moral values that favoured adult males over females. A newer analysis of the same data by Bina Agarwal, however, emphasizes a conflict-cooperation model of household-level decision making in which scarcity-affected Bengali women 'negotiated' with men according to their 'resource endowments' and 'fall-back positions'. Because women began their negotiations from a disadvantaged position *vis-ˆ-vis* fathers and husbands, the result was still massive impoverishment, abandonment and death, but at least women are represented as having actively participated in deciding their destinies rather than simply submitting to 'despotic' males (Agarwal 1990).

Agarwal (1990), following an analytic track opened up by Amartya Sen, assumes that Bengali men in 1943–44 succeeded in imposing their wills on women and in securing their survival in greater numbers because they held larger resource-bundles and more numerous 'fall-backs' at the beginning of the crisis; hence moral codes that prioritized adult males over females may have provided men with convenient ideological cover during a resource free-for-all but were not materially responsible for their survival. I am no more persuaded now than I was ten years ago that a whole gender category reasons so amorally, that is economistically, but I am attracted by Agarwal's argument that Bengali women were active during the crisis. Her position can be directly related to Dyson's study, because while Agarwal says there are women who 'bargain' for their subsistence during a famine, Dyson suggests there are women who 'plan' to inhibit their conception at the same time. Why should not these be the same

3 Citing the ethnographic evidence of Caldwell, Reddy and Caldwell 1988: 196–219; I have not yet been able to examine this work.

women? And why cannot the verbs be interchanged to hypothesize a South Asian woman who, in crisis situations, bargains with her husband over conception and makes plans for her own subsistence? Linked together, these suggestions give an impetus to a view of rural South Asian women as resolute, self-interested actors rather than passive victims. The problem with this formulation is that there are so few data. Neither Dyson's nor Agarwal's reinterpretation offers new observations on these issues.

It seems evident that we should now explore, both in historical studies and through field work, the hypothesis that scarcity-affected South Asian women take recourse on a large scale to behaviours calculated to save their lives. Existing accounts of women's subsistence-enhancing activities during famine – diet-shifting, lowering intake, foraging, selling off jewellery and utensils, taking up paddy-husking and other crafts, conversion, begging etc. (Agarwal 1990) – can be easily fitted within the hypothesis, but we need also to explore the less visible 'fallbacks' such as hoarding, taking secret loans, stealing, and prostitution, which the hypothesis of women's active agency implies. We also want to know in much more detail about the conception-inhibiting 'mechanisms' already mentioned by Dyson: do women in fact *initiate* the postponement of marriages in times of scarcity? Do married women *refuse* intercourse with their husbands and initiate their separation? What other planning goes into fertility inhibition? Do they, for example, procure abortions? While we need to learn much more about these matters, and while we want to know about the mental resolutions and moral visions of the women most affected, I suspect that such knowledge will only be available from informants who are actually trapped in real-world situations of grave scarcity. Grave ethical considerations will intervene, and these intrusive enquiries can be prosecuted only if they are accompanied by direct relief, and carefully explained to informants; and if the women's consent is secured and the interviews are consistent with their self-interest and dignity.

References

Agarwal, Bina. 1990. Social security and the family: coping with seasonality and calamity in rural India. *Journal of Peasant Studies* 17:341–412.

Caldwell, J.C., P.H. Reddy and P. Caldwell. 1988. *The Causes of Demographic Change: Experimental Research in South India*. Madison: University of Wisconsin Press.

Dyson, T. 1991a. On the demography of South Asian famines, Part I. *Population Studies* 45:5–25.

Dyson, T. 1991b. On the demography of South Asian famines, Part II. *Population Studies* 45:279–297.

Greenough, Paul. 1982. *Prosperity and Misery in Modern Bengal: The Famine of 1943–44*. New York: Oxford University Press.

A note on 'conscious planning'

John C. Caldwell[1], P.H. Reddy[2], Pat Caldwell[1], Indra Gajanayake[3], W.K. Gaminiratne[4], Indrani Pieris[5] and Bruce Caldwell[5]

[1]*Health Transition Centre, NCEPH, ANU, GPO Box 4, Canberra ACT 2601, Australia*
[2]*Population Centre, Bangalore, India*
[3]*Australian Institute of Health, Canberra*
[4]*Centre for Aboriginal Economic Policy Research, ANU*
[5]*Graduate Diploma in Demography, ANU*

Tim Dyson (1991b:293) writes:

One recalls Caldwell, Reddy and Caldwell's conclusions regarding survival strategies in south India during the drought of 1980–83; the birth rate declined, yet mortality failed to increase. Their ethnography suggests that we might be wrong to rule out an element of conscious planning in explaining such anticipatory behaviour. [70] The Matlab data show that fertility declined among women irrespective of their age or parity; this may have applied in earlier famines (footnote 70 reads, 'See J.C. Caldwell, P.H. Reddy and P. Caldwell, *The Causes of Demographic Change*, Madison, Wisconsin, 1988, especially chapter 9).

Two issues should be taken up.

The first is that the 1980–83 famine was, at least in southern Karnataka (once Mysore), a mild famine by the standards of the great famines discussed by Dyson. People were distressed; many lost their incomes; nearly everyone had to eat less food while most turned to foodstuffs they normally would have regarded as inferior or inedible; and almost all forwent all expenses except the most urgent if only because of the fear that the famine might be sustained and might intensify. The mildness of the famine explained the absence of any impact on mortality. The belt-tightening that was undertaken included the cancelling of nearly all festivals and ceremonial occasions, including religious festivals, for these events incur both direct costs and indirect ones in the form of new clothing which is regarded as essential. Marriages were markedly affected, principally because of the cost of dowry, although wedding expenses also played a role (Caldwell, Reddy and Caldwell 1988:208–209). Around 20 per cent, even of marriages already planned, were deferred, and the level reached 30 per cent among richer peasants who were obliged in normal times to have lavish weddings. Most families simply gave up planning marriages. Our estimate was that the marriage rate fell to one-quarter of its pre-drought level. When those who had deferred planned marriages were cross-questioned about their reasons, everyone spoke of dowry and wedding expenses, and not a single family referred to the subsequent costs and problems associated with pregnancies, births and the raising of children. The important points are the following.

(1) The marriage rate fell.

(2) Subsequently, the birth rate fell, and this was entirely explained by the fall in the first-birth rate arising from the deferment of marriage.

(3) Family-planning acceptance continued to rise slowly, and greater acceptance played an insignificant role in short-term fertility decline.

(4) The mortality level remained steady.

This explains why birth rates may begin to fall before death rates. It does not explain the fact that fertility decline in Matlab was not parity-related. The important point is that the 'conscious planning' related entirely to marriage and not to fertility. This would seem to be likely to occur in any society with arranged marriage, related ceremonial weddings, and either dowry or bridewealth. We were repeatedly told in the 1973 Sahelian famines that marriages could not take place because the bridewealth could not be found.

The second issue is the response of the Indian Tamils on Sri Lanka's tea estates to the 1973–74 famine which affected them so severely that infant mortality rates rose from 70 to over 100 per thousand and fertility fell by one-fifth. During anthropological demographic work in this area in 1987 our team repeatedly asked those of reproductive age during that period, why they had not conceived. By far the most common reply was that they had not had sexual relations. When asked why this was so, they most commonly said that those dire times were not the occasion for such frivolity or that people were weak and sick and that couples just did not do such things at such times. Some referred to the subsequent problems that would be incurred by pregnancy, birth and the addition of an infant, but this was not the common response. The emphasis was far more on the the unlikelihood of sex than on the

problems of fertility even though this was a population experiencing hard times and where a woman's pregnancy might curtail her income from tea-picking. Once again, the 'conscious planning' was not primarily a matter of fertility planning, but this experience conforms well with the finding that the Matlab fertility decline was unrelated to age or parity. It should be added that this was a population among whom sexual abstinence in the marriage after childbirth or terminally is culturally acceptable. It might also be noted that there apparently was some rise in illegal abortion during this period.

References

Caldwell, J.C., P.H. Reddy and P. Caldwell. 1988. *The Causes of Demographic Change.* Madison: University of Wisconsin.

Dyson, T. 1991. On the demography of South Asian famines, Part II. *Population Studies* 45:279–297.

Famine Reactions

Tim Dyson

Department of Population Studies, London School of Economics, Houghton Street, Aldwych, London WC2A 2AE, United Kingdom

I am grateful to the *Health Transition Review* for the chance to reply to the three valuable 'comments' which have been made about my recent paper on the demography of South Asian famines (Dyson 1991a,b). In each of the three comments there is much which extends the issues I addressed, and much with which I agree. However, here I will mainly address points of disagreement, some of which may have arisen because of a failure on my part to make myself sufficiently clear. Since Menken and Campbell make the most extensive and pointed remarks, I will follow them in discussing first, conceptions, second, mortality, and third, the long-term effects of famine.

Conceptions

I certainly stand by my statement that in the five major South Asian famines which I analysed (those of 1876-78, 1896-97, 1899-1900, the Bengal famine of 1943-44 and the Bangladesh famine of 1974-75) the fertility of the population was affected at a far earlier stage in the build-up to famine than was its mortality: in the sense that conceptions were reduced at a much earlier stage. Menken and Campbell say that they are puzzled by this because 'in these populations, conceptions were not observable until the birth occurred nine months later...'. They go on to state that the observation could not be used to predict famine (for example, because of the nine months lag from conception to birth, and because of the absence of birth registration data in many countries) and that therefore its utility seems to be circumscribed.

I entirely agree with Menken and Campbell that the predictive utility of the observation is circumscribed. In saying that reduced conceptions are often 'a reasonable sensitive index of the development of famine' I did not intend to imply any predictive utility, nor did I use the word 'prediction', for precisely the kinds of reason which Menken and Campbell give. My main point was simply to state something which seems to be strongly suggested from the data provided in the original paper, namely that in each of the five famines conceptions were much reduced before the rise in deaths. Since Menken and Campbell go on to state that 'Live-birth conceptions may be affected over an extended period of rising concern about food conditions, even before the mortality crisis has begun', they do not seem to be far from accepting this position.

Let me give a concrete example of how a decline in conceptions may usefully provide 'a reasonably sensitive index of the development of famine', but in a way which is specifically *not* predictive. Menken and Campbell themselves cite Razzaque et al.'s (1990:147) view that in the Matlab area of Bangladesh the 1974-75 famine ran from July of 1974. But the conception index which I calculated using the Matlab data suggests that the fall-off in conceptions dates from around January of 1974, and perhaps a little earlier (see Dyson 1991b:288). Therefore I would argue on this basis that the famine may have started well before July. Incidentally, this interpretation would accord well with the detailed time profile of the 1974-75 famine provided by Alamgir which starts with the period January-March of 1974 (Alamgir 1980:118-140).

Citing the work of Pebley et al. (1985) which showed that intrauterine mortality in Matlab increased as the nutritional status of the mother decreased, Menken and Campbell point out that what may appear to be changes in the conception rate nine months previously could partly reflect variation in intrauterine mortality. I accept that there may well be some validity to this. But one should also note

that Menken and Campbell themselves only propose this as a partial explanation. Moreover, Pebley et al.'s results were strongest for foetal mortality during the last trimester of pregnancy. But variation in such late intrauterine mortality would not account for the fact that in the nineteenth-century famines, my conception index tracks price movements so closely (though it could point to the relevance of early foetal wastage). In any event, there is certainly no shortage of plausible alternative explanations relating to changes in coital frequency to explain the time trends which are observed. Finally in this context, it is interesting that Chen and Chowdhury have specifically stated that 'Fetal wastage rates in Matlab *thana* ... were not affected by the crises [of 1971 and 1974]' (Chen and Chowdhury 1977:422).

In discussing the issue of conception rates, Menken and Campbell state that 'without an increase in mortality, it is doubtful that a crisis would be classified as famine'. However in India, historically severe food crises, which were certainly officially classified as famines, did occasionally occur without net increases in mortality. In my paper I cited the case of the famine in Bombay Presidency in 1905-06; another example is the famine in Punjab in 1896-97. In both cases ecological conditions associated with drought triggered a state of famine, while at the same time probably leading to a reduction in some infectious diseases, especially malaria, and in Bombay in 1905-06, plague: hot, dry conditions perhaps tending to kill the rat flea *X. Cheopis* (Gottfried 1983:9). The fact that there was no *net* increase in mortality, although there were most of the other signs of calamitous famine including, probably, some deaths related to problems in geting food, hardly seems sufficient reason to me to withdraw use of the epithet 'famine'.

In a very different context here, Caldwell and colleagues describe as a 'mild famine' the 1980-83 drought in Karnataka during which the mortality level remained steady while the birth rate fell. In some cases, perhaps that of Karnataka, the appropriateness of the word 'famine' may be largely a matter of semantics. But I think it would be a serious mistake to assume that a famine must always involve an increase in the population death rate. In this sense I indeed subscribe to Paul Greenough's summary statement here that 'inhibited conception is a better diagnostic [of famine] than crisis mortality because it is more consistently present'. So although a reduction in conceptions may be a poor 'predictor' of a population's experience of famine, *ex post facto* it may constitute a powerful piece of evidence that a famine has actually occurred. Unfortunately, in a world in which national governments are all too ready to deny the existence of conditions of famine, such evidence may be of more than passing interest.

This brings us explicitly to the issue of why the level of conceptions declines in a population that is subject to famine. To Paul Greenough I should explain that I used the term 'anticipatory' mostly to indicate the simple fact that the conception decline occurred before the rise in the death rate. (Incidentally, this is a very different sequence of events from those associated with epidemics, in which the conception decline is usually reactive with the rise in deaths.) The term 'anticipatory' was placed in quotation marks to express my considerable doubt as to whether there was any significant degree of conscious agency involved in accounting for such behaviour. I believe that the major causes of these reductions in conceptions were precisely those I mentioned: spousal separation through migration, general declines in coital frequency, deferment of marriages and decreases in fecundity. However, at the end of the paper, and citing the work of Caldwell, Reddy and Caldwell (1988), I very cautiously suggested that there might be an 'element' (by which I meant a small component) of 'conscious planning' behind such reductions in conceptions.

In this context I am very grateful for Caldwell et al.'s comment here explaining that in the case of the 1980-83 crisis in Karnataka the 'conscious planning' element was simply that marriages were deferred, and as a result the first birth rate fell. But I also note from them that in the case of the 1973-74 food crisis among tea estate Tamils in Sri Lanka, a few respondents did mention the problems of pregnancy, birth and the presence of an additional infant, when explaining why they had not had sexual

relations during the famine. Note also that there may have been some increase in illegal abortions. For my purposes these observations by Caldwell and his colleagues are sufficient to suggest that there may be a residual element of conscious planning apropos fertility during famines. Indeed, Caldwell's observations relate precisely to the area of research which Greenough identifies as important.

Famine mortality

Menken and Campbell are surprised when I say that the fact that male mortality deteriorated most during the 1974-75 famine in Matlab has been obscured by a general concern with excess female mortality in the Matlab data. Clearly, we now all agree that male mortality probably did deteriorate most in 1975. And, of course, a number of other analysts, including Watkins and Menken (1985:656), have indeed concluded that generally male mortality tends to increase more than female mortality during times of famine.

However, I stand squarely by my statement that a general concern with the undoubtedly important subject of excess female mortality in Matlab in normal times has tended to conceal the basic fact that overall (that is, at all ages combined) male mortality deteriorated most in the 1974-75 famine. Nowhere in the extensive literature on the demography of this famine have I found a single statement to the effect that overall it was the mortality of males which deteriorated most in 1975. Even Ruzicka and Chowdhury (1978) – who were the source of the life expectation estimates which I used to illustrate this fact – restrict their comments to excess male mortality beyond age 25. And they go on to suggest that chance variation due to small numbers may be at least partly responsible for the high ratio of male to female death rates at these later ages (Ruzicka and Chowdhury 1978:5). Likewise D'Souza and Chen also present Matlab data which show that male mortality in the age groups 15-44 and 45-64 was much higher than that for females. However the main explanation D'Souza and Chen propose for this (and they propose it twice) is that in 1975

> a higher proportion of males in the adult age groups migrated to the cities in search of food and work; as a result, the population left behind may have been sex-selectively biased in favour of less healthy males staying home and thus at greater death risk (D'Souza and Chen 1980:266).

This suggestion that a 'selection effect' may have been involved is worth comment. Unfortunately, we do not have detailed information on the socioeconomic composition of rural outmigrants from Matlab during this famine. Presumably, people who were too ill to move had no option other than to stay where they were, and some of them must have died in Matlab. On the other hand, members of relatively better-off households, who had or could afford to buy food, may have had no reason to leave the area. It may well have been that the most impoverished and weaker sections of the society were overrepresented in the migration flows out of Matlab. And migration itself could have further elevated their chances of death. In short, it seems quite plausible that out-migrant males were more likely to die than those who remained. However, in the absence of data this is only speculation.

To sum up on Menken and Campbell's 'surprise'. The literature analysing the Matlab data for the famine years of 1974-75 has stressed, again and again, that in the childhood ages the increased mortality was disproportionately experienced by females (see, for example, Chen and Chowdhury 1977:415, D'Souza and Chen 1980:264, Razzaque et al. 1990:153). In contrast, the evidence of excess male mortality at adult ages has received much less attention. And when it has received attention, it has been in ways which have tended to cast doubt on its authenticity. So far as I am aware, no one has looked at overall mortality during the Bangladesh (Matlab) famine and has plainly stated that, in relative terms, males were probably hardest hit. Indeed the impression which has emerged has tended to be precisely

the reverse. For example, to quote Alamgir (1980:145), himself quoting Chen and Chowdhury (1977:415):

> During baseline years, female mortality consistently exceeded male mortality in all age groups except infant deaths. The age-specific differentials were more pronounced in children 1-4 and 5-9 years and in the childbearing years. Disaster tended to accentuate even further these sex differentials, particularly among children.

I agree with Menken and Campbell that the age-pattern of mortality increase which I identified in the three nineteenth-century famines, and in the Bengal famine of 1943-44, is unlikely to apply in all famines, even within South Asia. Perhaps the most important single distinguishing feature of these four famines was that they were all immense disasters: 'great famines' in Caldwell et al.'s phrase. My results apropos the age-pattern of mortality increase in the Bangladesh famine are certainly influenced by the use (following D'Souza and Chen 1980) of average death rates for 1976-77 to represent 'normal' conditions. With the available data there was little else which could be done. However I think the exercise was worthwhile in that it provided some hints (no more) of similarities with the age pattern of increase found for the earlier major famines. In using the Matlab data in this context, I was at pains to stress its several major limitations and the unsatisfactory nature of using the 1976 figures as baseline death rates.

Menken and Campbell provide a concise summary of the considerable task that would be involved in correcting and interpreting the Matlab time-series of vital rates for the period around the famine. They strongly urge that a consistent time series be assembled so that age and sex differentials in famine impact can be examined. To me an even more important issue which such an exercise might help to address is the distribution of famine mortality through time in 1974-75. This is because the Matlab data are unique among the famines which I studied in showing a long 'tail' of elevated mortality, lasting throughout 1975. In other words, the distribution of famine deaths does not seem to have been merely an accentuation of the normal seasonal mortality pattern, which in Matlab peaks around November-December.

If this 'tail' feature is real then it would indeed represent a significant departure from the experience of the earlier major famines. However it seems possible that the 1975 'tail' may be at least partly spurious. This could have happened if some of the deaths which occurred to outmigrants in late 1974 were only recorded (and misdated) by the Demographic Surveillance System (DSS) in 1975 as people gradually returned home to Matlab. Thus a real 'peak' in mortality could have been obscured and dissipated to form a 'tail'. In informal discussions about this possibility with people who were intimately involved with the DSS at the time, I have met with responses which range from complete denial that this could have happened, to acceptance that it could well have occurred.

Menken and Campbell raise the important issue of absolute and proportional mortality increases during famine. They note that I mostly address proportional increases and state that I explicitly criticize some (unnamed) analysts for using absolute measures. In response I make three points. First, my reason for working mainly with proportional increases was because that is what previous analysts have used. Some previous researchers, including Watkins and Menken (1985), seem to have concluded that the greatest proportional increases in death rates happened to young children and the elderly. But I found this a little suspect a *priori* precisely because the absolute base or 'normal' mortality risks at these ages are already high. Secondly, no particular criticism was intended of analysts who have used absolute increases as their measure. I can only assume that Menken and Campbell believe that I was criticizing Greenough's (1982) use of absolute increases in death rates in his analysis of the 1943-44 Bengal famine. But no criticism of him was intended. I was merely pointing out that there is nothing surprising in Greenough's finding that the largest absolute increases in mortality happened to young

children and the elderly. Thirdly, as in many areas of demographic research, we clearly need to consider both absolute and proportional changes in tandem when considering the mortality impact of famine.

Let me give another illustration of this third point. In the major famines which I analysed the proportional mortality increases experienced by males tended to be greater than those experienced by females, especially in the prime adult reproductive years. But one cannot interpret this without cognizance of the fact that the normal absolute levels of female mortality in the reproductive age groups were often markedly higher than those of males. To be sure, other factors such as 'anticipatory' fertility decline (which in this context 'benefits' women), greater female body-fat reserves, and the probable greater propensity of adult men to migrate, may all be relevant to explaining why proportional increase in male mortality exceeded those of females at these ages. But so too is the absolute level of the base from which the proportional increases are being measured. And in South Asia female mortality tends to be particularly high in the reproductive years. In this context Greenough is incorrect to say here that anticipatory decline is my 'principal explanation' for the lower proportional mortality increases experienced by adult females. All of the factors mentioned above are probably important.

Incidentally, similar considerations apply to attempts to deduce inferences about a society's 'preferences' in the allocation of food and other resources during famines from interpretation of the age-pattern of mortality increase. I am sceptical of such attempts, partly because many factors combine to influence the age-pattern of famine mortality, and partly because it is unclear whether the pattern of proportional or absolute increase by age, which can be quite different, should be the subject study.

Finally, on the subject of the mortality impact of famine, Menken and Campbell are right to say that previous analyses of the 1943-44 Bengal famine have arrived at faulty conclusions. In particular, Dyson and Maharatna (1991a) have recently shown that figures of 3 or 4 million excess deaths are significant exaggerations; a figure of about 2.1 million is much more likely. On the other hand, Greenough is incorrect in his assumption here that there was 'virtually no famine mortality' in Maharashtra in 1970-73. Even in what was a relatively well-managed disaster there were probably around 130,000 excess deaths (Dyson and Maharatna 1991b).

The long-term impact of famine

My main interest in famine demography was in the short-term effects. It does not surprise me that Menken and Campbell's new simulations, which incorporate the different age-patterns of mortality increase, make no real difference to the principal conclusion previously propounded by Watkins and Menken (1985:669) namely, that 'Unless famine intensities exceed those upon which our calculations were based, there is little likelihood that famines will be a major determinant of population growth in the future, any more than they appear to have been in the past'. While I agree with this statement, I think it is important to stress two things which it does not mean.

First, it does not mean that famines were never major determinants of population growth in the past. In their original paper Watkins and Menken mentioned Lardinois's (1985) work on the Madras famine of the 1870s and McAlpin's (1983) and Davis's (1951) fine studies relating to India 'when there was virtually no growth between 1891 and 1901, and several major famines occurred' (Watkins and Menken 1985:665). In fact if one looks at India over the fifty-year period from 1871 to 1921, when the average annual rate of population growth was about 0.36 per cent, one can make out quite a strong case that famines were a major determinant of population growth; although this is partly dependent upon what is held to constitute 'major'. The famine of 1876-78 devastated vast regions well beyond Madras Presidency; the north of the subcontinent was badly afflicted by famine in 1877-79; there were then the huge successive disasters of 1896-97 and 1899-1900. Summing the estimates of excess mortality assembled by Seavoy (1986:242) for these famines produces a figure of over 16 million excess deaths

in a population that was roughly 280 million, although the true toll may well have been still greater. Then the widespread famine of 1907-08 also killed millions of people. And the existence of famine conditions in central and northwestern regions of India in 1918 may well have contributed to the gigantic mortality toll associated with the influenza epidemic (Mills 1989:251). Of course, in addition, there were many minor famines during 1871-1921. And one would also need to take account of the fertility loss associated with these various crises. In short, the emerging picture of India's historical demography is one which, *inter alia*, does lend some support to famines acting as a major determinant of population growth. It also provides empirical examples of how special factors could come into play in the immediate post-famine periods which helped to hasten population recovery (see the various studies in Dyson 1989).

Secondly, and unfortunately, acceptance of Watkins and Menken's conclusion does not mean that major famines with large-scale excess mortality are a thing of the past. With, for example, increasing problems in global per capita food levels, especially since the mid-1980s (Brown 1991, FAO 1991), with signs of mounting environmental stress in many parts of the world, and most important of all, with widespread social strife and warfare in places such as the former Soviet Union, and much of Africa, there are few reasons for confidence that Caldwell's 'great famines' can be firmly assigned to the past.

References

Alamgir, M. 1980. *Famine in South Asia: Political Economy of Mass Starvation*. Cambridge, Mass: Oelgeschlager, Gunn and Hain.

Brown, L.R. 1991. The New World Order. In *State of the World 1991*, ed. L.R. Brown. Washington D.C.: Worldwatch Institute.

Caldwell, J.C., P.H. Reddy and P. Caldwell. 1988. *The Causes of Demographic Change*. Madison: University of Wisconsin Press.

Chen, L. and A.K.M.A. Chowdhury. 1977. The dynamics of contemporary famine. Pp.409–426 in *International Population Conference, Mexico*, Vol. 1. Li• ge: International Union for the Scientific Study of Population.

Davis, K. 1951. *The Population of India and Pakistan*. Princeton: Princeton University Press.

D'Souza, S. and L. Chen. 1980. Sex differentials in mortality in rural Bangladesh. *Population and Development Review* 6:257-270.

Dyson, T. ed. 1989. *India's Historical Demography: Studies in Famine, Disease and Society*. London: Curzon Press.

Dyson, T. 1991a. On the demography of South Asian famines, Part I. *Population Studies* 45:5-25.

Dyson, T. 1991b. On the demography of South Asian famines, Part II. *Population Studies* 45:279-297.

Dyson, T. and A. Maharatna. 1991a. Excess mortality during the Bengal famine: a re-evaluation. *The Indian Economic and Social History Review* 28:281-297.

Dyson, T. and A. Maharatna. 1991b. On the demographic consequences of the Bihar famine of 1966-67 and the Maharashtra drought of 1970-73. Paper presented at Society for the Social History of Medicine Conference on Famine and Disease, Christ's College, Cambridge. Forthcoming in *Famine and Disease*, ed. P. Garnsey and J. Henderson.

Food and Agriculture Organization. 1991. *The State of Food and Agriculture 1990*. Rome.

Gottfried, R.S. 1983. *The Black Death*. New York: Free Press.

Greenough, P. 1982. *Prosperity and Misery in Modern Bengal: The Famine of 1943-44*. New York: Oxford University Press.

Lardinois, R. 1985. Famine, epidemics and mortality in South India: a reappraisal of the demographic crisis of 1876-78. *Economic and Political Weekly* 20:454-465.

McAlpin, M.B. 1983. *Subject to Famine: Food Crises and Economic Change in Western India, 1860-1920.* Princeton: Princeton University Press.

Mills, I. 1989. Influenza in India during 1918-19. In *India's Historical Demography: Studies in Famine, Disease and Society,* ed. T. Dyson. London: Curzon Press.

Pebley, A.R., S.L. Huffman, A.K.M.A. Chowdhury and P.W. Stupp. 1985. Intra-uterine mortality and maternal nutritional status in rural Bangladesh. *Population Studies* 39:425-440.

Razzaque, A., N. Alam, L. Wai and A. Foster. 1990. Sustained effects of the 1974-75 famine on infant and child mortality in a rural area of Bangladesh. *Population Studies* 44:145-154.

Ruzicka, L.T. and A.K.M.A. Chowdhury. 1978. *Vital Events and Migration - 1975: Demographic Surveillance System - Matlab*, Vol. 4. Scientific Report No. 12. Dacca: Cholera Research Laboratory.

Seavoy, R.E. 1986. *Famine in Peasant Societies.* Westport: : Greenwood Press.

Watkins, S.C. and J. Menken. 1985. Famines in historical perspective. *Population and Development Review* 11: 647-675.