The Potential Impact of Changes in Fertility on Infant, Child, and Maternal Mortality

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In this paper we explore the relation between changes in reproductive behavior, such as those that might result from an effective family planning program in developing countries, and changes in child and maternal mortality. Specifically, we use the results from recent multivariate studies to estimate the changes in mortality that might result from altering maternal age, birth order, and birth spacing distributions of live births. Our results indicate that if childbearing were confined to the "prime" reproductive ages of 20–34, then infant and child mortality rates would fall by about 5 percent. Limiting childbearing to ages 20–39 may also reduce the maternal mortality ratio by 11 percent. The elimination of fourth and higher order births would reduce infant and child mortality by about 8 percent, and the elimination of fifth and higher order births would reduce the maternal mortality ratio by about 4 percent. Universal adoption of an "ideal" spacing pattern in which all births subsequent to the first are spaced at least two years apart may reduce infant mortality by about 10 percent and child mortality by about 21 percent.

Certain aspects of reproductive behavior, such as the maternal age and parity at the time a child is born and birth spacing, have long been thought to affect both a mother's and a child's chance of survival.[1] During the transition from high to low fertility, as contraceptive use increases, these variables change in ways that may alter infant, child and maternal mortality rates. For example, the proportion of live births occurring to women at high parities and older ages is likely to decline significantly. Although reduction in maternal and child mortality is frequently one of the stated objectives of family planning programs in developing countries, there are relatively few estimates of the potential size of these reductions.

In this paper, we explore the relation between changes in reproductive behavior and fertility, such as those that may result from an effective family planning program in developing countries, and changes in infant, child, and maternal mortality. First, we outline ways in which reproductive behavior may influence child and maternal mortality in a population. Next, we use results from recent studies to estimate the changes in mortality that might result from hypothetical alterations in maternal age, birth order, and birth spacing distributions of live births. Finally, we discuss the problem of predicting how these distributions would actually change as the result of implementing effective family planning programs or, more specifically, increasing contraceptive use in populations of developing countries.

We consider three ways in which the successful implementation of a family planning program may affect infant and child mortality: (1) by changing the ages at which women have children, (2) by reducing the proportion of births that occur at high parities, and (3) by lengthening the interval between births. Previous research suggests that all three changes would reduce infant and child mortality.[2]

When examined one at a time, both maternal age at time of birth and a child's birth order are consistently found to be related to the risk of infant and child mortality, usually in a J-shaped pattern (Nortman, 1974; Omram, 1976 and 1981). That is, the risk is moderately high for infants of very young mothers and for first births, declines for infants of mothers in their twenties and of birth orders two and three, and then rises with further increases in maternal age and birth order. Maternal age seems to be related to the risk of child mortality even when parity is controlled, and vice versa (parity seems to be related to mortality even when maternal age is controlled); there is, moreover, likely to be an important interaction between maternal age and parity.

Nortman (1974, p. 4) argues that the basic curvilinear relationship between infant mortality, birth order, and

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maternal age results from underlying biological factors in the mother. Very young women's reproductive systems, for example, may not be adequately prepared for the stress of a pregnancy, while advanced aging seems to reduce the efficiency of the entire reproductive process. Infants in higher birth orders may be at greater risk because of general exhaustion of a woman's reproductive system, whereas a first birth may be riskier because the body is undergoing parturition for the first time.

Observed maternal age and birth order differentials in infant and child mortality may be exaggerated, however, for at least two reasons. First, socioeconomic status may be a confounding factor. Poorer women, who experience higher child mortality, contribute disproportionate shares of higher order births and births at very early and late maternal ages. Second, the effect of high parity, or of high age when parity is not controlled, may actually be a reflection of the harmful impact on child survival of closely spaced births. Hence, it is essential to control for socioeconomic correlates of child mortality and for birth spacing patterns when trying to isolate the real effects of maternal age and parity.[3]

Most discussions about the ways in which close spacing of births may contribute to higher child mortality center around two principal possibilities. One is that competition for limited maternal and familial resources among closely spaced children (Omram, 1981) may jeopardize the health of both the older and younger sibling. For example, the beginning of another pregnancy shortly after a previous birth may result in early weaning of the first child. Similarly, the younger child may also receive less adult attention because of competition from a slightly older sibling with similar needs. The second hypothesis suggests that a mother's nutritional status is eroded by a rapid sequence of pregnancies and periods of lactation (Jelliffe and Jelliffe, 1978). Poor maternal nutritional status increases the risk of premature births and low birth weight (small-for-date) babies, who have a poorer chance of survival (Fedrick and Adelstein, 1973). Malnourished mothers may also be less successful at breastfeeding their chiidren either because of diminished quantity or quality of breastmilk or because low birth weight infants have a harder time breastfeeding. Hence, taken together, the two mechanisms suggest that close spacing of two births may result in poorer survival chances for both children. Therefore, we might expect that lengthening birth intervals would increase the overall likelihood of infant and child survival.

Increased use of contraception may also lower the risk of the maternal mortality *ratio*[4] (maternal death per birth) by: (1) reducing the proportion of births occurring to youngest and oldest women, (2) reducing the proportion of births at high parity, and (3) lengthening birth intervals. As a consequence of these changes, the maternal mortality *rate* is further lowered because childbearing is less frequent.[5] As in the case of infant and child mortality, there is evidence that the risk of maternal mortality is higher for women under age 20 and over age 35, and for women at parity 0 and very high parities (see, for example, Nortman, 1974 and Tietze, 1977). By contrast, little is known about the effect of birth spacing on maternal mortality, although it is likely that maternal depletion (if it exists) would increase the risk of maternal mortality among women with very short birth intervals. Another effect of increased contraceptive use on maternal mortality would be a reduction in the overall level of fertility. Thus, a smaller fraction of women would be exposed to the risk of maternal mortality in any given year. In summary, increased contraceptive use is likely to result in lower maternal mortality because of a re duction both in the number of deaths per number of births and in the number of births.

How large an impact could changes in fertility resulting from a family planning program have on infant, child, and maternal mortality? In the next section we describe recent studies that explore the relation between fertility variables and mortality. We then employ results from these studies to estimate the magnitude of changes in mortality that might be expected from different types of fertility changes.

Sources

We draw on the results of five studies to assess the potential impact of increased contraceptive use on infant and child mortality. The first two, by Martin et al. (1983) and Trussell and Hammerslough (1983), employ hazard models to estimate the effects of several covariates, including maternal age and parity, on infant and child mortality in Indonesia, Pakistan, the Philippines, and Sri Lanka. Both studies find that these two variables have significant effects on the risk of dying. In the third study, Holland (1983) also uses hazard models to assess the effect of breastfeeding on infant mortality in Malaysia. To avoid confounding his estimates, he includes several other covariates as controls, including the length of the prior birth interval. This variable is constructed by combining it with birth order so that first births can be included in the analysis.[6]

A fourth study by Hobcraft et al. (1983) focuses specifically on the effects of birth spacing. The investigators are interested in estimating the relationship between mortality of one child, designated as an "index" child, and the number of other births that occurred within certain time periods before and after that child's birth. The periods the investigators select are: 2–6 years before the birth of the index child, 0–2 years before, and 0–17 months (for ${}_1q_1$) or 0–29 months (for ${}_3q_2$) after the birth of the index child.[7] By separating the effects of prior birth intervals from the effects of subsequent ones, they hoped to determine whether short child spacing is harmful because of maternal depletion or because of sibling competition for resources. Since they find a negative association between survival probabilities of the index child and closely spaced subsequent siblings, they conclude that the competition effect in this case is strong since the principal impact of maternal depletion syndrome is thought to operate through a short *prior* interval. They also find that prior close spacing has a substantial negative impact on survival probabilities, but they are unable to distinguish possible competition effects from the possible maternal depletion syndrome. Since the study is a cross-country analysis involving 25 countries, only one control variable (mother's education) was included, to reduce costs.

Finally, Cleland and Sathar (1983) use a sample of births occurring between one and fifteen years prior to the Pakistan Fertility Survey to explore the relation between birth spacing and infant and child mortality in greater depth. They calculate infant (${}_1q_0$), early childhood (${}_1q_1$), and later childhood (${}_3q_2$) mortality rates by the length of the preceding birth interval and by whether or not the previous child survived until his or her second birthday. Their results indicate that a strong positive relation between the length of the preceding interval and child survival generally persists even when the survivorship of previous child, maternal education, rural–urban residence, sex of the child, maternal age, and parity are held constant.[8] One possible explanation for this association might be that women breastfeed all children for similar lengths of time; those women who lactate for short periods, therefore, would have short interbirth intervals and also higher infant and child mortality. If so, then the association between interval length and mortality would be spurious. Cleland and Sathar conclude, however, that early weaning of the index child cannot, in general, explain the association. They also examine the effect of *subsequent* interval length on child mortality and find that short subsequent intervals have a negative effect on child survival, principally because of earlier weaning. They conclude that maternal depletion is a more plausible explanation for the relation between child spacing and infant and child mortality since this association is not diluted by inclusion of information on the survivorship of the previous child.

All the analysts except Holland use World Fertility Survey (WFS) data. Holland's results come from the Malaysian Family Life Survey (MFLS), conducted by the Malaysian Department of Statistics, Survey Malaysia, and the Rand Corporation. The MFLS and WFS data sets that have been evaluated have been shown to be of high quality. The variables included in each study for each country are shown in Table 1. This table also includes estimates of ${}_1q_0$ and ${}_4q_1$ for each population from Hobcraft et al. (1983).

Information on the levels, trends, and correlates of maternal mortality in developing countries is far more rare, and measurement is much more difficult, than for infant and child mortality. Part of the reason is that reproductive complications are only one of the many causes of death among women of reproductive age. Thus, an accurate classification of deaths by cause is required to determine the level of maternal mortality. Even in the United States, maternal mortality is believed to be underestimated by at least 25 percent (Sachs et al., 1982). Furthermore, even in high mortality and fertility settings, maternal mortality is a rare occurrence. For example, Chen et al. (1977) report a high maternal mortality ratio (maternal deaths per 1,000 live births) for Matlab Thana,

Table 1 Analyses employed in assessing the effect of family planning on infant and child mortality

Country	Factors in model[e]	Birth cohorts	Reference ${}_1q_0$[f]	Reference ${}_4q_1$[f]
Africa				
Senegal	M,PB,SB	1963–73	120	151
Lesotho	M,PB,SB	1962–72	132	054
Kenya	M,PB,SB	1962–73	095	064
Sudan	M,PB,SB	1963–74	076	063
Ghana	M,PB,SB	1964–75	075	058
Americas				
Haiti	M,PB,SB	1962–72	147	082
Peru	M,PB,SB	1962–73	107	064
Ecuador	M,PB,SB	1964–74	085	052
Mexico	M,PB,SB	1961–72	075	036
Colombia	M,PB,SB	1961–71	068	042
Costa Rica	M,PB,SB	1961–71	071	019
Guyana	M,PB,SB	1960–70	055	014
Panama	M,PB,SB	1960–71	044	018
Jamaica	M,PB,SB	1960–71	041	014
Asia				
Nepal	M,PB,SB	1961–71	168	096
Bangladesh	M,PB,SB	1960–71	136	080
Pakistan	M,PB,SB	1960–70	130	077
Pakistan[a]	M,F,TP,A,S, BO,R,U	ca. 1941–75	130	077
Pakistan[b]	M,A,PB,SB, S,BO,U,BF,SS	ca. 1960–75	130	077
Indonesia	M,PB,SB	1961–71	106	081
Indonesia[a]	M,F,TP,A,S, BO,R,U	ca. 1972–76	106	081
Thailand	M,PB,SB	1960–70	079	034
Jordan	M,PB,SB	1961–71	069	026
Syria	M,PB,SB	1963–73	067	025
Philippines	M,PB,SB	1963–73	055	012
Philippines[a]	M,F,TP,A,W,L, S,BO,T,R,U	ca. 1944–78	055	012
Sri Lanka	M,PB,SB	1960–70	055	024
Sri Lanka[c]	M,F,TP,A,W,S, BO,T,E,U	ca. 1941–75	055	024
Korea	M,PB,SB	1959–69	053	021
Malaysia	M,PB,SB	1959–69	043	009
Malaysia[d]	BF,M,BW,BO, PB,TP,S,E,W,A	ca. 1942–76	043	009

Note: Countries are listed by region, and within region by descending value of ${}_5q_0$ (not shown).
[a] Martin et al., 1983.
[b] Cleland and Sathar, 1983.
[c] Trussell and Hammerslough, 1983.
[d] Holland, 1983.
[e] Factors: M—mother's education, F—father's education, TP—time period of birth, A—age of mother, PB—previous birth interval, SB—subsequent birth interval, W—water supply, L—lighting source, S—sex, BO—birth order, T—toilet facilities, R—region, E—ethnicity, U—urban-rural, BF—breast-feeding, BW—birth weight, SS—survival status of previous child.
[f] Reference ${}_1q_0$ and ${}_4q_1$: pertains to the birth cohorts for each country listed for Hobcraft et al., 1983. Values taken from Hobcraft et al. (1983), Table 1.
Sources: For all countries except as indicated in footnotes a–d, data are from Hobcraft et al., 1983.

Bangladesh of 5.7 deaths per 1,000 live births. By contrast, the infant mortality rate in Matlab for the same period was 124 deaths per 1,000 live births. The maternal mortality rate was 0.9 maternal deaths per 1,000 women-years of exposure between the ages of 15 and 49, indicating that the probability of a woman dying from a maternity-related cause was small indeed.

The majority of studies of maternal mortality in developing countries concern the experience of women who enter a maternity hospital for delivery and/or treatment of pregnancy complications. One example is given by Chi et al. (1981) using data from admissions to 12 teaching hospitals in Indonesia. The obvious problem with generalizing from such results, particularly in a poor country, is that only a select sample of women deliver in a hospital and, furthermore, that women who are suffering serious complications are more likely to be hospitalized.

By contrast, Chen et al. (1974) were able to follow an entire population of women in Matlab Thana, Bangladesh. Their results by age and parity are shown in Table 2. To our knowledge, this is the only population-based study available that reports rates cross-classified by age and parity.

Table 2 Maternal mortality ratio per 1,000 live births by maternal age and maternal parity, Matlab Thana, Bangladesh, 1968–1970 study

	Parity				
Maternal age (years)	**0–1**	**2–3**	**4–5**	**6+**	**All parities**
10–19	8.8	3.1	45.4	0.0	8.6
	(4,069)	(323)	(22)	(2)	(4,416)
20–29	4.5	3.6	5.8	4.5	4.5
	(2,449)	(4,439)	(3,072)	(1,326)	(11,286)
30–39	12.8	2.5	2.5	7.4	5.8
	(78)	(396)	(1,212)	(2,981)	(4,667)
40–49	0.0	0.0	0.0	8.0	6.7
	(7)	(13)	(54)	(373)	(447)
All ages	7.2	3.5	5.0	6.6	5.7
	(6,603)	(5,171)	(4,360)	(4,682)	(20,816)

Note: Figures in parentheses are the numbers of live births.
Source: Adapted from Chen et al., 1977, page 338.

Impact on Mortality of Changing Maternal Age, Parity, and Birth Spacing Patterns

Infant and Child Mortality

To estimate the effect on mortality of changes in fertility, we must assume that maternal age, parity, and birth spacing are causally related to infant and child mortality when the effects of potentially confounding variables are controlled, as in the studies described above. We can then use the results of statistical models reported in these studies to predict how large a mortality change would result from a specific fertility pattern change. The methods employed to quantify the impact of a change in the fertility variables are briefly outlined below and are described fully in the Appendix. *The assumptions described in the Appendix are essential to a full understanding of the results.*

Our calculations are based on parameter estimates and sample characteristics reported in the studies described above. In each study, the analysts estimated the parameters (or coefficients) of equations linking the probability of surviving to individual characteristics of the child, such as the age of the child's mother and birth order. To give a simple example, we might estimate the coefficients a and b in the equation:

$$p_i = a + bx_i + error,$$

where p_i takes on a value of 1 if the child died and 0 otherwise, and x_i is the birth order of the child. Once a and b have been estimated, using standard statistical procedures (such as regression), we could use the equation to predict the probability $\hat{p}_j$ that the *jth* child died ($\hat{p}_j = \hat{a} + \hat{b}x_j$). We could also predict the average probability of a child dying if the average birth order of the sample, for example, was 3. In this simple model, $\hat{p}$ would then equal $\hat{a} + \hat{b}(3)$. We could further estimate the change in $\hat{p}$ that would result from reducing average birth order from 7 to 3. Clearly, all these results depend both on the values of the estimated parameters $\hat{a}$ and $\hat{b}$ and on the magnitude of the hypothesized change in the independent variable, x. In the calculations that follow, we consider precisely this type of change, except that the prediction equations are more complex than the linear function shown above and include several independent variables.

Results from this type of calculation using equations from Martin et al. (1983) and Trussell and Hammerslough (1983), shown in Table 3, indicate that the children of young mothers (less than 20 years of age) suffer much higher mortality than do the children of mothers 20 years of age or older. In this and subsequent tables, the proportionate changes indicate how much mortality rates would change if independent variables were changed from their present values to new hypothetical values. Hence, the results shown are relative to actual values of the independent variables in the samples. To determine the amount of change that would occur if explanatory variables were altered from one hypothetical distribution to another, a simple calculation is required. For example, if all births[9] in Sri Lanka occurred in women under age 20, the infant mortality rate would rise by 22 percent, as shown in Table 3, and if all births occurred in women 20–34, infant mortality would fall by 5 percent. Hence, if we moved from a situation where births occurred only to women under 20 to a situation where births occurred only to women 20–34, infant mortality would fall by $1-(.95/1.22)=22.1$ percent. Though the prime reproductive ages are generally considered to be between 20 and 34, the hazard model results reported in the original paper (Martin et al.) for Pakistan and Indonesia indicate that children of older women (35+) have lower mortality than the children of women in these "prime" repro-

Table 3 Effects of changing maternal age at birth and birth order on ${}_1q_0$ and ${}_4q_1$

	Reference		Effect of changes in percent[a]				
Country	${}_1q_0$	${}_4q_1$	**Maternal age <20**	**Maternal age = 20–34**	**Maternal age ≥35**	**No birth order 4+[b]**	**Maternal age = 20–34 and no birth order 4+[b]**
Pakistan	130	077	+36	−5	−19	−7	−12
Indonesia	106	081	+38	−6	−15	−11	−16
Philippines	055	012	+28	−2	−1	−9	−11
Sri Lanka	055	024	+22	−5	+15	−5	−9

[a] Values of all variables other than maternal age and birth order held constant at means. Only changes in ${}_1q_0$ presented; changes in ${}_4q_1$ differ slightly.
[b] Birth orders 4 and higher are distributed proportionately to the categories birth order = 1 and birth order = 2 and 3.
Source: Martin et al., 1983, for Pakistan, Indonesia, and Philippines. Trussell and Hammerslough, 1983, for Sri Lanka.

ductive ages. Only in Sri Lanka does the expected U-shaped pattern with respect to age emerge. In all four countries, the estimated effect of eliminating childbearing before age 20 and after age 34 is relatively small (averaging roughly 5 percent) because most childbearing is already concentrated in the 20–34 age span, so moving the remainder of births to this maternal age range has little effect on overall child mortality. Furthermore, as mentioned above, only in Sri Lanka does the elimination of childbearing above age 35 have a net impact of decreasing the child mortality level.

In fact, the impact on mortality of changes in maternal age due to an increase in contraceptive use is likely to be even smaller than indicated by the estimates shown in Table 3. The reason is that while the proportion of children born to old mothers (35+) is likely to decline as contraceptive use increases, the proportion of children born to young mothers (under age 20) is not likely to decline as contraceptive use increases, in most countries. It is this proportion of mothers under age 20 that contributes most to the small impact shown in Table 3. Increases in age at first birth and reductions in teenage fertility are likely to result only from changes in age at marriage accompanying general socioeconomic development and changes in the status of women.

The estimated impact on child mortality when childbearing ceases at three children is also shown in Table 3. On average across the four countries, child mortality rates would be lowered by 8 percent if higher order births were eliminated. A reasonable estimate of the combined effect of concentrating childbearing in the age range 20–34 and eliminating births after the third child can be obtained by simply summing the two separate effects; on average, mortality would decline by 12 percent. Since the role of family planning in helping couples to reduce higher order births is well documented, the impact of increased contraceptive use on mortality is likely to be greater through reducing parity than through lowering maternal age at birth. As discussed earlier, this parity effect may be largely a spacing effect in disguise.

Hobcraft et al. (1983), Holland (1983), and Cleland and Sathar (1983), have shown that birth spacing has a large impact on child mortality. In Table 4, we consider the impact on ${}_1q_0$ and ${}_4q_1$ of changing spacing patterns in three ways. In this table, the convention proposed by Hobcraft et al. is used: spacing patterns are represented by the format SZ.L, where S denotes the number of children born 2–6 years before the index child, Z denotes the number born 0–2 years before the index child, and L denotes the number born 0–17 (if estimating ${}_1q_1$) or 0–29 (if estimating ${}_3q_2$) months after the birth of the index child. The patterns we examine are SZ.L = 10.0, 11.0, and 21.1+. In words, these three patterns are: (1) 10.0 = only one sibling born within 2–6 years before the index child, and none born 0–2 years before or 0–17 (or 0–29) months after, (2) 11.0 = one born 2–6 years before, one born 0–2 years before, and none born 0–17 (or 0–29) months after, and (3) 21.1+ = two born 2–6 years before, one born 0–2 years before, and at least one born 0–17 (or 0–29) months after. Of the three, the first, SZ.L = 10.0, is expected to be the most favorable, and the last, 21.1+, the least favorable by far. The results generally bear out these expectations: as we move across the rows labeled 10.0, 11.0 and 21.1+, the percent change generally rises, indicating successively higher mortality. Moreover, switching universally to the "ideal" pattern SZ.L = 10.0, implying a prior birth interval of at least two years, would lower ${}_1q_0$ in all but two countries (Senegal and Lesotho) and would lower ${}_4q_1$ in all but five countries (Senegal, Haiti, Panama, Jamaica, and Malaysia).[10]

The variation across countries in the estimates in Table 4 is mainly mechanically caused by differences in the parameter estimates in the original study by Hobcraft et al.; instability in the parameter estimates reflects primarily small sample sizes in many of the covariate categories. Other sources of variability in the parameter estimates are differences in data quality across country and in the extent of birth date imputation in the data (see Trussell, 1984). Fortunately, we do not have to rely on estimates from a single country. Instead, we can examine the distribution of estimates generated for many populations. These distributions are displayed in Figure 1, using box plots.[11] Examination of these box plots confirms our expectation that the spacing pattern 10.0 is su-

Table 4 Effects of changing spacing* patterns on $_1q_0$ and $_4q_1$

	$_1q_0$				$_4q_1$			
	Reference value[b]	% change if all births spaced[a]			Reference Value[b]	% change if all births spaced[a]		
Country		10.0	11.0	21.1+		10.0	11.0	21.1+
Africa								
Senegal	120	0	12	4	151	14	−13	−29
Lesotho	132	2	−7	15	054	−7	14	117
Kenya	095	−25	−15	−13	064	−13	21	39
Sudan	076	−28	−9	−1	063	−14	17	59
Ghana	075	−30	−16	8	058	−11	15	134
Americas								
Haiti	147	−23	−6	16	082	1	6	25
Peru	107	−27	4	18	064	−33	10	109
Ecuador	085	−23	−9	1	052	−14	36	95
Mexico	075	−22	−5	0	036	−18	11	79
Colombia	068	−28	4	5	042	−9	40	17
Costa Rica	071	−33	8	31	019	−28	−15	24
Guyana	055	−24	6	10	014	−25	−14	111
Panama	044	−11	−9	−2	018	1	6	21
Jamaica	041	−5	5	8	014	0	14	41
Asia								
Nepal	168	−27	−9	15	096	−20	24	106
Bangladesh	136	−23	−2	−1	080	−29	9	151
Pakistan	130	−23	−18	−18	077	−22	26	111
Indonesia	106	−24	−6	−2	081	−33	7	207
Thailand	079	−28	−19	−23	034	−34	28	242
Jordan	069	−37	−6	0	026	−32	39	80
Syria	067	−31	−25	−18	025	−18	37	87
Philippines	055	−23	22	24	012	−1	97	154
Sri Lanka	053	−10	7	−6	024	−20	−2	44
Korea	043	−26	−19	−1	021	−20	42	336
Malaysia	043	−13	−2	−6	009	16	45	59

* Calculated from Hobcraft et al. (1983).

[a] Spacing is defined by the notation SZ.L, where S = number of previous births 2–6 years before index child, Z = number previous births 0–2 years before index child and L = number subsequent births 0–17 months for $_1q_1$ or 0–29 months for $_3q_2$ after birth of index child.

[b] See Table 1, footnote f, for description.

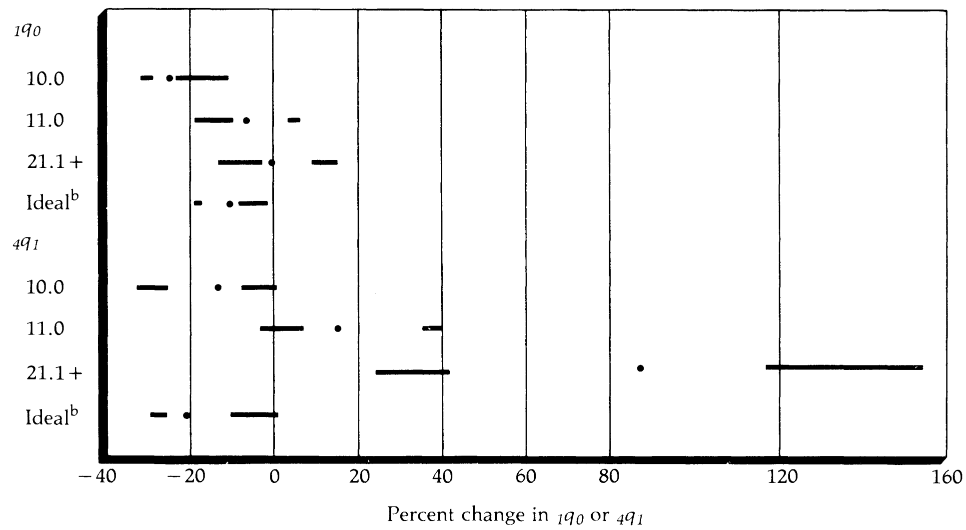
perior to 11.0 which in turn is superior to 21.1+. These estimates indicate that if all births were spaced in the "ideal" pattern (10.0), the median reductions in $_1q_0$ and $_4q_1$ would be 24 percent and 13 percent, respectively.

These results are misleading, however, for at least three reasons. First, it is logically impossible for all births to have the ideal spacing pattern of one birth 2–6 years before and no births 0–2 years before. Obviously, first births could not have this spacing. Thus, our ideal spacing pattern applies only to second and higher order births. For first births, SZ by definition would equal 00 and L would ideally equal 0. Hence, the ideal pattern for first births would be SZ.L = 00.0. Figure 1 shows that if first births and subsequent births each were ideally spaced, then $_1q_0$ would be reduced by an average across countries of 10 percent and $_4q_1$ by an average of 21 percent.[12] Because first births cannot be explicitly distinguished from higher order births in the Hobcraft et al. study, however, the predicted mortality rate for first births is likely to be biased. Examination of the results convinces us that the estimates of mortality for first births are biased upward; therefore, the estimated changes in overall mortality resulting from the adoption of the ideal spacing pattern are biased downward.

Even with this adjustment, it would be unrealistic to believe that changing spacing patterns alone—for example, through the use of contraception—would have an effect of this magnitude. The reason is that these "child spacing effects" are almost certainly due, in part, to the effects of breastfeeding in both delaying resumption of menstruation and improving child health. A spurious spacing effect would result if a woman tended to breastfeed each of her children for the same length of time. Then, a short prior birth interval reflects early weaning of the prior child which in turn is associated with early weaning of the index child. Lengthening birth intervals through breastfeeding would have a greater positive impact on child survival than would increasing birth intervals through contraceptive use, other things being equal.

Hence, it is important to establish whether spacing effects persist after the effects of breastfeeding the index

Figure 1 Percentage change in $_1q_0$ or $_4q_1$ induced by a change in spacing patterns to SZ.L[a]



Note: The dot indicates the median; the solid lines extend from the quartiles to the eighths.
[a]Spacing is defined by the notation SZ.L, where S = number of previous births 2–6 years before index child, Z = number previous births 0–2 years before index child, and L = number subsequent births 0–17 months for $_1q_1$ or 0–29 months for $_3q_2$ after birth of index child.
[b]In the ideal pattern, first births have the ideal pattern 00.0 and subsequent births have the ideal pattern 10.0.

child are controlled. Cleland and Sathar provide a partial answer. Because of the coding of age at death in the Pakistan survey and the imprecision of retrospective breastfeeding reports, it is impossible in most cases to decide whether termination of breastfeeding was caused by death or vice versa. Therefore, Cleland and Sathar could control for breastfeeding only when analyzing mortality after the first year. They present results only for early child mortality and conclude that the negative association between birth interval length and $_1q_1$ cannot be explained by the timing of weaning.

Holland used a different methodology in his Malaysia study, and the coding of age at death was more precise in the Malaysian Family Life Survey than in the Pakistan survey. As a consequence, the difficulty in separating cause and effect in the weaning/death association was largely eliminated. He found that prior interval length was negatively related to $_1q_0$ even when the effect of breastfeeding was controlled. If breastfeeding duration is held constant but short prior birth intervals are eliminated, the estimated change in $_1q_0$ is a reduction of 16 percent. In comparison, the estimate implied by the Hobcraft et al. study for Malaysia is a reduction of 9.3 percent for $_1q_0$ (not shown), after allowing for first births. Although we might have expected the Hobcraft et al. study to produce a larger estimated reduction since the effects of breastfeeding are not controlled, the two studies are not directly comparable because they employ different data sets and different specifications of spacing effects.

Even the Holland estimate, however, is potentially biased. When a breastfeeding child dies, ovulation is likely to resume more quickly. In the absence of contraception, the consequence would be a more rapid next conception. Some short prior birth intervals are likely to be caused by the death of the previous child. The death of the index child is more probable if the previous child died. Thus, the birth spacing variable may be spuriously capturing an intra-family correlation in the propensity for children dying. To avoid this potential bias, the survival status of the previous birth must be controlled. Cleland and Sathar found a strong spacing effect regardless of whether the preceding child survived for two years or died before the age of two. They did not, however, control simultaneously for the survival status of the preceding child and breastfeeding of the index child. They estimate that the universal adoption of the "ideal" spacing pattern discussed above would reduce $_1q_0$ by 15 percent in Pakistan. Unfortunately, it is not clear whether the effects of any potentially confounding variables were controlled or how first births were handled in this calculation.

Producing a "best" estimate of the impact that the "ideal" spacing pattern would have on mortality reduction is difficult. No single study has controlled for all obvious confounding variables simultaneously. The estimates from the Hobcraft et al. study are probably slightly overstated because they do not control for the effects of breastfeeding and survival of the previous child, but they are also likely to be understated because the estimated mortality rates of first births are probably exaggerated. These two effects will, on average, tend to compensate for each other. Hence, our preferred estimate is the median change for the 25 countries in the Hobcraft et al. study; if all births are spaced at least two years apart, we might expect reductions of 10 percent in ${}_1q_0$ and 21 percent in ${}_4q_0$.

Maternal Mortality

To estimate the amount of change in maternal mortality that would result from a hypothetical change in the age and parity distribution of births, we use maternal mortality ratios and distributions of live births by maternal age and parity from the Chen et al. study (1974).[13] In the first row of Table 5, we show the ratio that was actually observed in the Matlab population, plus changes in maternal mortality ratios that might occur in four hypothetical situations. In each case, we have altered the observed distribution of live births by maternal age and/or parity and then used the age and parity distribution of maternal mortality ratios for the Matlab population to calculate new ratios. In column B, the maternal age distribution of live births is changed by eliminating all births to women under 20 and over 39. In this case, we ignore the fact that the age and parity distributions of births are closely linked in this and other populations. Therefore, the figure 4.9 maternal deaths per 1,000 live births is calculated from the proportions of live births to women 20–29 and 30–39 years old and the ratios for these two age groups. In this case, the reduction in the maternal mortality ratio would be 14 percent, as shown in row 2.

Next, in column C, we consider the analogous exercise for parity: elimination of births at parities above 5, ignoring any interaction between maternal age and parity. For convenience, the rates for primagravida and 0–1 multigravida in Table 2 are combined into a single rate. This change in the parity distribution reduces the maternal mortality ratio to 5.5, for a reduction in mortality of only 3.5 percent. Eliminating all births beyond the third parity also has little effect in this population: the ratio (not shown) in this case is 5.6 deaths per 1,000 births, or a reduction of only 1.8 percent. The reason is that maternal mortality is greatest in the 0–1 parity group, and simply redistributing births away from the higher

Table 5 Changes in the maternal mortality ratio resulting from hypothetical changes in the maternal age and parity distribution of live births

	(A) Observed	(B) Eliminating births under 20 and over 39,[b] and ignoring parity distribution	(C) Eliminating births to women above parity 5, ignoring age distribution[c]	(D) Eliminating births under 20 and over 39, with commensurate change in parity distribution[d]	(E) Eliminating births under 20 and over 39 and eliminating births to women above parity 5
(1) Maternal mortality ratio[a]	5.7	4.9	5.5	5.1	4.5
(2) Percent reduction	—	14.0	3.5	10.5	21.1
(3) Maternal mortality rate if general fertility rate is constant	0.9	0.8	0.9	0.8	0.7
(4) Percent reduction	—	11.1	0.0	11.1	22.2
(5) Overall mortality rate for women of reproductive age if general fertility rate is constant	3.4	3.3	3.4	3.4	3.3
(6) Percent reduction	—	2.9	0.0	0.0	2.9
(7) Maternal mortality rate if general fertility rate declines by 25%	0.7	0.6	0.7	0.6	0.6
(8) Percent reduction[e]	22.2	33.3	22.2	33.3	33.3
(9) Overall mortality rate for women of reproductive age if general fertility rate declines by 25%	3.2	3.1	3.2	3.1	3.1
(10) Percent reduction[f]	5.9	8.8	5.9	8.8	8.8

[a] Number of maternal deaths per 1,000 live births.
[b] Based on marginal distribution of age only.
[c] Based on marginal distribution of parity only.
[d] Assumes arbitrarily that all births occurring at ages 10–19 and one-half of the births occurring at 20–29 in the original distribution now occur at ages 20–29, and that one-half of the original 20–29 births and all births at ages 30–49 now occur at ages 30–39.
[e] Relative to the observed rate in column (3).
[f] Relative to the observed rate in column (5).
Source: Based on results from Matlab Thana, Bangladesh in Chen et al. (1977), Table 9, p. 338.

parity groups shifts proportionately more into the high risk 0–1 parity.

Clearly, it is unrealistic to pretend that the distributions of births by maternal age and parity are unrelated. For example, among mothers who are 10–19 years old, there are likely to be more first births than higher order births. Since maternal mortality is related to both age and parity, and most probably to the interaction of the two, it is important to consider how the parity distribution of births by age might change if we eliminate births at youngest (under 20) and oldest (over 39) ages. In column D we have, therefore, calculated the maternal mortality ratio eliminating births under 20 and over 39 and rearranging the parity distributions for the age groups 20 to 29 and 30 to 39 as described in Table 5 (footnote d) to compensate for the change in age distribution. The choice of this redistribution procedure is arbitrary, and other methods may yield slightly different results. As might be expected, the resulting amount of maternal mortality reduction in this case is somewhat less than what resulted from eliminating births at youngest and oldest ages alone in column B. Overall, the reduction expected would be approximately 10.5 percent.

Finally, in column E we take the age-parity distribution of live births in column D and eliminate all births at parities above 5. The effect is to reduce the maternal mortality ratio further from 5.1 to 4.5. Thus, if no births occurred in this population below age 20 and above age 39 and there were no births beyond parity 5, the maternal mortality ratio might drop from 5.7 reported by Chen et al. to something like 4.5, a decline of 21.1 percent.

In rows 3–10 of Table 5, we consider the impact of these changes in the maternal mortality ratio on the maternal mortality rate (maternal deaths per 1,000 women aged 15–49) and the total death rate for women of reproductive age. The maternal mortality rate as used here is the product of the maternal mortality ratio and the general fertility rate (GFR)—the number of live births per 1,000 women of reproductive age. Obviously, if fewer women are exposed to pregnancy in a given period, the maternal mortality rate will be lower. The effects of our four changes in the age and parity distribution on the maternal mortality rate are evaluated in two ways. First, in rows 3–6 we consider the unlikely circumstance in which the age and parity distribution of live births changes but the general fertility rate remains constant. Second, we assume that the changes in age and parity distribution of live births result in a 25 percent decline in the general fertility rate.[14] In this case, the maternal mortality rate in row 7 is simply 75 percent of the rate in row 3. As is clear in Table 5 (rows 6 and 10), changes in the age/parity distribution of live births and in their number may have an important but generally small impact on the proportion of women of reproductive age who die.[15] The largest effect, which comes from eliminating births below 20 and above 39 and to women above parity 5, while the general fertility rate is reduced by 25 percent, is a reduction in the maternal mortality rate by one-third (row 8). The overall mortality rate for women of reproductive age in this particular population would change from 3.4 per 1,000 to 3.1, for a reduction of about 8.8 percent.

Actual Impact of Family Planning

As is apparent from the previous sections, to determine the impact that a change in reproductive behavior (induced, perhaps, by a family planning program) might actually have on mortality, we need to know how the change would affect the maternal age, parity, and birth interval distributions of live births.

For illustrative purposes, we have attempted to make some reasonable guesses about the types of changes that might take place in reproductive behavior in response to family planning: higher order births and short birth intervals would be less common, women would be less likely to give birth below the age of 20 and after 35, and fertility would decline due to increased contraceptive use. It is important to bear in mind that although these are changes that reflect the goals of family planning, their actual occurrence in a particular country is another matter. For example, it is unlikely that a family planning program will reduce the number of births occurring to women under age 20 unless there is also an increase in the age at marriage. Even if contraceptives were made widely available to young, newly married women, very few might want to delay their first birth.

To obtain a more realistic picture than provided in this paper of the impact a family planning program might have on mortality, we need to know not only what type of changes in reproductive behavior may take place, but also the magnitude of those changes. In other words, rather than assuming all births at birth orders 5 and above are eliminated, we would like to know what proportion of births would occur at each birth order.

There are two ways in which we might attempt to determine how the distribution of births by maternal age, parity, and birth spacing would actually change. First, we might compare the distribution of births at two or more periods of time in countries that have implemented successful family planning programs. Such a comparison could, in principle, be readily made using maternity history data from surveys conducted at different points in time. The major problem with this approach is that the observed changes may not be solely attributable to the activities of the family planning program, as the continuing debate about the relative roles of family planning programs and economic and social development in reducing fertility suggests (Lapham and Mauldin, 1984).

A second approach would be to simulate the effect that a family planning program, or more specifically, increased contraceptive use, would have on the distribution of births by maternal age, parity, and birth spacing occurring each year. Through simulation, we could assess the potential impact a family planning program would have in a population, rather than document what has happened in the past. Two types of simulations have been developed for exactly this purpose. The first group includes macrosimulations such as TABRAP and CONVERSE (Nortman et al., 1978), designed to project a population, with annual variations in fertility depending on the number of new contraceptive acceptors and the rate at which contraceptive use is discontinued. These simulations are usually based only on age-specific fertility calculations, and therefore they cannot provide the necessary information about resulting parity distributions or birth spacing. The second group consists of microsimulations or family building models, such as POPSIM and POPREP (Lachenbruch, Sheps, and Sorant, 1973; Horvitz et al., 1971). These programs simulate actual maternity histories of individual women in a population using Monte Carlo techniques. Although they are fairly complicated and expensive to use, they have the potential of producing the type of results needed to assess the impact of a family planning program.

The obvious difficulty with simulations is that they necessitate making asumptions about how contraceptive use is diffused throughout a population and how effectively couples who adopt contraception will use it. While much research has been done concerning the impact of family planning programs on the *level* of fertility (see, for example, Mauldin, 1982; Mauldin and Berelson, 1978; and Lapham and Mauldin, 1984), considerably less is known about how the implementation of family planning programs actually alters reproductive behavior. Furthermore, there is undoubtedly substantial intercountry variation in the way a program affects fertility. For example, in Taiwan and Korea, fertility rates started to decline first among older women, but in Costa Rica, the fertility decline began among younger women (Stycos, 1982). Thus, while the estimates of changes in a population's fertility pattern generated by a simulation may be more realistic than the changes we have considered, they would still depend heavily on the assumptions made about the implementation and effectiveness of a family planning program.

Summary

In this paper, we have discussed several mechanisms by which widespread adoption of contraception could lower infant, child, and maternal mortality. We have also generated quantitative estimates that show to what degree mortality in developing countries would be reduced under hypothetical changes in the distributions of maternal age at birth, birth order, and birth spacing patterns. Our results indicate that if childbearing were confined to the "prime" reproductive ages of 20–34, the infant and child mortality rate would fall by about 5 percent. If childbearing is limited to ages 20–39, the maternal mortality ratio could be reduced by about 11 percent. The elimination of fourth and higher order births would reduce infant and child mortality by about 8 percent. The maternal mortality ratio would decline by about 4 percent if fifth and higher order births were eliminated. The elimination of fourth and higher order births while simultaneously confining childbearing to ages 20–34 would reduce ${}_1q_0$ by 12 percent.

Alternatively, an improvement in birth spacing patterns, so that all births subsequent to the first are spaced at least two years apart, is estimated to reduce ${}_1q_0$ by an average of 10 percent and ${}_4q_1$ by an average of 21 percent among the 25 countries studied. Hence, improving birth spacing patterns and eliminating births to women at highest risk ages and parities are expected to lower mortality by approximately the same amount. Unfortunately, we are unable to estimate the combined effect of changing age, parity, and birth spacing simultaneously because there are no studies that report results when all three factors are included in the same multivariate model.

Thus, there is definite potential for reducing infant, child, and maternal mortality through increased use of contraception. Whether or not family planning programs can effect the types of changes in behavior discussed in this paper is open to question.

In absolute magnitude, the potential effects on infant and child mortality of changing fertility patterns appear to be modest, though certainly important. We can apply this same methodology to estimate the relative effect that raising levels of maternal education would have on infant and child morality. In the 25 countries included in the comparative study by Hobcraft et al. (1983), giving all mothers seven or more years of education is estimated to lower ${}_1q_0$ by an average of 41 percent and ${}_4q_1$ by an average of 60 percent, other things equal. In the more intensive studies by Martin et al. (1983) and Trussell and Hammerslough (1983), in which the effects of many more socioeconomic variables are controlled, the implied reduction in ${}_1q_0$ is still large, averaging 25 percent for Indonesia, Pakistan, the Philippines, and Sri Lanka.

It would appear, therefore, that the education of girls lowers mortality to a greater extent than does altering reproductive behavior. The costs of reducing mortality through female education as opposed to implementing family planning programs would surely differ. The reduction of infant and child mortality, while it may be one goal, is not the primary rationale for either education or family planning. Nevertheless, it is highly probable that either approach may have the side effect of lowering mortality.

Appendix

Calculation of ${}_1q_0$ and ${}_4q_1$

Two measures of childhood mortality are used in this paper, ${}_1q_0$ and ${}_4q_1$. To calculate these rates—either for the actual sample or for a hypothetical population—we use estimated parameters and a set of average characteristics from each study. In the models on which our results are based, mortality rates are nonlinear functions of these parameters and the distributions of these characteristics in the sample. The set of parameters can be divided into two subsets, consisting of the set of coefficients of the independent variables (hereafter denoted by the vector β) and other parameters (hereafter denoted by the vector α). The purpose of the original papers was to estimate the value of β and α given information on the covariates such as age, education of the mother, birth order, and sex of the infant. We, on the other hand, want to predict the probability of dying for various hypothetical values of the covariates or independent variables (hereafter denoted by the vector of characteristics X), given the estimated parameters. In symbols, ${}_nq_x = f(\beta'X,\alpha)$, where ${}_nq_x$ is the predicted probability of dying between exact ages x and $x+n$; $\beta'X$ is the linear combination of the covariates, each multiplied by its estimated coefficient; and α is the vector of other parameters estimated in the model. Because different statistical procedures are used, we describe the calculation of ${}_1q_0$ and ${}_4q_1$ separately for each study.

Martin et al. (1983) and Trussell and Hammerslough (1983) both use a hazards model approach, described in detail by Trussell and Hammerslough (1983). Both studies use the same age categories, of which only the first six (through age five) are needed for our calculations. We denote the set of covariates by the vector X, and the associated parameter estimates, including the intercept, by the vector β. The symbols $\alpha_1, \alpha_2, \ldots, \alpha_6$, denote the age parameters and the width, in years, of each category. Then,

$${}_1q_0 = 1.0 - exp\,[-(w_1e^{\alpha_1} + w_2e^{\alpha_2} + w_3e^{\alpha_3} + w_4e^{\alpha_4})\,e^{\beta'X}] \quad \text{(A1)}$$

and

$${}_4q_1 = 1.0 - exp\,[-(w_5e^{\alpha_5} + w_6e^{\alpha_6})\,e^{\beta'X}] \quad \text{(A2)}$$

The values of $w_1, w_2, \ldots, w_6$ in these studies are 1/12, 2/12, 3/12, 6/12, 1, and 3. Values of β are taken from Table 3 of Trussell and Hammerslough and Tables 1, 2, and 3 of Martin et al. Our calculations consisted of substituting in a set of X values and, using the parameter estimates from the original studies (Martin et al., 1983 and Trussell and Hammerslough, 1983), solving the equations for ${}_nq_x$.

Holland (1983) also used hazards models, but estimated separate equations for each of four subintervals in the first year, so that there are four sets of βs, denoted β_k where $k = 1, 2, 3, 4$. Since his analysis is limited to infant mortality, only ${}_1q_0$ can be calculated.

$${}_1q_0 = 1.0 - \prod_k exp\,(-w_ke^{\beta_k'X}) \quad \text{(A3)}$$

Because exposure is measured in months (not years), the values of w_k are 2, 2, 3, and 5. The values of β_k are taken from Holland's Table 17 (p. 98).

Hobcraft et al. (1983) used *probability* models, not hazard models, so the equations are simpler. See Hobcraft et al. for further details. There are four separate equations for neonatal, postneonatal, toddler, and child mortality. The associated parameter vectors are denoted β_k where $k=1, 2, 3, 4$.

$${}_1q_0 = 1.0 - (1.0 - e^{\beta_1'X})\,(1.0 - e^{\beta_2'X}) \quad \text{(A4)}$$

$${}_4q_0 = 1.0 - (1.0 - e^{\beta_3'X})\,(1.0 - e^{\beta_4'X}) \quad \text{(A5)}$$

Values of βs were supplied by John McDonald of the World Fertility Survey. Exponentiated βs are shown in Tables 3, 4, 5, and 6 of Hobcraft et al. (pp. 594, 596, 598, 599).

Evaluating the Effect of a Change in Population Characteristics on ${}_1q_0$ and ${}_4q_1$

We are interested in assessing what would happen to ${}_1q_0$ or ${}_4q_1$ if certain characteristics of births in a population were changed. For example, other things remaining equal, what would happen to predicted mortality rates if all women had 7+ years of education? To find the answer, we need to know the distribution of characteristics before and after the change. To get the predicted rate before the change, we should calculate the predicted rate for each cell in the covariate matrix and then find the weighted average, where the weights are the fractions of births in each cell in the covariate matrix.

We do not, however, have the proportions in the covariate matrixes. In fact, there are approximately 500,000 cells in some of these matrixes. We only have the *marginals*, which are printed in the various studies discussed here. Hence, we are forced to evaluate the predicted mortality rate for a baby with *average* characteristics. We do this calculation by substituting $\bar{X}$ for X in equations A1 through A5 above. The overall level predicted by the equations will not be the correct one, since the functions are highly nonlinear (the function of an average is not the average function) and since (having only the marginals) we in effect must assume that none of the covariates are correlated. Even though *levels* are incorrect, however, implied proportional changes caused by substituting a hypothetical distribution (say, X^*) instead of $\bar{X}$ are generally close to the answers that would be obtained in the correct manner. Econometricians have recognized this property in logit and tobit regressions; the predicted Y when the Xs are set at their means is not $\bar{Y}$ as it would be with ordinary least squares (OLS), but the derivatives with respect to the parameters, estimated at the means of the Xs, are generally close to the OLS parameter estimates. When we tried adjusting the constants to give the correct level, the implied proportional changes (when X^* was substituted for $\bar{X}$) were nearly identical to those obtained without adjusting the constants. Hence, we are confident that the implied proportional impacts that we estimate are reasonably accurate.

Of far more concern is the fact that we do not have the appropriate $\bar{X}$s, except for the Hobcraft et al. study. Hobcraft et al. estimated their models on a sample consisting of all births 5 to 15 years before the WFS survey, and the marginals for covariates are available. The other three studies, however, have a sample of *all* births to women in the surveys. Thus, the marginals do not refer to any period. For example, since the surveys pertain to women who were 15–49 years of age *at the time of the survey*, births to older women are grossly under-represented and those to younger women are over-represented (relative to what a distribution of births in a given period would be) since all women can contribute births when young but only a few can when old. Means of almost all covariates, calculated from the marginals, are distorted and thus require adjustment before we can determine how the mortality rate would change if the Xs change. Information on the current period is clearly unavailable. We could, in principle, get the characteristics of births near the time of each WFS survey, but most of these surveys were conducted 5–10 years ago. Since the Hobcraft et al. means do apply to a particular period, they are not distorted by sample selection bias. The means, however, pertain to a period on average more than 15 years ago. Though in principle we could obtain means for a time closer to the present from the WFS standard recode files, in fact we have access only to a few of those used in these studies.

Thus we were forced to resort to cruder adjustments. The means for the Hobcraft et al. study were not adjusted. For the other studies we adjusted only the maternal age and birth order means because these were the variables we altered when assessing the potential effects of family planning on mortality. Adjustment of the means of other variables would have far less impact on our estimates, because they remain constant throughout. Maternal age distributions were adjusted by estimating the births for age groups of women during the three years before the survey, with age-specific fertility rates taken from Hannenberg (1980), and number of women married and proportion married taken from Goldman and Hobcraft (1982). The distribution of births by birth order (one, two–three, four+) was much harder to adjust, since data on the distribution of births by birth order have seldom been published by the World Fertility Survey. Some of the evaluations of individual WFS surveys include total fertility rates, total first birth rates, and total fourth-and-higher order birth rates. These reports indicate that during the five years before the surveys, first births constituted about 14 percent of births when the total fertility rate was high (~5.5) and about 18 percent when the total fertility rate was lower (~4.5). The proportion of second and third order births was generally about twice the proportion of first order births. Information for Malaysia was available directly from Yatim (1982), and indicates a distribution split of 17 percent for first births; 30 percent for second and third births; and 53 percent for fourth and higher order births. Based on these findings, we split the births in the Philippines and Indonesia in the proportions 18/36/46, in Pakistan in the proportions 14/28/58, and in Sri Lanka 20/40/40. These latter estimates almost certainly are not correct, but they are equally certain to be better than those "observed" from the entire sample. For example, in the Malaysian Survey used by Holland, the births were split 20/17/63. (This result seems quite implausible, but it is the one reported.) Within each parity for Malaysia, the observed distribution of births by short and long prior birth intervals was not adjusted since there was insufficient information.

Notes

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1 For reviews of this literature, see Nortman, 1974; Gray, 1981; Omram, 1976 and 1981; Wray, 1971, and Winikoff, 1983.

2 It is also possible that a family planning program emphasizing oral contraceptives (OCs) may increase infant and child mortality. The use of OCs appears to reduce the quantity of breastmilk a woman can produce, although it is less certain whether there is any change in the quality of the milk (Hull, 1981). Because breastmilk is important to the survival of infants in many developing countries, a reduction in breastmilk due to OC use could result in increased mortality. However, results from the World Fertility Survey indicate that, even in developing countries with large proportions of women using OCs, there is very little overlap between OC use and lactation (Pebley et al., 1982).

3 The real effect of parity may, in fact, not be an entirely biological effect, but may also represent competition for limited maternal and familial resources.

4 Maternal mortality is generally measured in two ways: (1) the maternal mortality *ratio* relates the number of maternity-associated deaths to the number of live births, (2) the maternal mortality *rate* relates the number of maternity-associated deaths to the number of women of reproductive age.

5 Another important effect of a family planning program on maternal mortality is the reduction of mortality resulting from induced abortions (see, for example, Rochat et al., 1981). If women who want to avoid pregnancy adopt contraception, they are far less likely to have to resort to abortion (often performed under unsafe conditions in developing countries). We do not attempt to estimate directly the degree of maternal mortality caused by abortions that may be eliminated by contraceptive use, since it is virtually impossible to obtain adequate data on induced abortion in many countries.

6 Each birth is assigned to one of five categories: (1) first, (2) second or third, short prior interval, (3) second or third, long prior interval, (4) fourth or higher, short prior interval, or (5) fourth or higher, long prior interval.

7 The symbol ${}_nq_x$ is defined as the probability of dying between exact ages x and $x+n$.

8 These variables are held constant sequentially, or in groups, but not simultaneously.

9 Note that only the *distribution* of births across the categories of the independent variables, not the absolute number of births, affects the estimated mortality rates.

10 We do not mean to indicate by our choice of the word *ideal* that even longer intervals would not yield lower mortality. The available information on this point is ambiguous. Hobcraft et al. originally had divided the period two to six years before the survey into two subintervals, two to four and four to six years. On the basis of exploratory work, they concluded that their models fit no better with the more complicated specification. The implication is that spacing births more than four years apart does not result in any further reductions in mortality. The Hobcraft et al. study is silent on the question of whether spacing births three to four years apart is superior to spacing them two to three years apart. Cleland and Sathar, however, found that for neonatal mortality, there appears to be a threshold of about three years, above which no further advantage is conferred on the index child. For post-neonatal and early child (${}_1q_0$) mortality, an approximately linear decline occurs up to four years. Note that our definition of the "ideal" spacing pattern allows only one birth in the *four*-year interval, two to six years before the birth of the index child. Hence the implied spacing pattern is really a birth every *four* (not *two*) years.

11 The median of the distribution is plotted as a single dot. The lines on either side of the median extend from the quartiles to the eights. Fifty percent of the observations lie above and fifty percent lie below the median. Twenty-five percent of the observations lie above the upper quartiles and eighths, and twelve and a half percent lie below the lower quartiles and eighths.

12 In these calculations, we assume that one-sixth of births are first births, since the exact distribution of births by birth order is unavailable (see the Appendix). Thus, we compute the expected change as five-sixths of the change if all births were spaced SZ.L = 10.0 plus one-sixth of the change if all births were spaced SZ.L = 00.0. In every case, the change to SZ.L = 00.0 raises ${}_1q_0$, but in all except three countries, it lowers ${}_4q_1$. As a consequence, the overall change, when first births are explicitly included, is smaller for ${}_1q_0$ than the corresponding figure in Table 4 but larger for ${}_4q_1$. Note that births with a spacing pattern of 00.0 include two very different groups: first births, and higher order births with extremely long prior interbirth intervals. There is some evidence that the latter group experiences higher mortality than the former.

13 It is important to note that Chen et al. do not control for potentially confounding variables, as the child mortality analyses in the last section do. Thus, the assumption that Chen et al.'s figures represent coefficients of causal relationships may be less tenable than in the case of the child mortality studies. Nevertheless, these are the only such data available.

14 The change is instantaneous. The women in Matlab Thana are assumed simply to have had 25 percent fewer births during the two-year observation period.

15 Because maternal mortality is likely to be underreported, even in the Matlab population, the effect may well be larger than our estimates indicate.

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