The impact of a public-health intervention on sex differentials in childhood mortality in rural Punjab, India*

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Abstract

This paper examines the effects of a public-health intervention program on sex differentials in health and mortality during childhood. Among the different health-service packages offered as part of the experimental design, those including nutritional services seem to have been more successful in reducing excess female mortality. The reason for this success appears to have been careful follow-up of undernourished children by project workers. The results also indicate that, consistent with earlier research, girls with surviving older sisters had higher mortality rates after their first month of life. Contrary to earlier research, however, boys with surviving older brothers also have higher mortality rates, at least between the ages of one and three years. We conclude that these effects for boys and girls cannot be attributed to problems associated with larger family size, since the number of older siblings of the opposite sex (regardless of survival status) does not generally appear to be related to children's chances of survival.

In most contemporary populations, male mortality is higher at all ages than female mortality (Heligman 1983),[1] apparently both because of males' higher degree of biologically-based susceptibility to disease and because of a higher prevalence of poor health habits and risky behaviour, especially among adolescent and adult men (Waldron 1986). In many parts of South Asia, the picture is quite different. After the neonatal period, female mortality rates are higher than those for males throughout childhood and often throughout adulthood until the end of the reproductive period (Visaria 1969; D'Souza and Bhuiya 1982; Dyson and Moore 1983; Das Gupta 1987; Basu 1989).

In this paper, we examine the effects of a public-health intervention program on sex differentials in health and mortality during childhood in the Ludhiana District of Punjab state in northern India. We also attempt to determine whether public-health programs seeking to reduce the effects of preferential treatment for sons in areas like the Ludhiana District should focus on *all* girls in a family, or whether, as Das Gupta's (1987) results suggest, some girls are at substantially higher risk of poor health and mortality than others in the same family. Finally, we look briefly at whether the public-health intervention in the Ludhiana District had a greater effect on reducing mortality for daughters identified as being at higher risk.

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1 This pattern has also been observed in other populations in the past (Preston 1976; Johansson 1990) as well as in some contemporary populations outside South Asia (Makinson 1986).

A great deal of demographic, health and social information is available for the Ludhiana District, the site of two major health-intervention research projects and several other studies conducted since the 1950s. The pioneering Khanna Study in the 1950s, which investigated the potential demand for contraception in pre-industrial agrarian societies, took place in 16 villages in the Ludhiana District (Wyon and Gordon 1971).[2] The Narangwal Project, described later in this paper, was conducted in a different group of villages in the same district in the early 1970s (Kielmann et al. 1983; Taylor et al. 1983). There have also been several studies by anthropologists, including one by Das Gupta (1987) on child health and another by Nag and Kak (1984) on fertility attitudes and behaviour.

Table 1
Infant and child mortality (deaths per 1000 live births) by age at death, Ludhiana District, Punjab, India; and Matlab, Bangladesh

	Age at death (months)					
	0	**1–11**	**0–11**	**12–23**	**24–59**	**0–59**
Ludhiana District						
Khanna Study[a] Villages (1957–59)						
Mortality level	73.5	82.7	156.2	72.2[e]		
Male/female			0.86	0.44		
Narangwal Study[b] Villages (1965–73)						
Mortality level	54.6	51.2	103.0	29.6	9.6[f]	137.9[f]
Male/female	0.93	0.51	0.71	0.37	0.74	0.64
Khanna Study[c] Villages (1965–84)						
Mortality level	47.0	38.6	85.6	13.8	10.3	109.6
Male/female	1.18	0.53	0.82	0.51	0.65	0.76
Matlab, Bangladesh						
1974–77[c]						
Mortality level	73.0	58.2	131.2			
Male/female	1.16	0.82	1.00			
1983[d]						
Mortality level	64.0	42.0	106.1	41.8[e]	24.5[e]	47.7[e]
Male/female	1.01	0.77	0.90	0.72	0.53	0.73

[a] Gordon et al. (1965) as reported by Das Gupta (1987) Table 2.

[b] Calculated by the authors from the Narangwal Study data.

[c] Das Gupta (1987) Table 2.

[d] Shaikh et al. (1985) Table VI.

[e] Calculated per 1000 population in the age group rather than per 1000 live births

[f] Age group ends at 35 months rather than 59 months.

In Table 1 we present measures of infant and child mortality and of the sex differentials in mortality from three of these studies in the Ludhiana District. For comparison, we also show figures for the Matlab study area in Bangladesh where the International Centre for Diarrhoeal Disease Research, Bangladesh (ICDDR,B) has collected reliable vital statistics since the early 1960s. For each time period and location we present measures of overall mortality for all children of a given age and the ratio of male-to-female mortality for that age group. All measures are calculated per 1000 live births, except

2 The aims of the study, as specified by Wyon and Gordon (1971:18) were '(1) to test the power of existing contraceptive methods to change birth rates in a preindustrial population as exemplified by rural India; and (2) to inquire into factors within that population influencing the frequency of births, deaths and migrations, the cardinal determinants of altered population numbers'.

where noted, because they were presented this way in the original Khanna Study (Gordon et al. 1965) and in Das Gupta's (1987) follow-up. For both Ludhiana and Matlab at each time period, the South Asian pattern of sex differentials in mortality described above is apparent: males generally have lower mortality than females after the first month of life. In the first month of life, male mortality is equal to or higher than female mortality, except in the Narangwal Study.

The figures in Table 1 indicate that mortality has declined significantly during the period shown in both the Ludhiana District in Punjab and Matlab in Bangladesh. However, there is no evidence in Table 1 of a consistent improvement over time in female mortality levels relative to those for males.

Table 2
Cause-specific death rates based on lay-reporting by sex for Matlab, Bangladesh, 1983

	Less than 1 Year (Rates per 1000 Live Births)			1–4 Years (Rates per 1000 Person-Years)		
Reported cause	**Males**	**Females**	**F minus M**	**Males**	**Females**	**F minus M**
Tetanus	39.2	38.2	-1.0	0.9	0.4	-0.5
Diarrhoea/dysentery	7.6	8.4	0.8	7.7	17.1	9.4
GI tract infection	0.5	0.6	0.1	0.2	0.3	0.1
Respiratory infection	9.2	10.3	1.1	1.6	1.3	-0.3
Fever	6.3	9.8	3.5	2.4	3.7	1.3
Measles	1.1	1.4	0.3	1.5	1.8	0.3
Other infection	2.7	4.2	1.5	2.3	3.2	0.9
Violence and accidents	0.3	0.6	0.3	2.2	3.2	1.0
Organ/degenerative diseases	1.6	1.1	0.5	0.2	0.7	0.5
Unknown or other	33.4	37.1	3.7	2.8	5.3	2.5
All causes	100.9	111.5	10.6[a]	21.9	37.0	15.1[a]
Total deaths	377	400		270	41.4	
Live births	3738	3587				
Mid-year population				12340	11183	

[a] The figures in this column do not total exactly to the female/male differences in the mortality rate from all causes owing to rounding error.
Source: Figures calculated from Shaikh et al. (1985) Table V.

Reported cause-specific mortality rates for children younger than one year and 1–4 years in Matlab, Bangladesh, are shown in Table 2 separately for girls and boys. These data are based on lay-reports of cause, and suffer from the problems of misclassification and attribution of a substantial proportion of deaths to the 'unknown' category which are common to cause-of-death data of this type (Zimicki 1990). Nonetheless, the broad categories shown in Table 2 provide a general indication of whether the excess female mortality is due to particular types of causes. Also shown in this table are the differences between female and male cause-specific mortality rates for each age group. No single cause or set of causes appears to be responsible for higher mortality rates for females; rather, female rates are generally higher for most causes. For the first year of life, fever and unknown or other causes are responsible for a substantial portion of the female-male difference, while in years 1–4, diarrhoea and unknown causes are the major contributors to the difference. In both cases, the fact that these causes of death make the greatest contribution to the sex differential in mortality is partly because they are the major causes of death for children of both sexes. No comparable cause-specific death rate data by sex from the Khanna or Narangwal studies have been published to our knowledge, but distributions of deaths by cause for both sexes combined are shown in the Appendix. Gordon et al. (1965) report findings concerning sex differences in cause-specific mortality rates for Ludhiana in the late 1950s that are similar to those for

Matlab: no particular cause of death accounted for higher female than male mortality rates in childhood. Rather, except for neonatal tetanus, death rates from most causes were higher for girls than for boys.

Why are mortality rates higher for girls?

Evidence from studies in Punjab, as well as elsewhere in northern India and in Bangladesh, indicates that higher female mortality rates in childhood after the neonatal period, result from preferential treatment by family members for sons[3] (Chen et al. 1981; Miller 1981; Sen and Sengupta 1983; Bhatia 1983; Das Gupta 1987; Basu 1989; Freed and Freed 1989). The observation that male mortality rates are higher than those for females in the neonatal period is consistent with this explanation, since most causes of death in the first month of life are either beyond families' immediate control (related to congenital malformation, birth trauma, etc.), or are not due to sex-specific treatment of children (e.g. neonatal tetanus). After the neonatal period, environmental factors that are under control of the family such as nutritional intake, exposure to disease, breastfeeding, parental time and attention, and use of health-care services become predominant.

Table 3 summarizes results concerning preferential treatment of sons from the Narangwal Study and Das Gupta's more recent study in the Ludhiana District and from a study by Chen et al. (1981) in the Matlab study area. The basic pattern is consistent with reports of officials, health workers, and anthropologists in these populations (Miller 1981; Dyson and Moore 1983; Freed and Freed 1989) as well as with the fact that the sex differential in mortality does not appear to be due to any specific set of causes: sons receive preferential treatment in almost all aspects of child care that were measured.

In the Ludhiana District during the Narangwal Study, girls were breastfed on average for a shorter time. However, in a study in the Matlab area, Huffman et al. (1987) found that the median duration of breastfeeding and the median duration of full (unsupplemented) breastfeeding were, in fact, slightly higher for girls than for boys.

In both study areas, girls received less food. The ratios of male-to-female nutrient intake reported by Das Gupta for the Ludhiana District in the 1980s suggest a narrowing of the sex differential in food intake compared with the Narangwal Study in the early 1970s, since all the ratios in Das Gupta's study except for fat intake are very close to 1. Nonetheless, girls still received somewhat less food in the 1980s than boys, especially high-prestige food such as fats and milk. Lower food intake presumably contributes to the poorer growth observed for girls in both study areas, even when measured against sex-specific standards as in the Matlab study.

Table 3

Evidence on preferential treatment for boys from Ludhiana District of Punjab, India, and Matlab, Bangladesh

	Ludhiana District		
	Narangwal Study[a] 1970–73	**Das Gupta[b] 1984**	**Matlab Study[c] Early 1970s**
Breastfeeding duration	Jat Sikhs: Average duration 2 months longer for boys	—	Median duration of breastfeeding: Males 31.7 Females 32.8[d] Median duration of full breastfeeding: Males 6.0 Females 7.2[d]
Dietary intake	Among weaned children aged	Male-to-female intake,	Male-to-female,

[3] Makinson (1986) came to the same conclusion about the explanation for higher female mortality rates in childhood in Egypt.

	6 months to 3 years, ratio of male-to-female intake: Calories 1.16 Protein 1.19 Calcium 1.45 Iron 1.14 Vitamin A 1.28	For 0 to 1 year-olds: Cereals 0.76 Milk 1.09 Fats 1.22 Sugars 0.98 For 1 to 4 year-olds: Cereals 1.06 Milk 1.05 Fats 1.09 Sugars 1.03	for 0 to 4 year-olds: Calories 1.16 Protein 1.14
Anthropometry	Females significantly shorter and lighter than males at ages 1 to 36 months	—	Compared with sex-specific Harvard weight-for-age standards, among children under 5 years: 14.4% of female children were severely malnourished compared to 5.1% of boys, and 59.6% of girls were moderately malnourished compared to 54.8% for boys. (Results were similar for height-for-age).
Morbidity	Prevalence of gastro-intestinal symptoms somewhat higher for girls (0 to 3 years), but roughly the same for eye symptoms, and nasal discharge	—	Number of episodes of select diseases (diarrhoea, respiratory, skin, and eye-ear) roughly the same for male and female children
Treatment of illness	For children who subsequently died, proportion receiving care within the first 24 hours: 48% of girls 64% of boys	—	Males are roughly twice as likely to receive treatment at free ICDDR,B diarrhoea clinic. (Transport to clinic also free).
Expenditure on children		Male-female ratios for 0 to 4 year-olds: For clothing 1.36 For food 1.21	

[a] Kielmann et al. (1983) Tables 4–1, 4–2, 4–6, 4–7, 5–1, and text. [b] Das Gupta (1987) Tables 6 and 7.
[c] Chen et al. (1981) Tables 2, 3, 4, and text, unless otherwise noted. [d] Huffman et al. (1987) Table 2.

Surprisingly, Chen et al. (1981) show that despite differences in diet and other forms of care, the prevalence of illnesses, including diarrhoea, is roughly equal among girls and boys aged 0–4 years. However, boys are twice as likely as girls to receive treatment for an episode of diarrhoea, even though the treatment and transport to the clinic were provided free by ICDDR,B. Researchers in the Narangwal Study published less information about the sex-specific prevalence of morbidity and treatment sought for illness. They do report that the prevalence of gastro-intestinal symptoms was somewhat higher for girls early in their study,[4] but roughly the same for two other symptoms (Kielmann et al. 1983: Figures 7-4, 7-5, and 7-6). However, in an investigation of children who had died, they concluded that boys generally received medical attention earlier during their terminal illness than girls (Kielmann et al. 1983:200). Das Gupta (1987) also showed that families in her 1984 study spent less on medication, as well as less on clothing, for girls than for boys.

4 Later in their study the difference in the prevalence of gastro-intestinal and other symptoms for girls and boys was negligible.

On the basis of a study of samples of families originating in the Indian states of Uttar Pradesh in the north and Tamil Nadu in the south, who are currently residing in resettlement slums in Delhi, Basu (1989) argues that the reason underlying higher mortality rates for girls in northern India is more frequent use of health care for boys, rather than discrimination against girls in food allocation. Among families originally from Uttar Pradesh, who have higher female than male mortality for 0- to 12-year-olds, anthropometric indicators (weight-for-age and weight-for-height) show that girls are actually slightly *less* likely to be malnourished than boys. Conversely, among families originating in Tamil Nadu, whose mortality rates are lower for girls than for boys, girls are *more* likely to be classified as malnourished. By contrast, Basu shows that boys from Uttar Pradesh are more likely to get treatment than girls, whereas care was relatively equal for families originating in Tamil Nadu.

Basu's results on nutritional status differ from those in Table 3 for the Ludhiana District of Punjab state and for Matlab, Bangladesh. Our own hypothesis regarding the difference is that while preferential treatment for sons is the general explanation behind higher female than male mortality rates in northern India, the form that this preferential treatment takes may vary over time and in different social settings. While the respondents in Basu's study are poor and originally not from Delhi, they now live in a major urban area, where use of health services may be a more salient issue for children's health than in rural Punjab or Bangladesh. Furthermore, there are likely to be major cultural differences between Basu's Uttar Pradesh sample and the populations of the Ludhiana District and of Matlab. As discussed below, Das Gupta argues that the Jat culture, which is socially and economically predominant in Punjab and neighbouring Haryana, has an unusually strong preference for sons, and may be more likely to discriminate across the board against daughters. However, evidence from the Narangwal Study and from Das Gupta's study, shown in Table 3, suggests that, even in Ludhiana, the manner in which preferential treatment is given to sons may be changing. Specifically, as noted above, the differences in diet between boys and girls may have narrowed over time as economic and social conditions have changed. Preferential treatment for sons in the form of food allocation as well as other types of care may also be more common and pervasive in rural Bangladesh than among Uttar Pradeshis living in Delhi slums.

Why preferential treatment for sons?

Perhaps the most common explanation for preferential treatment of sons is that in the very strong patriarchal system prevalent in northern India, and especially in the northwest where Punjab is located, sons are essential while daughters are not (Cain et al. 1979; Miller 1981; Bhatia 1983; Dyson and Moore 1983; Das Gupta 1987).[5] Sons maintain and extend the family's lineage and existing social ties, farm the family land, inherit property, act as a hedge against financial disaster, and provide support for parents in old age. Daughters, on the other hand, are liabilities: they marry early and outside their villages, contribute little to household income, require increasingly large dowries and expensive weddings in order to marry, and provide no support to their families of origin after marriage.[6] Furthermore, they can bring disgrace to their families by violation of modesty and propriety before

5 It is important to recall, as Miller (1981) cautions, that there is considerable variation in marriage patterns and the social roles of women within northern India. However, strong patriarchal systems, exogenous marriage, and large dowry costs are common in northern India, and contrast sharply with less patriarchal kinship systems in the south.

6 Das Gupta (1987:92) reports that, in Punjab, the flow of resources from natal kin to daughters continues after marriage as well: 'Throughout a woman's life her father or brothers provide her with clothes and gifts for her in-laws on specified occasions... They are supposed to receive nothing in exchange. This lack of reciprocity is symbolized by the custom, widespread among many castes in North India, that a woman's father and brothers do not accept food or water in her husband's home'.

marriage, or by being shown to be inadequate wives after marriage. Speaking of the Jats in Punjab, Das Gupta (1987:92) explained:

> In this social structure women are conceived as transitory components, the vessels whereby the men of the lineage [into which they marry] reproduce themselves. This is made very clear by the fact that while men remember their genealogies up to several generations, women are not mentioned at all, except namelessly to clarify fine details of men's relationships to one another.

Women, as the primary caretakers of children, have considerable incentive to bear sons and maintain them in good health, because they themselves are entirely dependent on sons for their position in the family and for support in case of financial difficulties or old age.

Some observers have argued that the relative importance to families of sons and daughters results in preferential treatment for boys primarily when families are poor and under financial stress. Extreme economic pressure may force poor families to allocate their limited resources in order to improve their own chances of social and economic survival. Similarly, in other areas of the world, poor families may give, or may have given in the past, preferential care to the oldest children or to children thought to be the most likely to succeed (Scrimshaw 1978; Miller 1981). In northern India, where girls do not make important long-run contributions to the family and may be seen as a net loss, partly because of the need for a large dowry, it is argued that poor families may maximize their chances of survival by giving preferential treatment to boys. Consistent with this hypothesis is evidence from Matlab that during the severe famine in 1974-75, sex differentials in child mortality increased (Bairagi 1986). The reason was, presumably, that girls received even less food and other care than usual, relative to boys. It is also true that the elimination of excess female mortality in Sri Lanka took place during a period when family income was increasing, which suggests that improvements in living standards may have allowed families the 'luxury' of providing good care to both boys and girls (Arriaga and Way 1987).

However, other evidence suggests that preferential care for sons may not occur only in conditions of poverty. Das Gupta (1987) shows that per capita income levels for states in India are not correlated with sex ratios for the population of those states. In fact the two states with the highest levels of income, Punjab and Haryana, in 1983-84 had the most unbalanced sex ratios, reflecting substantially higher mortality for females than males. The figures in Table 1 also suggest that the disparity in male and female mortality rates in childhood has not diminished in the Ludhiana District and in Matlab, at least during the period covered, despite improvements in survivorship. Furthermore, tabulations from the Narangwal Study and by Das Gupta (1987) indicate that, within Punjab, the ratio of male-to-female mortality in childhood either does not vary with socio-economic status, or, as in the case of maternal education in 1984, is lower (i.e., excess female mortality is higher) for women with more education.

Another, more controversial, hypothesis advanced by Das Gupta (1987) and other anthropologists (see Miller 1981) is that discrimination against girls is a means by which families increase their control over the sex composition of their children. Many studies in India demonstrate that parents have a strong preference for bearing more sons than daughters. Das Gupta's results (1987: Table 1), for example, indicate that the number of additional daughters desired by women who had one surviving daughter was 0.03, whereas the number of additional sons desired by women who had at least one surviving son was 0.81.[7] She also shows that, in Punjabi families, girls with at least one surviving older sister have higher risks of mortality, compared both to girls with no surviving older sisters and to boys (regardless of the number of surviving older brothers and sisters they have). Despite their strong preference for sons,

7 Similarly, women in the Narangwal Study villages in 1969 reported desired numbers of sons and daughters that would produce a sex ratio of about 1.8 boys per girl (calculated from Taylor et al. 1983: Table 7–6).

families have no means of affecting the sex ratio at birth of their children.[8] Therefore, Das Gupta suggests, ‘neglect is applied selectively among female children’ (p. 95) – in particular, to girls who have a surviving older sister, so as to remove ‘unwanted daughters as early as possible...’ (p. 93). Das Gupta also suggests that reductions in fertility, which have occurred in recent years in the Punjab, may have increased discrimination against daughters who have surviving older sisters: families want to have fewer children, but still want to have two surviving sons.

The effect of a public-health program on sex differentials in mortality

With the foregoing discussion as background, we now turn to the problems posed for delivery of public-health services by patterns of intra-family resource allocation based on sex. The experience of Sri Lanka in South Asia (Arriaga and Way 1987) and of societies in other regions (Preston 1976; Johansson 1990) suggests that in the future, as economic and social patterns change, female mortality rates in childhood in northern India may be equal to or below those of males. However, the results described above indicate that preferential treatment for sons in this region remains an important aspect of family behaviour affecting children’s health.

Public-health workers have long realized that underlying preference systems used by families to allocate resources internally may affect – or, from the health workers’ point of view, distort – the impact of health services on family members. For example, adult males may benefit disproportionately from food supplements provided to families, even though the supplements were intended for children. Similarly, as noted above, Chen et al. (1981) show that parents in Matlab were much more likely to use free, readily available diarrhoeal treatment for boys, although they were encouraged to bring in any family member suffering from acute diarrhoea. Thus, paradoxically, in any area where preferential treatment for sons remains common, the introduction or expansion of public-health services, while bringing about an overall mortality decline, may actually exacerbate sex differentials in childhood mortality.

In this section, we investigate the effect of the Narangwal Study (specifically the Narangwal Nutrition Project) on sex differentials in mortality in the Ludhiana District. The central questions motivating the analysis are: (1) Can public-health interventions improve the health of girls relative to boys in areas such as Punjab in the early 1970s, while at the same time raising levels of child survival? and (2) Are some types of health interventions more effective than others in reducing sex differentials in mortality? The Narangwal Study was carried out in the early 1970s. While significant social, economic, and political changes have occurred in Punjab since the 1970s, we believe that a reanalysis of these data is worthwhile for several reasons. First, Punjab remains one of the states of India with the largest sex differentials in mortality and, as shown in Table 1, sex differentials in mortality do not appear to have changed dramatically, at least between the 1950s and the 1980s. Secondly, as described below, the Narangwal Study employed an experimental design that allows us to compare the effects of different child-health service ‘packages’. Thirdly, the Narangwal project team collected extensive data on a large population, which has unfortunately not been true in many other public-health experiments of this type. Finally, as described above, background information on demographic and social trends in the Ludhiana District in which the study was conducted is readily available from a variety of sources.

The Narangwal Study

The Narangwal Study was conducted in 26 rural villages by the Indian Council of Medical Research, in collaboration with researchers at Johns Hopkins University. The project had two central objectives: to determine whether an integrated family-planning and maternal- and child-health (MCH) program would

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[8] Except, of course, through sex-selective abortion; but this is not widely available or widely used.

increase acceptance of family-planning services; and to determine whether integration of a nutritional monitoring and supplementation program with a health-care program would improve children's health status more than a single program alone. The project is described in detail by Kielmann et al. (1983) and Taylor et al. (1983), and more briefly by Pebley (1984).

As shown in Figure 1, the Narangwal Study consisted of two overlapping projects known as the Population Project and the Nutrition Project. Each project involved provision of several alternative packages of family planning and/or health services to the population of villages according to an experimental design. The results presented in the rest of this paper come from the Nutrition Project, which focused primarily on issues related to child health. The Nutrition Project included four experimental[9] groups, receiving (1) health-care services (HC), (2) nutrition services (NUT), (3) combined health-care and nutrition services (HC+NUT), and (4) no services, i.e., the control group (CONT-N). The content of each service package is described below. The combined service group (HC+NUT) in the Nutrition Project was also part of the Population Project and received family-planning services in addition to children's health-care and nutritional services. The single service groups (HC and NUT), on the other hand, did not receive family-planning services. This is an important distinction because a conscious decision was made during the project to allocate roughly equal amounts of personnel time to each service group regardless of the types of services provided. Thus, in the Nutrition Project's combined services group (HC+NUT), personnel provided not only two kinds of child-related services but also family-planning services. Yet the amount of personnel time in the HC+NUT villages was roughly the same as in the villages where a single child-related service was provided (i.e., the HC group and the NUT group).

We also incorporate into the analysis results from the control group of the Population Project. Throughout the paper the Population Project control group (CONT-P) is combined with the Nutrition Project control group to increase sample size, since the original Nutrition Project control group included only two villages.

The Nutrition Project focused on children aged 0–36 months. Nutrition services for children in the nutrition (NUT) villages and the combined-services (HC+NUT) villages consisted of growth monitoring; nutrition education for parents, including encouragement of late weaning and nutritional supplementation starting at 4–6 months; and supplementation for children showing a weight deficit. The project set up feeding centres in each village, open twice a day, to which parents of children with growth deficiencies were encouraged to take them.

Health-care services for children in the health care (HC) and combined services (HC+NUT) villages consisted of curative and preventive care for common illnesses (especially infections) in clinics set up in each village, immunization,[10] and health education for parents. The main emphasis was on early diagnosis and treatment of infectious disease.

The Nutrition Project was carried out between 1969 and 1973, while the Population Project continued into 1974. Service packages were introduced at different dates at the beginning of the study. Data for this analysis come from an on-going pregnancy and birth file known as the Longitudinal Fertility Survey, and from a two-round retrospective Pregnancy History Survey. The files were thoroughly cross-checked, combined, and cleaned to produce a data file containing one record per child.

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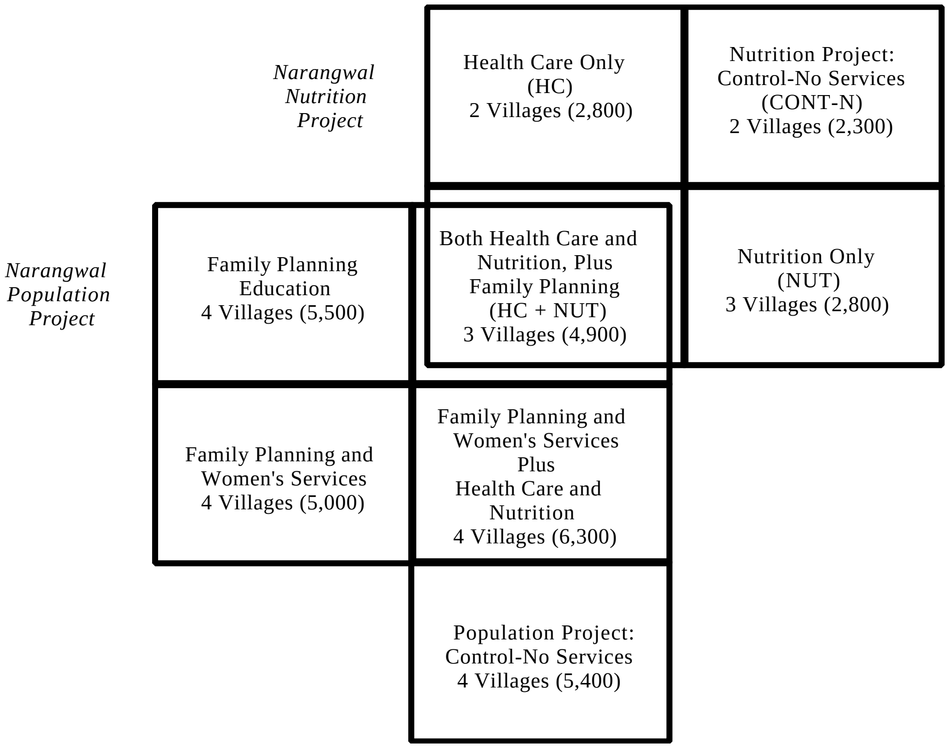
9 Throughout this paper we use the phrase 'treatment group' to refer to the villages that received one of the health-service packages, to distinguish them from the 'control group' that received no services. 'Experimental group' is used as a more general term referring to both control and treatment groups.

10 Children received DPT in the village clinic, and BCG and measles vaccines in regularly scheduled immunization campaigns. In these villages, pregnant women were also given tetanus toxoid injections. The government health services provided smallpox immunization.

Figure 1
Experimental design of the Narangwal Study



Source: Kielmann et al. (1983), p.17.

Changes in infant and child mortality

Figure 2 shows infant, 1-3-year-old, and child (0-3-year-old) mortality rates for children before and during the intervention in each set of experimental villages and for the control villages. These and subsequent rates are calculated on the basis of person-years of exposure lived by each child during a particular age interval before and during the intervention. For example, a child born a few months before the intervention who survived until age 5 would contribute exposure to the ‘before-intervention’ infant mortality rate, to the ‘during-intervention’ infant mortality rate, and to the ‘during-intervention’ child mortality rate (until its third birthday or the end of observation, whichever came first). The assumption underlying this type of calculation is that the probability of dying at a given age is dependent only on whether a child is exposed to the intervention at the particular age in question, and not experience at earlier ages. Because rates are calculated by splitting exposure, the infant mortality rate in Figure 2 is per 1000 person-years of exposure (equivalent to ${}_1m_0$) rather than per 1000 live births (equivalent to ${}_1q_0$), as it is conventionally termed.

Figure 2a
Infant mortality rate before and during intervention

Source: Narangwal Study Data

Figure 2b
1–3–year–old mortality rate before and during intervention

Source: Narangwal Study Data

Figure 2c
Child mortality rate before and during intervention

Source: Narangwal Study Data

We have defined the 'before-intervention' period as 1968-70 and the 'during-intervention' period as 1971-73. Although several of the intervention programs were implemented in 1970, others were not in place until 1971. The effect of extending the 'before-intervention' period into 1970 may be to decrease the size of the observed changes in mortality between the two periods. The advantages of using a comparison period immediately prior to the intervention are that mortality rates are less likely to have declined as rapidly for reasons other than the intervention during a shorter period of time; and that retrospective reporting of births and deaths for all the villages is likely to be more accurate for the period immediately before the survey.

It is clear from Figure 2 that pre-intervention levels of mortality varied considerably among experimental groups. Thus, it is important to take into account differences in mortality rates before the intervention when evaluating differences in rates in the same villages during the intervention. The proportionate decline in all experimental groups in child mortality rates ($_3m_0$) is the same: the rates for all treatment groups during the intervention are 76-77 per cent of those before intervention. The proportionate decline in child mortality rates for the control villages is very similar: the rate during the intervention is 81 per cent of the pre-intervention rate.

For infant mortality, the rates ($_1m_0$) during the intervention are 94, 81, 104 and 101 per cent of the pre-intervention rates for the HC, NUT, HC+NUT and the control villages, respectively, indicating that infant mortality fell substantially only in the nutrition-care villages, where the pre-intervention rate was the highest. Thus, most of the decline in childhood mortality occurred for children between the ages of one and three years. In contrast to infant mortality rates, the smallest decline in 1-3-year-old rates occurred in the nutrition-care villages. The rates during the intervention were 39, 57, 35 and 49 per cent of the pre-intervention rates, respectively, for the HC, NUT, HC+NUT, and the control villages. Greater decline in infant mortality and less decline in 1–3-year-old mortality in the nutrition-service villages relative to the other villages is plausible because of the emphasis placed on breastfeeding and appropriate nutritional supplementation in the nutrition-service package.

Given the overall success of the Narangwal Project in improving the health status of children (Kielmann et al. 1983), it is surprising that the change in mortality rates was not consistently higher in the treatment villages than in the control villages.[11] An obvious explanation in this setting is that while the treatment programs may have had a greater effect on children's health than in the control villages, they had less effect on mortality rates than other factors did. It may be, for example, that the social and economic changes brought about in this region by the Green Revolution had a far greater impact on child mortality rates than the intervention programs, and that these changes affected all experimental groups more or less equally.

There are at least three other possible explanations for the apparent similarity in the rate of mortality decline in the treatment and control villages. First, during the Narangwal Project, a large number of residents in one of the control villages in the Nutrition Project found employment at a newly constructed airfield, leading to a dramatic improvement in their socio-economic status (Kielmann et al. 1983:38). A separate tabulation of the rates shown in Figure 2 for the Population Project control villages alone, however, showed results virtually identical with those described above and in Figure 2, for all control villages combined.[12]

Secondly, data on child mortality during the intervention period from the control villages, and especially the control villages in the Population Project, may not have been as complete as in the treatment villages. The reason is that children in the treatment villages were very closely monitored by project personnel, while monitoring was less complete in the control village. By contrast, all information on mortality before the intervention for all villages came from the same source: a retrospective pregnancy history that was uniformly administered in all villages. The result may be that the mortality rates for the treatment villages during the intervention were accurate, while those observed for the control villages were artificially low. Contradictory evidence is offered, however, by comparing the two control groups: the Population Project control-group villages, where monitoring of child mortality rates may have been poorer, had higher mortality rates than the Nutrition Project control-group villages, though the difference may have been due to causes other than differential reporting.

A third possible explanation is what Narangwal Project personnel called 'technical knock-outs', or TKOs (Kielmann et al. 1983:50). For ethical reasons, children in the Nutrition Project villages who were thought to be sufficiently ill that their lives or eyesight were endangered were declared TKOs and given emergency medical care. This procedure was applied in all villages, but presumably had more effect in the control villages, where children otherwise received no care from project personnel. Unfortunately, the data files contain no information with which to identify surviving children who were treated as TKOs, nor information on the numbers of children involved. Despite the TKO procedure and the decline in mortality rates during the project, many children still died during the intervention, suggesting that the number of TKOs may not have been very large. Still, it is possible that the TKO procedure accounts for the similarity in mortality rates between the control and treatment groups during the intervention.

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11 The mortality results presented by Kielmann et al. (1983: Figures 7–9 to 7–14) compared mortality rates in the treatment villages with those in the control villages during the years 1970–73 without controls for inter-group variation in mortality rates before the intervention.

12 The mortality rates before the intervention for the Population Project control villages were 121.5, 28.4 and 71.0 for infant mortality (${}_1m_0$), 1–3–year–old mortality (${}_3m_1$), and childhood mortality (${}_3m_0$) respectively. The comparable rates during the intervention (1971–73) were 128.1, 13.8 and 53.9.

Changes in sex-specific mortality rates

Next we turn to whether the interventions had differential effects on male and female mortality rates. Although the problems described above make it difficult to determine whether the service packages in the Nutrition Project actually reduced mortality rates relative to the control group, there is little reason to believe that either the data collection procedures or the TKO procedure would differentially affect children of one sex more than the other.[13]

Table 4
Child mortality rates ($_3m_0$) and male/female ratios, before and during the intervention, by experimental group

Experimental Group	Number of Villages	Before Intervention (1968–70)			During Intervention 1971–73			Change in
		Male	Female	M/F	Male	Female	M/F	M/F Ratio[b]
Health care (HC) [person-years]	2	36.3 [275.6]	57.3 [261.9]	.63	31.6 [348.2]	40.3 [373.2]	.78	1.24
Nutrition (NUT) [person-years]	3	37.4 [481.5]	76.2 [393.7]	.49	35.6 [477.3]	48.5 [433.3]	.73	1.49
Combined HC & NUT [person-years]	3	35.1 [570.0]	76.6 [509.3]	.46	38.3 [679.4]	46.5 [537.4]	.83	1.80
Control villages[a] [person-years]	6	50.0 [819.7]	79.1 [771.7]	.63	43.5 [942.7]	61.7 [809.8]	.71	1.13

[a] Includes control villages both for nutrition project (CONT-N) and population project (CONT-P).
[b] The change is calculated as (M/F Ratio During Intervention) ÷ M/F Ratio Before Intervention).
Source: Narangwal Project data

In Table 4, we show the child mortality rate ($_3m_0$) by sex for each of the experimental groups, before and during the intervention. Also shown is the ratio of male-to-female mortality rates in each period. Note that higher male/female ratios imply less excess female mortality and that in most populations outside South Asia, male/female ratios exceed 1.0. For all experimental groups the male/female ratios fall between 0.46 and 0.63 in the pre-intervention period. All ratios rise (indicating a reduction in excess female mortality) during the intervention, to levels above 0.70.

The final column in Table 4 shows the relative change in excess female mortality for each experimental group by comparing the male/female ratio during the intervention to the male/female ratio before the intervention. This index will take the value of 1.0 if there was no change in the relative mortality rates for girls and boys between the two periods. A value higher than unity indicates that mortality rates for boys and girls were becoming more nearly equal. The results in Table 4 suggest that there was a substantial reduction in all three treatment groups (HC, NUT, and HC+NUT) in the gap between male and female mortality, while there was little change in the control villages. In other words, the interventions appear to have had a greater impact on female survival chances than on those for

13 The sex ratios at birth for children in all experimental groups before and during the intervention fall within the plausible range, suggesting that the births of girls who died shortly after they were born are not systematically underreported in the control villages. It seems unlikely that the deaths of boys are more apt to be underreported than girls' deaths. If girls' deaths are systematically underreported, then the results shown in subsequent tables would be conservative estimates of the gender gap in survival chances.

males. Among the treatment groups, the biggest reduction in excess female mortality was in the combined-services (HC+NUT) group, followed by the nutritional-services (NUT) group, and the health-care (HC) group. Separate results (not shown) for infant mortality and for 1–3 year-old mortality are similar: the largest reductions in excess female mortality in both age groups occurred in the two treatment groups that included nutritional services (NUT and HC+NUT).

Despite the large number of children covered by the Narangwal intervention relative to other comparable public-health experiments, it is possible that any of the changes shown in Table 4 are due to random variation. The issue of testing whether the observed changes are statistically distinguishable from no change is complicated in this study. Neither the villages nor their inhabitants can be assumed to be a random sample of a larger population, and therefore the assumptions underlying standard statistical tests are violated. However, even if we were to assume that this is a random sample of a population, a substantially larger number of cases would be required to detect real changes, because of the fact that the variance of a difference between two ratios is large.[14] Nonetheless, we believe that the results are useful because other public-health interventions collecting data of this kind are often based on even smaller populations. Most experimental public-health interventions in developing countries cannot afford the cost of collecting high-quality data on samples of the size which would be required to determine whether observed changes in sex ratios are statistically significant.[15]

How much of the change in excess female mortality shown in the last column of Table 4 can be attributed to the intervention? If taken at face value, the decline in the control villages suggests that part of the mortality decline for both sexes would have taken place in the absence of the intervention. The likely change in excess female mortality in the treatment villages in the absence of the intervention is more difficult to gauge because while the decline in female mortality was greater in the treatment than in the control villages as would be expected from the last column of Table 4, the decline in male mortality in the treatment villages was generally somewhat less than in the control villages.[16] If we assume that male and female mortality rates would have declined in the treatment villages between 1968-70 and 1971-73 by the same proportion as they did in the control villages, had there been no intervention, we can estimate the effects of the intervention itself on excess female mortality net of the decline due to factors other than the intervention.

Table 5
Estimation of the effect of intervention on excess female mortality net of the effects of other factors

Experimental group	'Expected' mortality rates	Actual rates[b]	'Net' rates[c]	'Net' M/F	Change in M/F

[14] For example, in the HC+NUT villages, where the change in the male/female ratio was greatest and thus most likely to be statistically significant, the standard deviation of the difference between the two ratios (i.e., 0.46 - 0.83 = -0.37) is approximately 0.29. Therefore, the 95 per cent confidence interval for the difference equals -0.37 plus or minus 0.58, a large interval that includes zero. Thus, none of the changes shown in Table 4 would be statistically significant using standard tests, and these results should therefore be interpreted with caution.

[15] ICDDR,B's study in Matlab Thana is a very rare exception. In a recent study using these data, Koenig et al. (1991) investigate the consequences of an immunization program on infant and child mortality, although they do not look at this impact on sex differentials in mortality. From the results of their study, they conclude that 'while immunization programs will significantly reduce mortality during ages 1–4 years, their ability to influence mortality during the initial months of life is limited' (1991:200).

[16] Male mortality rates during the intervention were 87, 95, 109 and 87 per cent of the pre-intervention rates for the HC, NUT, combined HC & NUT, and control villages. Comparable figures for female mortality rates are 70, 64, 61 and 78 per cent.

factors	1971–73 without intervention[a]		minus 'expected' rates		during intervention (1971–73)		ratio	ratio net of other
	Male	**Female**	**Male**	**Female**	**Male**	**Female**		
Health care (HC)	31.6	44.7	0	-4.4	36.3	52.9	.69	1.08
Nutrition (NUT)	32.5	59.4	3.1	-10.9	40.5	65.3	.62	1.26
Combined HC & NUT	30.5	59.7	7.8	-13.2	42.9	63.4	.68	1.48
Control villages	43.5	61.7	0	0	50.0	79.1	.63	1.00

[a] Calculated by multiplying the intervention-period mortality rate as a proportion of the pre-intervention rate for the control villages (i.e., 0.87 for males and 0.78 for females) by the pre-intervention rate for each treatment group, to estimate how much change would have occurred in the absence of the intervention.

[b] These are the actual mortality rate minus the 'expected' rate for each treatment group and sex during the intervention. It represents the estimated change in mortality rates attributable to the intervention alone.

[c] These are the sum of the before-intervention mortality rate and the estimated change attributable to the intervention. They represent the estimated mortality rates which would have occurred in each treatment group solely due to the intervention.

Source: All figures in this table are derived from Table 4.

The results of this calculation are shown in Table 5. First, 'expected' mortality rates for the intervention period are calculated by multiplying the pre-intervention rate for each sex and treatment group by the amount of change for that sex in the control group. These expected rates represent the mortality decline which would have been expected from non-intervention-related causes, based on the experience of the control group. Secondly, expected rates are subtracted from actual rates, to produce the amount of decline attributable to the intervention itself, based on the assumption that the experience of the control group represents how much change would occur without the intervention. Thirdly, rates during the intervention attributable to the intervention and net of change due to other factors are calculated by adding the change in mortality attributable to the intervention to the pre-intervention rates. Finally, these 'net' rates for the intervention period are used to calculate a net male/female ratio for the intervention period, and a net change in the male/female ratio shown in the last column of Table 5. This net change represents the estimated change in excess female mortality for each treatment group which is attributable to the intervention under the assumptions described above. A comparison of the last column of Tables 4 and 5 shows that the estimated change in excess female mortality net of non-intervention factors is considerably smaller than the actual change shown in Table 4. However, the amount of change remains substantial in the combined health and nutrition group, and to a lesser degree, in the nutrition group.[17]

Discussion

Our results show that while child (0–3-year-old) mortality declined roughly equally in the control and treatment villages – with the possible exception of the nutrition-service villages – the level of excess female mortality appears to have been reduced in the treatment groups, especially those including nutritional services, while remaining roughly constant in the control villages. Furthermore, while the female mortality decline in the treatment villages was greater than in the control villages, the decline in male mortality rates in the treatment villages was either the same as or less than that in the control villages. There are at least two possible explanations for these results. First, for reasons described above, the apparent mortality decline in the control villages may have been exaggerated by poorer

[17] It is important to note that this type of decomposition ignores possible interactions between the effects of the intervention and non-intervention-related causes of the mortality decline.

registration of deaths in these villages than in the treatment villages during the intervention. While this hypothesis is certainly plausible, given the manner of data collection, it is difficult to prove or disprove with the available data. Secondly, the substantial social changes taking place during the intervention period may have swamped improvements in mortality which might have been brought about by the intervention itself, particularly for boys who may have received more of the newly available household resources. Girls, on the other hand, while also benefiting from the general improvement in living standards, still had higher mortality rates than boys. They may have benefited from the intervention to a greater degree both because they were more likely to die to begin with and because the health services were generally provided more impartially to boys and girls than were other family resources.

Given the strong evidence of preferential treatment for sons in Punjab, we expected that health-service packages which relied less on the family for implementation would be more successful in reducing excess female mortality in childhood. For example, we conjectured that the immunization services included in the health-care service package would reduce the incidence of immunizable infectious diseases equally for children of both sexes since health workers would ensure that all children in the treatment villages were vaccinated, and that girls would gain more from reductions in incidence since they would have been less likely to receive treatment had they become ill. We thought that nutrition services would have had less effect because parents would have to ensure that malnourished children attended the feeding centres, thereby requiring an investment of parents' time.

Initially, the finding that the two service packages including nutritional services appear to have been more successful in reducing excess female mortality seemed to contradict these hypotheses. Additional investigation, however, suggests that the reason for this finding is the careful follow-up by project fieldworkers of children showing signs of malnutrition in the nutrition-surveillance system. Kielmann et al. (1983:47–8) state:

> When children dropped below 70 percent of the Harvard median weight-for-height standard, a special effort was made to encourage their regular attendance at the feeding station. This was sometimes difficult to achieve because most of the malnourished children came from the lower socioeconomic class and their families found it difficult to bring them.

Under these circumstances, given the experience in Matlab, Bangladesh (Chen et al. 1981), we might expect that families would be more likely to spend the time to bring sons than daughters with growth deficits to the feeding centres. However, in the Narangwal Project, if children showing weight deficits did not attend the supplementation centre, health workers would bring the nutritional supplement to the child's home, and supervise feeding until the child reached an appropriate weight (Kielmann et al. 1983:48). By contrast, much of the emphasis in the health-care villages was on curative care provided in a village clinic, to which parents or other family members had to bring a sick child for treatment.

The mortality experience of younger siblings

If public-health programs have the potential to reduce excess female mortality in areas like the Ludhiana District by ensuring that girls receive more equal access to health services, should they be targeted toward all girls, or might they achieve the same outcome by focusing on particular groups of girls who are at especially high risk of poor health and death?

The results of Das Gupta's (1987) study suggest, at least for the Ludhiana District, that a particularly important high-risk group is younger girls who have at least one surviving older sister, since much of the excess female mortality occurs among this group. As discussed in the introduction, Das Gupta (1987) argued that parents in the Punjab might use preferential treatment for boys as a means of affecting the sex composition of their offspring. As evidence, she showed that in the Khanna villages in the Ludhiana District in the early 1980s, daughters with surviving older sisters suffered higher mortality rates than daughters with no surviving older sisters. By contrast, the number of surviving older brothers did not appear to affect survival chances for sons or daughters.

Replication of Das Gupta's table for the Narangwal villages gives very similar results: among second- and higher-order children, girls with one surviving older sister have 1.2 times the risk of dying of girls with no surviving older sister. The risk is multiplied by 1.5 for girls with two or more surviving older sisters. In this section we examine Das Gupta's hypothesis in greater detail by looking at the effects of sex composition and survival status of all children born before the child in question (the 'index child') and by controlling for socio-economic factors that may have affected both fertility decisions and child survival chances. The question we seek to answer is whether younger sisters actually suffer more discriminatory treatment than their older sisters and brothers, or whether there are other aspects of family composition or socio-economic status which account for the association between being a younger sister and higher mortality risks. For example, mortality rates may appear to be higher for girls with one or more surviving older sisters because higher-order children are generally at greater risk of dying, or because poor families are both more likely to have more children and to experience more child mortality, rather than because girls with surviving older sisters receive less care from their families.

To examine the effects of the sex and survival status of siblings born before the index child's birth, we have combined records for children from all experimental groups included in the Narangwal Nutrition Project plus the Population Project control villages and included in the analysis a variable to control for year of birth. The results presented in Table 6 show the relative risk of dying associated with having a larger number of prior siblings, depending on the sex and survival status of these siblings. Results are shown separately for girls and boys, and for neonates, post-neonates and 1-3-year-old children. The figures are based on a multivariate analysis in which all variables shown in Table 6, plus birth year and caste, are included.[18] Thus, they reflect the effect of a change in one of the variables holding constant the others. For example, the 0.90 for girls in the neonatal period associated with an increase in the number of surviving older sisters indicates that having an additional surviving older sister multiplies the risk of dying by approximately 0.90, that is, reduces it by 10 per cent, *ceteris paribus*. Relative risks above 1 indicate an increased risk of dying with an increase in the variable listed on the left side of the table, and relative risks below 1 indicate a decreased risk.

Table 6

Net change in the risk of dying[a] associated with an increase in the number of older sisters and brothers, by survival status of the older sibling and sex of the index child (controlling for caste and year of birth)

	Age Group		
	Neonates (0–1 month)	**Post-neonates (1–12 months)**	**Childhood (12–36 months)**
		Girls (N=2340)	
Increase in the number surviving			
Older sisters	.90	1.26[b]	1.22[b]

[18] The results are from a hazard model with three age intervals as shown, in which effects are allowed to vary among age intervals. The figures shown are relative risks or exponentiated coefficients. Also included in the model were caste and birth year. Other models including the educational attainment of the child's mother, the occupational status of the father and the family's landholdings were also estimated for a smaller sample for which this information was available. Results were not substantially different from those shown in Table 5.

Older brothers	.81	.93	1.05
Increase in the number who died			
Older sisters	1.70[b]	1.37[b]	1.37[b]
Older brothers	1.60[b]	.87	.97
		Boys (N=2660)	
Increase in the number surviving			
Older sisters	1.11	.92	.97
Older brothers	.55[b]	.87	1.37[b]
Increase in the number who died			
Older sisters	1.33	1.05	1.47
Older brothers	1.51[b]	1.81[b]	.63

[a] The figures presented in this table are relative risks of dying. They are calculated from exponentiated coefficients in a hazard model. Included in each equation were the sibling variables shown above, plus caste and year of birth.
[b] Difference is statistically significantly different from zero in a two-tailed t-test with a probability >.05.
Source: Narangwal Project data

We would expect, *a priori*, that the number of older siblings (regardless of sex) who died would adversely affect a child's survival chances, because mortality risks for children within a family are likely to be correlated. Reasons for this intrafamily correlation may include interfamily differences in economic status, child-care practices, and genetic endowment. There is also some evidence that higher-order children are at greater risk of dying than those at lower birth orders, possibly because of greater competition among children in large families for scarce resources, because poorer families tend to have more children, or because of physiological stress placed on the mother by giving birth to a larger number of children (NAS–NRC 1989). Thus, we expect to see increases in mortality risks for children who have had more older siblings (regardless of sex) die, even when birth order is held constant.

The results in the top panel of Table 6 show that having a larger number of surviving older sisters substantially[19] increases the risk of dying for girls after the first month of life. However, girls with more surviving older brothers do not have significantly higher risks of dying. This result suggests that the effect of having more surviving older sisters is not merely due to increased competition among children for family resources, since younger daughters would presumably be as much in competition with older brothers as with older sisters.

In the neonatal period, girls who have had older siblings of either sex die have higher risks of dying themselves, most likely because of intrafamily correlation in mortality risks. After the neonatal period, however, only the number of previously born sisters who have died is associated with an elevated risk of dying for girls. This finding is contrary to our expectation that the death of any older siblings (regardless of sex) would be related to higher mortality risks for the index child.

The results for boys, shown in the bottom panel of Table 6, indicate that the number of surviving older sisters is not significantly related to risk of dying for boys. The fact of having more surviving older brothers significantly reduces the chances of dying for the index son in the neonatal period, and has no effect in the post-neonatal period, but significantly increases the risk for 1-3-year-olds. Thus, for 1-3-year-olds of both sexes, having more older surviving same-sex siblings appears to increase the risk of dying significantly, while having more surviving siblings of the opposite sex does not seem to have any effect.

Boys who had a larger number of older brothers who died were significantly more likely to die in the first year of life. Their survival chances in the 1-3-year-old age group were not significantly

[19] As discussed earlier in the text, the assumptions underlying tests for statistical significance are not strictly met in this case because the Narangwal Study is not based on a random sample from a population.

affected, however, by the death of older brothers. Although the effects of having a larger number of older sisters who died are not statistically significant, they are still substantially above 1.0 for the neonatal and 1-3-year-old age group.

Discussion

The figures in Table 6 support Das Gupta's hypothesis that in the Ludhiana District, girls with older surviving sisters have higher mortality risks than other daughters, even when other aspects of family composition and socio-economic factors that may affect both family size and mortality risks are held constant. This effect is unlikely to reflect exclusively either birth order *per se* or the number of surviving older siblings competing with the index child for family resources, because the number of a girl's older brothers, regardless of their survival status, has no effect on her survival chances, after the first month of life. Consistent with our expectation, the number of surviving older sisters affects girls' survival chances only after the first month of life, since preferential treatment for children by sex is less likely to affect neonatal mortality rates.

While the results for girls are as expected, there are also some surprises in Table 6. First, although boys' survival chances are substantially improved in the neonatal period by having older surviving brothers (probably because of lower overall family health risks), the survival chances for 1-3-year-old boys are negatively affected by having surviving older brothers, though not by having surviving older sisters. Secondly, except in the case of neonatal mortality for girls, only the mortality experience of *same-sex* older siblings is significantly related to the risk of dying for the index child. Thus, as is the case for girls, boys' survival chances in this population appear to be affected by the number of older surviving and non-surviving same-sex siblings. This finding suggests that the system of preferential treatment for children in Punjabi families may be more complex than Das Gupta implies. While boys may generally receive preferential treatment relative to girls, children of both sexes who have fewer surviving same-sex siblings may also receive preferential treatment. Further investigation of food allocation, use of health services, and other aspects of child care within Punjabi households will be necessary to determine whether boys and girls are, in fact, treated differently depending on the number of older brothers and sisters. However, these results and those of Das Gupta's study suggest that in Northern India, younger children with older same-sex siblings may be a high-risk group requiring greater attention from public-health programs.

Excess female mortality among younger sisters and the Narangwal program

Finally, we return to the Narangwal Project to determine whether the apparent improvement in excess female mortality in the intervention groups receiving nutrition services was greater among girls with surviving older sisters than among other girls. If, as Das Gupta's and our results suggest, girls with surviving older sisters are more likely to experience discriminatory treatment in the allocation of family resources, they may be more likely to have benefited from the more equitable distribution of nutrition services apparently effected by the program. To look at the effect of having a surviving older sister, we pooled the cases for the two experimental groups in which the apparent decline in excess female mortality was the largest, that is, nutrition villages and the combined nutrition and health-care villages, to increase the amount of exposure on which the rates were based. We calculated the ratios of male-to-female mortality rates for the periods before and during the intervention, and then calculated the change in these ratios between the two periods, as done in Table 4, in this case according to whether or not children had an older surviving sister.

The results show that for children with no surviving older sister the ratio of male-to-female 0–3-year-old mortality rates went from 0.56 to 0.83. Thus, the change in the excess female mortality ratio for children with no surviving older siblings is 1.48.[20] For children with one or more surviving older siblings the male/female ratio was 0.32 in the pre-intervention period and 0.64 during the intervention, for a change in the excess female mortality ratio of 2.00. As in Table 4, the ratio of excess female mortality during the intervention relative to the pre-intervention period would equal 1 if there had been no change, would be less than one if excess female mortality had increased, and would be greater than one if excess female mortality declined.[21] The fact that the change in excess female mortality was greater for children with one or more surviving older sisters than for those with no surviving older sisters suggests that the nutrition intervention may have had the greatest effect on more vulnerable girls within each family, as anticipated.[22]

Conclusions

In this paper we have examined the effects of a public-health intervention program on sex differentials in mortality during childhood, and investigated the hypothesis that public-health programs might have a greater impact on excess female mortality by focusing on daughters with one or more surviving older sisters. Assessment of the impact of the Narangwal Project on infant and child mortality is complicated by the fact that the Ludhiana District was undergoing rapid social and economic change at the time of the study, and by possible differences in data-collection procedures among the experimental groups. Child mortality rates in the groups receiving health services appear to have declined about as much as rates in the control groups, relative to pre-intervention levels in each experimental group. By contrast, the health-service packages, especially those including nutritional services, appear to have had a substantial effect on excess female mortality in childhood: the ratio of male-to-female mortality rates declined in the nutritional services (NUT and HC+NUT) groups, but remained essentially constant in the control groups.

If the nutritional services improved the health of girls more than boys during the intervention, this result reinforces the notion that preferential treatment for boys in the pre-intervention period may have taken the form, at least in part, of greater attention to the diet and growth of sons. Based on our understanding of program-implementation procedures during the Narangwal Project, we conjecture that at least part of the reason for the apparent success of the nutritional-service packages in reducing excess female mortality is that project health workers essentially overrode the family allocation system in the case of measurably malnourished children who did not regularly attend the feeding centre. An alternative hypothesis is that the project's focus on the health of all children within a family changed families' attitudes toward preferential treatment for sons. However, it seems unlikely to us that the brief duration of the Narangwal Project, by itself, would bring about a sufficient change in traditional family

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[20] As in Table 4, this ratio is the during-interention male/female ratio divided by the before-intervention male/female ratio. In this case, it is 0.83 divided by 0.56 or 1.48.

[21] As in Table 4, the amount of exposure is sufficiently small and the width of confidence intervals around ratios is sufficiently broad to make it impossible to dismiss the possibility that these results are due to sampling variability (on the assumption that this is a sample).

[22] Similar calculations were carried out for children in the control groups, even though excess female mortality appeared to decline very slightly, if at all, in these villages. The results show that mortality rates for girls without any surviving older sisters were actually lower than those for boys in these villages both before and during the intervention. For this group, the ratio of male-to-female mortality changed very little between the two time periods. For children with surviving older sisters female mortality exceeded that for males, for both time periods, and excess female mortality appears to have increased during the intervention period. Rates and ratios were also calculated for the two health-care villages, but the small number of cases makes the results difficult to interpret and they are not reported here.

attitudes toward preferential treatment for sons to explain the observed decline in excess female mortality.

We also investigated the hypothesis, based on earlier work by Das Gupta, that families discriminate most against daughters who have at least one surviving older sister, and that these girls might benefit most from public-health intervention offering services on a more equitable basis. Our results indicate that, even when other aspects of family composition and socio-economic status are held constant, girls with surviving older sisters do have higher mortality rates, after their first month of life. There is also some evidence from the villages where excess female mortality appeared to have declined during the intervention (i.e., the NUT and HC+NUT villages) that excess female mortality declined more for girls with surviving older sisters than for other girls.

However, our results also show that boys with surviving older brothers have higher mortality rates, at least between the ages of 1 and 3 years. These effects for boys and girls cannot be attributed to problems associated with larger family size, since the number of older siblings of the opposite sex (regardless of survival status) does not generally appear to be related to children’s chances of survival. Instead, we conclude that in areas like Punjab, preferential treatment for some children at the expense of others may be more complex than suggested by Das Gupta.

Appendix Table 1

Causes of death for sample of deaths investigated in the Khanna Study and the Narangwal Study

	Khanna Study[a] (1957–59)			
	0–11 Months		1–4 Years	
Cause	Number	Per cent	Number	Per cent
• Diseases peculiar to infancy; prematurity (B44)	43	19.6	0	0
• Birth injury; post-neonatal asphyxia; atelectasis (B42)	29	13.2	0	0
• Infections of newborns (B43)	6	2.7	0	0
• Tetanus (B17–061)	30	13.7	0	0
• Congential malformations (B41)	5	2.3	0	0
• Diarrhoea (B6/B36)	39	17.8	28	24.6
• Pneumonia (B31)	15	6.8	9	7.9
• Measles (B14)	8	3.7	10	8.8
• Typhoid fever (B4)	4	1.8	10	8.8
• Tuberculosis (B1-2)	3	1.4	8	7.0
• Whooping cough (B9)	0	0	6	5.3
• Accidents (B48)	6	2.7	0	0
• Unknown (B45)	14	6.4	17	14.9
• Other residual (B46)	8	3.7	11	9.6
• Other known causes	9	4.1	21	18.4
Total	219	100.0	120	100.0

	Narangwal Study[b] (1970–73)			
	0–7 Days		8 Days to 5 Years (Primary Cause)	
	Number	Per cent	Number	Per cent
• Prematurity	40	32.2	7	6.0
• Intrauterine asphyxia	24	19.4	0	0
• Birth trauma	22	17.7	0	0
• Tetanus neonatorum	4	3.2	3	2.6
• Other neonatal infections	3	2.4	0	0
• Congenital anomalies	8	6.5	3	2.6
• Diarrhoea, gastroenteritis	0	0	0	0
• Lower respiratory infection	0	0	20	17.1
• Other gastrointestinal	0	0	4	3.4
• Malnutrition	0	0	8	6.8
• Septicaemia	0	0	5	4.3
• Accident/violence	0	0	4	3.4
• Other/miscellaneous	8	6.5	8	6.8
• Unknown	15	12.1	12	10.3
Total	124	100.0	117	100.0

[a] From Gordon et al. (1965): Tables I and II. Deaths were investigated at the time of occurrence by a qualified physician. ‘All diagnoses were by clinical and epidemiological methods; laboratory aid was rarely available and the study included no necropsies’ (p.1). The numbers in parentheses refer to the abbreviated list of the International Statistical Classification of Diseases, Injuries and Causes of Death (WHO 1957).

[b] From Kielmann et al. (1983): Tables 7–3 and 7–4. Diagnoses were made from ‘verbal autopsies’ by a consensus of staff physicians. A diagnosis of malnutrition was assigned when no other cause was apparent and the child was less than 70 per cent of weight-for age.

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