

Consequences of Unwanted Childbearing: A Study of Child Outcomes in Bangladesh

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21 March 2007

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Paper for presentation at the annual meeting of the Population Association of America,
New York, 29-31 March 2007. This research is supported by the U.S. Agency for
International Development (USAID), Bangladesh.

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ABSTRACT

The prevention of unwanted births (defined as births not wanted at the time of conception) has long been a fundamental justification for investment of public and private resources in family planning services. Unwanted childbearing is assumed to have detrimental consequences – for the child and for its family and larger community – that are distinctive, substantial, and potentially long-term. There is, however, surprisingly little empirical research that offers a solid scientific basis for this assumption, especially in low-income non-Western societies. We analyze longitudinal data from rural Bangladesh collected in the period 1982-2002 that are unusually well-suited to address the issue of the consequences of unwanted childbearing: the sample of children is moderately large, child wantedness is measured prospectively and on a sex-specific basis, and the follow-up is relatively long-term, extending through childhood into adolescence for some of the sample. Two child outcomes are examined: mortality before age 5; and educational attainment. We employ two analytical strategies to remove confounding effects of unmeasured factors: models with fixed effects for sibset; and a “natural experiment” provided by the random assignment of child sex. Estimation employing both of these strategies yields significant effects of child wantedness. Large effects on infant mortality (both neonatal and post-neonatal) emerge under the fixed effects approach, with odds ratios on the order of 2.0. Corresponding effects on mortality are not evident in the natural experiment analysis, however. The estimated effects on schooling are more consistent across the two approaches: unwanted children attain on average 7%-9% fewer years of schooling than wanted children.

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INTRODUCTION

The prevention of unwanted births has long been a fundamental justification for investment of public and private resources in family planning services. There are multiple rationales for the prevention of unwanted births, defined here as births not wanted at the time of conception. Where reduction in population growth rates is a policy goal, the prevention of unwanted births can contribute to the attainment of this societal goal. Unwanted births are generally assumed to be less costly to avert. The prevention of unwanted births also closes the gap between reproductive aspirations and outcomes, a worthy goal in its own right. But perhaps the most compelling rationale for investment in the prevention of unwanted births derives from the assumption that unwanted births bring distinctive and substantial costs. These costs can consist of disadvantage suffered by the unwanted child – in health, in early childhood development, and in later social and economic opportunity – and of damage to the well-being of siblings, parents, and communities. The detrimental repercussions of unwanted childbearing are presumed to be multifaceted and potentially long-term.

There is, however, surprisingly little empirical research that offers a solid scientific basis for this widespread belief. While a relatively large number of studies have been conducted in the U.S. and Europe (Brown and Eisenberg 1995; Gipson *et al.* 2007), much of this research does not stand up to rigorous scientific scrutiny, as we elaborate in the next section (see also Lloyd and Montgomery 1996; Joyce *et al.* 2000). Far fewer studies have been conducted in low-income societies outside the West (see reviews in Greene and Merrick 2005; Gipson *et al.* 2007). This is a curious lapse, given the enormous commitment of resources to family planning programs in these societies in the period since 1960, justified in part as interventions to reduce unwanted fertility and its attendant costs (although concerns about population growth were probably the crucial incentive in most instances). Indeed, it is plausible that the costs of unwanted fertility are on balance larger in low-income settings, because resource-constrained households and communities have less capacity to accommodate unwanted births and offset their various costs.

In this paper we contribute to filling this gap in the research literature through analysis of longitudinal data collected in rural Bangladesh in the period 1982-2002. The contribution of this research is twofold. First, the research design is advantaged in several key respects as compared to most

previous research, including most research conducted in the U.S. and Europe. Second, we analyze a child outcome – educational attainment – that to date has been examined in just a few studies. Schooling is an important human capital investment, with multiple benefits to the individual and positive externalities for the family and society.

The underlying motivation for research on the consequences of unwanted fertility is implicit in the above discussion but deserves explicit statement at the outset. At issue are the potential gains in the well-being of children, their families, and their communities that would follow from more perfect birth control. Because estimation of these potential gains is the objective, unwanted births are defined as births not desired at the time of conception; these are the births that more perfect birth control would avert. Of course these births may later be highly valued and receive investments equivalent to the investments in wanted births. And parents and communities may have means of accommodating the spillover effects of unwanted births on other persons. The research question, then, is whether child wantedness at conception has pervasive and longer-term effects on parental care and investment. An answer in the affirmative would provide a justification for investment in the prevention of unwanted births.

The next section discusses the challenge of conducting research on the consequences of unwanted fertility, identifying three major obstacles to obtaining valid conclusions. The subsequent section describes the data to be analyzed and the analytical approaches that we adopt. Results are presented in the fourth section of the paper, with a final section offering a summary and some concluding comments.

RESEARCH ON THE CONSEQUENCES OF UNWANTED CHILDBEARING

Analytical Challenges

All efforts to assess the repercussions of unwanted childbearing must confront three major analytical challenges.

Context and locus of the effect. There are two separable issues here. Consider first “context”. There are very good reasons to expect that the magnitude of the effects of unwanted childbearing will vary across societal setting (Lloyd 1994; Lloyd and Montgomery 1996). One can imagine contexts in which one or more factors work against unwanted childbearing having consequences of any magnitude. To be more explicit, likely conditioning factors include: (i) Level of economic development – the potential for differential investment in children grows as households acquire more resources; (ii) Stage of demographic transition – where fertility is more regulated, the

disruption caused by an unintended birth may be greater; (iii) Kinship and family norms – these will influence parental decisions about how to distribute the costs of an unwanted birth; (iv) Social expenditure on families and children by the state and other extra-familial agencies – these determine to what degree child well-being is a function of parental investment. To elaborate on this latter point, consider schooling, which is one of the outcomes analyzed in this paper: where primary schooling is compulsory and entails little direct financial outlay on the part of parents (although opportunity costs of lost child labor may remain), one might not expect unwanted childbearing to have much bearing on levels of primary school enrollment and completion.

“Locus of the effect” is a matter of who bears the cost of unwanted births. A first candidate is, of course, the child who was not wanted at conception. This is the focus of the empirical analysis in this paper, *viz* the effects of being an unwanted birth. But one can also posit effects on others, starting with the unwanted child’s siblings. Where parental decisions are guided by a norm of equality of distribution of resources among their children (Behrman *et al.* 1982; Behrman 1988), the costs of unwanted childbearing may be spread among all siblings. A specific form of cross-sib effect commonly described in the literature is an older sibling shouldering childcare responsibilities for a younger unwanted sib. (Greene and Merrick (2005) cite an extensive literature from all major regions that describes such a dynamic.) Among other consequences, the additional childcare responsibilities might compel the older child to drop out of school, especially if this older child is a girl. Alternatively, parents may cut back on non-childbearing investments so as to preserve equity of investments among their children, including the unwanted child (typically a higher-order birth). Under this scenario, parents will themselves bear the costs of unwanted childbearing, including possibly physical and emotional stress and reduction in leisure time. Finally, there may even be costs to the local community and larger society. The controversial hypothesis that legalization of induced abortion in the U.S. in the early 1970s reverberated in lower crime rates in the 1990s is in this vein (Donohue and Levitt 2001). In short, the absence of detectable effects of unwanted childbearing on certain persons – in particular the unwanted child – does not preclude the possibility that other persons have been substantially affected. Empirical investigation of this possibility, however, must surmount imposing measurement and analytical obstacles – information must be gathered on all those who might be affected by an unwanted birth, and a statistical model that incorporates the full set of effects of interest must be identified. We are not aware of any existing data-sets that are up to the task.

Measurement of child wantedness. An unwanted birth is defined as a child unwanted at the time of conception, and therefore the measurement of fertility preferences should be close to the time of conception. To ensure this would require more frequent interviews with couples at risk of conception than is feasible. In practice preferences are measured either retrospectively or prospectively at widely-

spaced intervals. Retrospective measurement typically takes the form “At the time you became pregnant with <name>, did you want to become pregnant at that time, sometime later, or not again?”. This item has the quite desirable feature of asking about feelings at the time of the conception. But certainly responses to this item are subject to recall error, and, of perhaps more concern, respondents’ discomfort with labeling specific children “unwanted”. This reluctance to admit to unwanted births can be detected in comparisons of prospective (“Do you want another child?”) and retrospective preferences: in those few empirical studies where this comparison can be made, there is a distinct pattern of births that would have been classified as unwanted prospectively being reported as wanted retrospectively, i.e. *ex post* revision towards wantedness (McClelland 1983; Bankole and Westoff 1998; Williams and Abna 2000; Koenig *et al.* 2006). It seems likely that births declared unwanted retrospectively are, among all unwanted births, those that are more strongly unwanted. It follows that the estimated consequences of unwanted childbearing based on retrospective reports, as is most common in the existing empirical literature, are probably upwardly biased estimates of the potential gains in child and family well-being that would follow from the elimination of unwanted births. Because of the demonstrated aversion to reporting retrospectively that a birth was unwanted, prospective preferences would appear to provide a far sounder basis for classifying births as wanted or unwanted (Bongaarts 1990, Lloyd and Montgomery 1996). But of course classification via prospective preferences requires panel data, and such data are relatively rare.

There are two further issues concerning the measurement of child wantedness. First, most demographic surveys treat this as a categorical variable, but it seems likely that the underlying variable is continuous, ranging from a strong desire not to have another birth through relative indifference to a strong desire to have another birth (Bachrach and Newcomer 1999; Santelli *et al.* 2003). Indeed, the underlying variable may well be multidimensional, and if so at any given moment another child may be wanted for some reasons and not wanted for others, leaving the respondent with ambivalent feelings towards becoming pregnant and having another birth. There is empirical evidence that such ambivalence is common. For example, in an analysis of DHS data from Africa, Speizer (2006) finds that a substantial fraction of respondents classified as having unmet need for limiting (i.e. do not want another child) also report that they would not mind were they to become pregnant soon. A second issue is from whom fertility preferences should be obtained. Virtually always data are available from the mother, but rarely from the father. Yet research in the U.S. reveals added explanatory power when both parents’ preferences are taken into account (Korenman *et al.* 2002). Typically men exercise more authority over household resources than women, and therefore men’s actions will be more determinative of the consequences of an unwanted birth, a further reason to ascertain child wantedness from the perspective of both the mother and father. Regrettably, the Bangladesh data analyzed in this paper are

deficient in both respects discussed in this paragraph: intensity of preferences is not measured, and preferences are measured for the mother but not the father.

Unobserved heterogeneity. Almost certainly child wantedness is not distributed randomly, rather is associated with a host of other factors that also affect the well-being of children and their families. Some of these factors are routinely measured and therefore can be explicitly controlled; these include sex of child, birth order, parental education, and household wealth. But other factors likely to be of some significance for child and family well-being are typically not measured in large demographic data collection efforts, in particular social and psychological factors that can influence parenting styles and degree of parental commitment to child investment. (An exception is the Detroit data analyzed in Axinn *et al.* 1998 and Barbar *et al.* 1999.) Moreover, although household resources are often measured, it is likely this yields an incomplete accounting of the resources that are associated with both the likelihood of unwanted births occurring and the repercussions of those births. Note that ideally the associated factors should be measured prior to the occurrence of the birth, because the birth itself may affect household resources, parental social and psychological states, and so forth. This is a further reason to employ longitudinal designs when investigating the consequences of unwanted fertility. Because direct measurement of the full set of hypothesized associated factors is a near-impossibility, researchers have turned to other strategies for eliminating their confounding influence, including fixed effects models (Joyce *et al.* 2000) and “natural experiments” (Rosenzweig and Wolpin 2000). We employ both of these approaches in the analysis presented in this paper. Virtually none of the existing empirical research on the consequences of unwanted fertility conducted in low-income societies has confronted head-on the serious threat to validity posed by unobserved heterogeneity.

A related theoretical issue is the extent to which the consequences of unwanted childbearing reflect deliberate quantity-quality tradeoff. Montgomery *et al.* (1997) argue that unwanted births are an exogenous (non-chosen) addition to the stock of wanted children, and hence are not endogenous to conscious quantity-quality strategizing on the part of couples. To some extent this theoretical stance relieves the analyst from the concerns about bias due to unobserved heterogeneity described in the previous paragraph. Our stance is less extreme. While we regard unwanted births as genuinely unwanted – i.e., the couple did indeed wish not to become pregnant at the time of conception – we believe that in a setting such as Bangladesh in the 1980s and 1990s, with contraception widely available, couples make deliberate decisions to place themselves at higher or lower risk of an unwanted conception after weighing the perceived costs/benefits of their recognized behavioral choices. Couples’ willingness to place themselves at higher risk of an unwanted birth is a kind of conscious strategizing about quantity of children. Hence the threat of endogeneity bias remains, and motivates the adoption of analytical strategies that reduce this threat.

Existing Research

The child outcomes examined in this paper are mortality (neonatal, post-neonatal, early childhood) and schooling, and hence we confine our review to these and closely related outcomes.

Mortality. Gipson *et al.* (2007) provide a comprehensive review of the existing literature on the effects of unwanted fertility on child health and survival. Numerous single- and multi-country studies have used DHS data to examine this topic. But these studies must rely on retrospective reporting of child wantedness which, we argued above, provides an *ex post* revised classification and, in all likelihood, yields upwardly biased estimates of the effects of a child being unwanted. Even so, in the presence of controls for other determinants of child survival, unwanted births do not consistently show higher risks of death in infancy and early childhood (e.g. Montgomery *et al.* 1997). Somewhat more consistent effects emerge for child health and its direct determinants, including antenatal care, post-natal preventive and curative care, and nutritional status (Jensen and Ahlburg 1999; Marston and Cleland 2003); but these studies too depend on the DHS retrospective measurement of child wantedness, and their estimation approaches take no account of unobserved heterogeneity that might bias the estimated effects of unwanted fertility. Apparently the only published research that directly tackles the threat posed by unobserved heterogeneity is Joyce *et al.* (2000) on the U.S.; in models with fixed effects for sibsets (which sweep away persistent family-level effects), unwanted births receive worse antenatal and postnatal care but show no difference in the one direct measure of health (birthweight). Montgomery *et al.* (1997) is distinctive in considering effects on child survival specific to unwanted births as well as effects on the entire sibset; they find more evidence for effects on the entire sibset than effects specific to the unwanted birth.

In sum, we concur with Gipson *et al.* (2007) that the existing literature does not provide a solid foundation for assessing whether or not unwanted fertility has detrimental consequences for child survival. Previous research is characterized by significant weaknesses in the measurement of child wantedness and/or in the extent to which the research design is robust to validity threats from unobserved heterogeneity. One could argue that the most revealing evidence comes not from studies that examine the effects of child wantedness *per se* but rather from studies of health and mortality differentials according to child sex and birth order (Das Gupta 1987; Muhiri and Preston 1991; Lloyd 1994; Desai 1995). But because these studies do not focus on child wantedness (i.e. many higher-order births are wanted at conception, as are many girls who are later disadvantaged postnatally), they do not provide direct estimates on the potential gains for child and family well-being from more perfect birth control.

Schooling. The existing literature on the effects of unwanted fertility on schooling is far thinner than the literature on child health and survival. The requirement of long follow-up is undoubtedly one reason for the scant literature. Greene and Merrick (2005) summarize the empirical record. Several studies in Europe have found that unwanted births have poorer educational outcomes: a follow-up of children from Czech women denied an abortion, matched to a control group of wanted births (David *et al.* 1988; David 2006); and children of Finnish women asked during their pregnancy whether it was wanted or not (Myhrman *et al.* 1995). Both studies can be faulted for inadequately accounting for confounding variables. At a higher level of aggregation, a Romanian study compares the educational attainment of cohorts born before and after changes in the availability of induced abortion, and infers from the higher attainment of cohorts born when abortion was more available that unwanted fertility detrimentally affects child schooling (Pop-Eleches 2006). Similar research could be conducted in the U.S., comparing cohorts born before and after *Roe v. Wade*, but we do not see it in the published literature. The evidence in Gruber *et al.* (1999) suggests that the Romanian results would be echoed in the U.S. In a similar vein, Foster and Roy (1997) compare children in the treatment and control areas of the Matlab family planning experiment in Bangladesh, and infer from the greater educational attainment of children in the treatment area that reduction of unwanted fertility has beneficial effects on child schooling. Finally, Montgomery *et al.* (1997) make clever use of DHS data to estimate the impact of the birth of an unwanted younger sibling on the schooling of his/her older sibling. They find a significant negative impact in the Dominican Republic and the Philippines but not in Egypt and Kenya. These authors are highly sensitive to the threat posed by unobserved heterogeneity and address this via a two-equation model that includes an equation for whether or not fertility is unwanted, with appropriate identifying exclusions. This is a commendable effort to deal with omitted variable bias but hinges, of course, on the adequacy of the identifying variables. Note too that Montgomery *et al.* are dependent on the DHS retrospective measurement of unwanted fertility.

In short, we can locate few studies of the consequences of unwanted fertility for child schooling, and the bulk of these are European. We are aware of just one micro-level investigation in developing countries – Montgomery *et al.* (1997) – and this study does not consider consequences for the unwanted child, rather examines only cross-sib effects.

SETTING, DATA AND METHODS

Setting

Rural Bangladesh is the setting for this research. Bangladesh is the 7th most populous nation and one of the most densely populated regions in the world. Given the country's agrarian profile, with only 23% of the population living in urban areas even recently (Population Reference Bureau 2006), the rural setting of our analysis is entirely appropriate. Accompanying the issues of population density and the predominance of agriculture are a host of developmental issues. With over 50 million people still living under a \$1 a day, improving maternal health, decreasing child malnutrition, and addressing the quality of education were identified as urgent goals in a 2005 joint report from the Bangladeshi government and the UN. Within this larger context are the two contrasting districts of Jessore and Sirajgonj where the data analyzed here were collected.

Jessore district lies in the south-western Khulna division which is geographically contiguous with the state of West Bengal in India. Jessore has consequently benefited from the exports of agricultural products like cotton, jute and fruit to India as well as from trans-border informal trade. It has also experienced greater urban growth than most other regions in the country (Afsar 2003). Sirajgonj, on the other hand, lies in the more remote north-central division of Rajshahi. Its location – adjacent to a principal river, Jamuna – makes it prone to severe flooding during the monsoons, resulting in the disruption of transportation and communication systems. The north-central region is worse off than the south-western region on several counts – greater prevalence of poverty (62% v. 52%), lower adult literacy rates (35% v. 47%), lower immunization rates (54% v. 81%), and higher infant mortality (8% v. 7%) to name a few (Bangladesh Human Development Report 2000). It is important, especially for this study, to note the differences in women's status between the two districts. Women have lower literacy rates in Sirajgonj, lower rates of contraceptive use (Amin *et al.* 2002), and have to adhere to norms of *purdah* more strictly which limits their mobility as well as their interaction with men (Balk 1994; Koenig *et al.* 2003).

Data

The data used in this paper were collected under the Maternal and Child Health & Family Planning (MCH-FP) Extension Project of the International Centre for Diarrhoeal Disease Research, Bangladesh (ICDDR, B). The information on births and deaths between 1982 and 2002 come from the Sample Registration System (SRS) which was put into place in the Jessore and Sirajgonj districts. While the Demographic Surveillance System (DSS) in the Matlab study area is considered the largest and longest-running longitudinal surveillance of a population geared towards studying demographic and health issues (INDEPTH Network 2002), the SRS was established with the very specific aim of evaluating the effects of the MCH-FP Extension Project. The project focused on innovations in the delivery of health and family planning services (Mozumder *et al.* 1990) so at the core of the SRS is a database of

demographic events in households, contraceptive use, and the provision of health and family planning services to women of reproductive age. These data were updated four times a year during the twenty-year observation period. The information on child's schooling also comes from the SRS. Although the availability of education histories would have been useful, the quarterly updating in the SRS yields a single measure of education, total number of years completed at the last observation of the child.

The area of SRC coverage is comprised of six *thanas/upazilas* (administrative subdistricts consisting of a population of roughly 200,000 each). Two of these were the site of a health and family planning intervention: Abhoynagar (a *thana* in the Jessore district) and Sirajgonj (a *thana* in the Sirajgonj district). These were matched with two *thanas* that served as comparison areas: Fultala (in Jessore) and Gopalpur (in Sirajgonj), both of which adjoin the intervention areas. In 1986, two additional *thanas*, Keshobpur and Bagherpara (both in the Jessore district), were incorporated into the SRS to evaluate the impact of increasing fieldworker-to-client staffing ratios. The surveillance system is based on a 2-stage cluster sampling design, first of unions within *thanas* (unions are subdivisions with a population of approximately 20,000 each), and then of households within unions. The data are considered to be very accurate due to the regular monitoring of eligible women. Mozumder *et al* (1990) provide an excellent description of the design of the SRS and the reliability of the data.

While the SRS provides a basic record of demographic events, the information on fertility preferences comes from separate in-depth surveys of all the reproductive-aged women in the SRS. These Knowledge, Attitudes and Practice Surveys (KAP) collected detailed baseline socio-demographic information, and asked about the women's health and reproductive knowledge, attitudes, and behavior. The surveys were conducted in 1982-83, 1985-86, 1990, 1993 and 1998 in those *thanas* that were under active surveillance at the time. The design is a bit complicated and is shown visually in Table 1.

Measures

This section describes the main variables used in the analyses. The description of the outcome variables is followed by a discussion of the wantedness variables.

Outcomes. For mortality, we adhere to widely accepted definitions of infant and child mortality as they are laid out in the International Classification of Diseases - 10 (WHO 1992).

- (i) Neonatal mortality is defined as the death of a liveborn infant within the first 28 days.
- (ii) Postneonatal mortality is defined as the death of a live birth after 28 days but within the first year of life.
- (iii) Early childhood mortality is defined as the death of a child older than 12 months but younger than 60 months.

Children's educational attainment is measured in total years of schooling completed. In the Bangladeshi school system, primary school is defined as grades one through five, and secondary school is defined as the next five years, grades six through ten. Grades eleven and twelve are known as higher secondary. Grades ten and twelve are important benchmarks because of the nationwide examinations that take place at the end of those grades. In the analysis, years of schooling completed is modeled as a function of child's age at last observation. Note that variation in years of schooling conditional on age will reflect delayed entry, grade repetition, and termination of schooling. Delayed entry and grade repetition are both relatively common in rural Bangladesh (Amin and Sedgh 1998, Arends-Kuenning and Amin 2000).

The education sector in Bangladesh underwent significant changes intended to increase enrollment during the observation period for this study.¹ Schools were nationalized post-independence (1971), and primary completion rates increased sharply beginning with cohorts entering school in the late 1970s and completing primary school in the mid- and late-1980s. Throughout this period, at the primary level government schools (which are the majority of primary schools) required no tuition and provided books at no cost. There were, however, modest primary school fees for examinations and activities. In the mid-1980s, secondary school fees for girls (but not boys) were eliminated, reducing the direct financial costs of secondary schooling. Note that these changes precede the schooling of the birth cohorts analyzed in this research. However, despite these efforts to facilitate school attendance, enrollment remained relatively low (e.g. national primary school completion rates under 50%) until the late 1980s.²

Of more direct relevance to this research are three developments in the 1990s. In 1994, two national incentive schemes were launched. Under the Food for Education Program, poor families that met a certain set of criteria received designated amounts of wheat in return for sending their children to school. (Amin and Sadgh (1998) estimate that the wheat ration amounted to 7% of average monthly income in rural areas.) Under the Female Stipend Program, families were offered monetary incentives (deposited in bank accounts) for sending their girls to secondary school. (Amin and Sadgh (1998) indicate that the financial value of this program was about one-half the value of the Food for Education Program.) The long-term goal of this program was to delay marriage among girls and reduce fertility. While incentives for secondary school were the central element, the program had an auxiliary positive effect on primary schooling as well. A third development was the growth in the 1990s of BRAC

¹ This discussion relies heavily on Amin and Sedgh (1998).

² One explanation is that the direct financial costs (putting aside the opportunity costs) to parents of schooling remained substantial, as demonstrated empirically by Amin and Sedgh (1998) using 1996 data. The largest cost is clothing – social norms require better and different clothing for children going to school – followed by books and supplies, for which there are household-level expenditures despite government policy to the contrary.

(Bangladesh Rural Advancement Committee) primary schools. BRAC is a large national nongovernmental organization. As of 1995 there were 30,000 such schools nationwide. These schools gave preferential enrollment to girls and children from poor families. On the whole, the mid-1990s marked a significant departure from previous trends in schooling. Our regression specification takes explicit account of this historical development.

Child wantedness. The fundamental assumption underlying the classification of births as either wanted, unwanted, or “up to God” is that a woman’s stated fertility preferences at an earlier date validly represent her feelings at the time of subsequent conceptions. In the KAP surveys, women were asked how many further children they wanted of each sex, variables that we term “prospective preferences”. With this information, we assign births that follow the survey into one of three wantedness categories: wanted, “up to God”, and unwanted. Because the questioning is sex-specific, we are able to construct two variables. One we label “general wantedness”; it has categories:

- “wanted” if either another boy or another girl is wanted
- “up to God” if the question about another boy and another girl both received this response
- “unwanted” if neither a boy nor a girl is wanted.

The second we label “sex-specific wantedness”; it has categories:

- “wanted” if a child of that sex is wanted
- “up to God” if the question about another child of this sex received this response
- “unwanted” if a child of that sex was not wanted

Hence under “general wantedness” a birth is classified as unwanted if the woman indicated that she wanted neither additional sons nor additional daughters, whereas under the sex-specific version a birth is classified as unwanted if the woman indicated that she did not want additional children of the child’s sex; she may, or may not, have indicated a desire for additional children of the other sex. The general construction captures aversion to any increase in family size; of the two wantedness variables, the general version more purely captures the woman’s concerns about quantity of children. In contrast, the sex-specific version captures aversion to the sex in question that may or may not be accompanied by quantity concerns; as such, effects of this measure mix concerns about family size with sex preference.

Further details about construction of these two key explanatory variables are provided in the Appendix

The assignment procedure hinges on several key assumptions, namely stability of fertility preferences over time (i.e. from the survey interview to the conception of the birth), and comparability of the pertinent items in the surveys. There is empirical evidence that calls into question the second

assumption: the percentage of women who state that whether or not they should have another birth is “up to God” is markedly higher in the 1982/83 and 1985/86 surveys (roughly 25%) than in the three surveys conducted in the 1990s (roughly 2%). It seems unlikely that this sharp drop reflects true changes in sentiment, rather is explained by change in the questionnaire design (but these are slight) and/or in the behavior of interviewers. We handle this data problem by retaining “up to God” as a valid response when it is given, and by including a control for historical period in all equations.

Some births cannot be classified on the basis of prospective preferences because the birth occurred before the woman was first interviewed in a KAP survey. It is common, for example, for women to move into the surveillance system at the time of marriage and have a birth relatively soon thereafter, before her first KAP interview. We have assumed that all first births that lack information on prospective preferences are wanted. By this rule we classify an additional 4090 births, or 18.6% of the total births available for this analysis. This imputation is discussed in more detail in the Appendix.

Methods

We examine two child outcomes: mortality before age 5, and years of schooling completed. We present the methods for each outcome in turn.

Mortality. The risk of mortality before age 5 is examined in three segments: neonatal mortality (first four weeks), post-neonatal mortality (5-52 weeks), and early childhood mortality (months 13-60). We model the hazard of a child dying using a discrete-time approach in which each child’s observed exposure to risk is segmented into time units that serve as the observation in the regression estimation (Allison 1982; Box-Steffensmeier and Jones 2004). The dependent variable is a binary indicator of whether or not the child died in the time unit, and we estimate logistic regressions. The time units are days for neonatal mortality, weeks for post-neonatal mortality, and three-month segments for early childhood mortality. Censored observations are included. Duration dependence is captured via linear and quadratic terms (with units days, weeks, or three-month segments). Control variables are: sex of the child, birth year of the child, birth order (continuous term, plus indicator for first birth), maternal and paternal education (years of schooling completed), whether the household head is Muslim, and thana.³

Years of schooling. We model the cumulative years of schooling completed as of the last observation of the child. We estimate Poisson regressions (Cameron and Trivedi 1998).⁴ Obviously

³ There is a further control for whether or not the wantedness of the child was determined from measured prospective preferences or imputed on the basis of the assumption that all first births are wanted.

⁴ The chi-square test of the overdispersion parameter in negative binomial regressions revealed no significant overdispersion in the data, making the Poisson an appropriate model.

years of schooling is highly dependent on the age of the child at last observation; this can be viewed as amount of exposure to the opportunity to attend school and is included as a right-hand-side variable, represented by both linear and quadratic terms. Only children six years and older are included in this analysis. Other control variables are: sex of the child, birth year of the child (before 1988 or after 1987, to take account of the launch in the mid-1990s of major programs to boost school attendance, especially on the part of girls), birth order (continuous term, plus indicator for first birth), maternal and paternal education (years of schooling completed), whether the household head is Muslim, and thana.¹

Distributions of the outcomes and all the explanatory variables are shown in Table 2.

We adopt two strategies for minimizing the threat to validity posed by unobserved heterogeneity: (i) Models with *fixed effects* for sibset; (ii) Estimation confined to a sample in which wantedness is solely a function of sex of the child and therefore randomly assigned, i.e. a *natural experiment* (Angrist and Krueger 2001). Both strategies can be viewed formally as instrumental variables approaches: in both, the goal is to purge the child wantedness variable of confounding variation from unobserved variables. We present each strategy in turn.

Fixed effects. Our model is:

$$\ln[Y_{ij}] = \alpha + \sum \beta_k X_{ijk} + \delta W_{ij} + \mu_j + \varepsilon_{ij} \quad (1)$$

where

- ln is the natural logarithm operator
- Y odds of child dying or years of schooling complete
- X_k vector of control variables
- W indicators of child wantedness
- α, β_k, δ regression coefficients to be estimated
- μ, ε randomly distributed disturbances
- i denotes individual child
- j denotes sibset

Note that δ is the coefficient of interest in this research – this is the estimated effect of child wantedness. In the fixed effects analysis consequences of both general and sex-specific wantedness can be estimated.

In equation (1), μ_j is the fixed effect for sibset. With this term in the equation, effects of explanatory variables X and W are based on within-sibset variation only. That is, δ is derived by comparing the experience of wanted and unwanted siblings in the same family. What this achieves is a purging from the parameter estimates of the confounding influence of sibset characteristics (observed or unobserved) that uniformly affect all sibs. The characteristics of key concern are assumed to be parental

and household, but could also include characteristics of the extended kin group and local community. The assumption that the effects of these characteristics are uniform within the sibset is also effectively an assumption of time-invariance of the effects, given that sibs risks of mortality and schooling do not occur simultaneously but rather unfold over time.

One consequence of adoption of the fixed effects model is that only sibsets with variation on the outcome – mortality or years of schooling – can contribute to this estimation. This significantly reduces the size of the sample (compare the number of observations in Table 3 and Table 4/5.) We make further comments on the reduced sample below. Estimation of equation (1) is via conventional fixed effects estimators (Woodridge 2002; Cameron and Trivedi 1998), as implemented in Stata version 9.2.

Natural experiment. The Bangladeshi women were asked separately about their desired number of additional sons and daughters. Consider women who indicate a desire for additional children of one sex but not the other. For this subset of women, the wantedness of the child is assigned randomly, because sex of child is assigned randomly.⁵ Our model for analysis of this subset of women is:

$$\ln[Y_i] = \alpha + \sum \beta_k X_{ik} + \delta W_i + \varepsilon_i \quad (2)$$

where all terms are as defined above for equation (1).

Again δ is the coefficient of interest. Under the natural experiment approach, only the sex-specific version of the child wantedness classification W is relevant.

The child's sex can be viewed as an instrumental variable for child wantedness. For it to serve this purpose, it must satisfy the essential criterion for an instrumental variable, namely that it is a determinant of the endogenous explanatory variable (W) but is not a direct determinant of the outcome (Y). But of course the child's sex can have substantial bearing on parental investment, and hence it is critical that the child's sex be included among the vector of control variables X . In principle the random assignment of child wantedness obviates the need for any other control variables. Nevertheless we include a full set, making for a more stringent test (because of loss of efficiency, if nothing else). Estimation of equation (2) is via conventional logit and Poisson algorithms, as implemented in Stata version 9.2.

Because a woman must want another birth of one sex but not the other to be eligible for this analysis, taken at face value the wantedness variable W captures sex preference only, unencumbered by

⁵ This assumes no sex-selective induced abortion. Whereas induced abortion, under the euphemism “menstrual regulation”, was relatively commonly practiced in Bangladesh during the study period, there is no evidence of widespread use for the purpose of sex selection.

concerns about the quantity of children. This presumes a relatively simple logic to fertility preferences, however. Quantity and sex composition might well interact in a complex manner. For example, the desire to have a second daughter might be affected by whether a woman already has no sons vs. one son vs. two or more sons.

We are able to conduct this “natural experiment” because of a confluence of societal setting and data measurement: child wantedness is highly gendered in Bangladesh, and in the ICDDR,B KAP surveys, fertility preferences were measured on a sex-specific basis and prospectively. We are unaware of any other data that contain this same combination of features.⁶

Sample selection and generalizability of the results. The fixed effects model and the “natural experiment” approach are both powerful strategies for recovering valid estimates of the consequences of child wantedness. As compared to virtually all entries in the existing literature on low-income societies, these strategies give us more secure standing to assert that true causal effects have been obtained, i.e. the impact of eliminating unwanted births. Both strategies, however, make use of a small portion of the variation offered by this Bangladesh sample of children. In terms of the sample of births, out of the total sample of 21920 births, the fixed effects analysis makes use of 3283 births (15.0%) and the natural experiment analysis makes use of 5576 births (25.4%).⁷ To be sure, this is a far larger number of observations than in Joyce *et al.* (2000), the other contribution in the existing literature that employs the fixed effects approach; their full sample varies between 7429 and 7751 (depending on the model) and their fixed effects sample varies between 560 and 1461. Nevertheless, it is important to pause to reflect on the limits this sample selection places on the generalizability of the results (or, put otherwise, whether the causal effects of ultimate interest are in fact recovered). The cautions of Rosenzweig and Wolpin (2000) about the validity of inferences drawn from “natural experiments” also apply to the fixed effect model.

Considering first the model with fixed effects, in these models the estimation of the effect of child wantedness is based solely on within-sibset variation. To the extent that between-sibset variation might be informative, there is clearly a loss of statistical efficiency in employing a model with fixed effects. Worse, there is also the risk of inconsistent estimates. How might this occur? Sibsets with no variation on the outcomes (mortality, schooling) are dropped from the analysis. Suppose parents who are most willing (and most able) to invest in their children, as well as more disciplined in their

⁶ A number of pieces have treated child sex (or sex composition of children) as an instrumental variable for quantity of children, with no distinction made between the quantity of wanted and unwanted fertility. See Iacovou (1996), Angrist and Evans (1998), and Lee (2004). More commonly the occurrence of twins has been employed as an instrument for quantity of children, again with no distinction between wanted and unwanted fertility. Glick *et al.* (2006) are unusual in using the occurrence of twins as an instrument for the quantity of unwanted births.

⁷ These are observations for the analysis of neonatal mortality.

commitment to achieving this, are far more likely to have only wanted births and, in part for this reason, far less likely to have any of their children die before age five. One might view these families as occupying one end of the spectrum in terms of both child wantedness and child well-being. That is, the sample is truncated on the key explanatory and outcome variables, and, as is well established, sample truncation often results in inconsistent estimation of regression parameters. Apart from this concern, clearly this analysis is incapable of detecting “spillover effects” of an unwanted birth, either on the household as a whole or on specific household members (the mother, or wanted siblings). It is frequently posited, for example, that younger unwanted sibs detract from the schooling of older sibs, especially older girls (Lloyd 1994).

In the case of the natural experiment analysis, child wantedness is determined entirely by sex of the child and is unrelated to total family size.⁸ If the consequences of excess childbearing are of interest, and surely they are, then they can be inferred from this analysis only if one assumes that they are the same as the consequences suffered by children of the wrong sex. It seems unlikely this assumption holds, but the extent of departure is unknown. Furthermore, it is also not clear whether women in the natural experiment sub-sample whose birth is of the wrong sex also have quantity concerns that are, as compared to the sample as a whole, relatively higher or lower or essentially the same (conditional on birth order of the child). On the one hand, these women have expressed a desire to have another child; implicit in this desire is a felt-capacity to accommodate another child, whether boy or girl. On the other hand, their receptivity to having a child of one sex but not the other is revealing of quantity concerns, certainly as compared to women who are prepared to accept a child of either sex.

For further perspective on the portion of full sample variation represented by the sub-samples for the two analyses (fixed effects and natural experiment, respectively), we have examined distributions of the full sample and the two sub-samples according to key measured variables: wantedness, birth order, sex, and child wantedness composition of the sibset. (Tabulation available from authors.) The most notable differences are between the sub-sample for the natural experiment analysis and the other two samples. In the natural experiment sub-sample the percentage unwanted is about ten points higher than in the other two samples (50% vs. approximately 40%). First births are quite rare in the natural experiment sub-sample, as against one-quarter to one-third of births in the other two samples. Unwanted births are less likely to be male in this sub-sample. Finally, births in the natural experiments sub-sample are less likely to have their complete sib-set present in the data (before

⁸ Note, however, that this assertion is not strictly correct if sex-specific desires for another birth vary by family size conditional on the number of children of the sex in question. This is as illustrated above: suppose, for example, that the desire to have a second daughter is conditional on whether the woman already has one, two, or three sons. If so, sex-specific desires are determined not only by the number of children of that sex but also by the total number of children.

selection of the sub-sample), and conditional on birth order a somewhat higher fraction of the sibs are unwanted. All of these differences follow naturally from the rule determining eligibility for the natural experiment analysis, namely that an additional child of one sex is desired but not a child of the other sex.

RESULTS

Mean levels of the child outcomes (mortality and schooling) are shown for each category of child wantedness in Table 3. There are two panels in this table, for the general and sex-specific wantedness measures, respectively. As compared to wanted children, unwanted children show lower neonatal mortality and higher mortality thereafter. The highest mortality is experienced by those children who were “up to God”. The differentials in years of schooling are more clearly patterned: wanted children complete somewhat more schooling on average than children who are “up to God” or unwanted.⁹ We draw no important conclusions from the differentials in Table 3, as these do not account for numerous associated measured and unmeasured explanatory variables (for example, birth order of the child).

The regression estimates are presented in Tables 4 and 5, for the mortality and schooling outcomes, respectively. These tables show the parameter estimates for the effects of child wantedness only (δ in equations (1) and (2)), as these are the estimates of interest in this research.

Beginning with the results for mortality (Table 4), the fixed effects analysis yields estimates of excess mortality of unwanted births during infancy that are large and statistically significant. The effects on early childhood, in contrast, are small in magnitude and do not test as significant. The effects on neonatal and post-neonatal mortality are of roughly the same magnitude whether child wantedness is measured on a general basis (top panel of Table 4) or a sex-specific basis (middle panel of Table 4). In both instances, the odds ratio is on the order of 2.00 for the probability of an unwanted child dying as compared to a wanted child. The parameter estimates are far smaller in the natural experiment analysis (bottom panel of Table 4) and not statistically significant.

Considering the patterning of the estimated effects on mortality in Table 4 -- effects evident in infancy but not early childhood, and evident when excess fertility is in play but not when unwantedness is determined entirely by the sex of the child – one is directed to causal mechanisms that operate either during pregnancy (when the sex of the child is not known) or in the immediate post-partum period. Considering first pregnancy, the woman’s nutritional status may be a factor, as well as sub-standard

⁹ Note that the schooling means are standardized on child age.

antenatal care (later first visit, fewer total visits). Marston and Cleland (2003) find some evidence of later timing of the first antenatal appointment when the pregnancy is unwanted. They hypothesize that unwanted pregnancies are recognized and/or acknowledged later, and this may result in poorer nutrition and care during the pregnancy. There are also multiple plausible mechanisms during the post-partum period, in particular preventive and curative care. Most notable among the former is tetanus toxoid immunization, which should be administered between four and fourteen days post-partum. Other research has shown this to be a powerful determinant of infant survival in Bangladesh. Measles and other standard vaccinations could also bear on survival chances. Curative care in response to child illness leaves ample scope for parental choice, as influenced by the wantedness of the child. More generally, one must entertain the possibility of selective neglect of unwanted children in the first months of infancy, however deliberate or inadvertent. We are not aware of descriptions of child neglect of this kind in Bangladesh, but ethnographic research offer vivid portraits of relatively conscious neglect in several Latin American settings (Scrimshaw 1978; Scheper-Hughes 1984, 1992). In these ethnographies, this neglect is motivated most of all by overwhelming resource deprivation at the household level. Indeed, one might take women's prospective expressions of desires to have no further births as a marker that their households do not possess the resources to accommodate another child.

It should also be underscored that the estimates from the fixed effects models are based entirely from within-sibset variation. That unwanted children are more likely to die than their wanted sibs indicates that the costs of an unwanted fertility are not shared rather equally by all children (as certain equity norms might imply), rather are borne disproportionately by the unwanted child.

Note that, whatever the mechanisms underlying the substantial effects of unwantedness on mortality evident in Table 4, they are not gendered. There are multiple pieces of evidence to this effect. First, the parameter estimates are far smaller (and not statistically significant) in the natural experiment analysis (Table 4), in which child wantedness is determined entirely by child sex. Second, we have re-estimated the regressions of Tables 4 and 5 with interactions between child sex and child wantedness added; none of these interactions test as significant. That is, the magnitude of the effect of unwantedness on mortality does not differ by sex. Third, all the regressions include a dummy variable for sex of child. The estimates indicate that in the neonatal and post-neonatal period males are at higher risk of death. Only in early childhood do females suffer higher mortality.

Turning to the consequences of child wantedness for schooling (Table 5), significant effects emerge in both the fixed effects analysis and the natural experiment analysis. The effect is somewhat sharper and attains a higher level of significance in the natural experiment analysis. The coefficients represent ratios of mean number of years of schooling completed (for the wantedness category in

question as compared to wanted births). The estimates indicate that unwanted children experience 5%-9% fewer years of schooling, conditional on their age (and other control variables). This relative shortfall of course implies an increasing absolute shortfall as children age.

Interestingly, children with wantedness status “up to God” complete even less years of schooling (Table 5, top panel only), although the point estimates for this category are not statistically different from the point estimates for the unwanted children. When it comes to schooling, the key distinction is between children who were explicitly wanted and other children, whereas mortality differs most between unwanted children and the other two categories. We do not offer an interpretation of this contrast in the results for mortality and schooling. This requires, we believe, some investigation of the meaning of the “up to God” response.

We do not place much weight on the fact that the estimated effects of child wantedness are somewhat larger in the natural experiment analysis (Table 5, bottom panel) than in the analysis with fixed effects (Table 5, top panel). The differences in the coefficients are slight, and this is the point that deserves emphasis: close agreement in estimates obtained from the application of two distinct strategies gives one greater confidence in the validity of the results.

SUMMARY AND CONCLUDING COMMENTS

For the purposes of assessing the consequences of unwanted fertility, the Bangladesh data possess three important attributes: child wantedness is measured prospectively, thereby avoiding biases due to *ex post* revision that probably plagues retrospective measurement (e.g. as in the DHS); child wantedness is measured on a sex-specific basis, crucial in societies such as Bangladesh where fertility desires are highly gendered; and the follow up is lengthy, beyond age 15 for some children, affording the opportunity to consider effects on late childhood and adolescent outcomes such as educational attainment. The data design also permits the application of two stringent tests of the causal impact of child wantedness, namely regression models with fixed effects for sibsets (thereby eliminating the confounding influence of persistent parental and household factors), and a “natural experiment” in which wantedness is assigned randomly to a subset of children via the mechanism of random assignment of sex of the child (thereby in principle eliminating the confounding influence of any other factors).

Under both of these stringent tests, significant effects of child wantedness are estimated. Large effects on infant mortality (both neonatal and post-neonatal) emerge under the fixed effects approach,

with odds ratios on the order of 2.0. The natural experiment analysis does not yield effects of equivalent magnitude, a disconcerting contradiction in the results. The estimated effects on schooling are more consistent across the two approaches: unwanted children attain on average 7%-9% fewer years of schooling than wanted children.

We are largely in the dark about the mechanisms that generate these effects. The effects on infant mortality are based on within-sibset comparisons – unwanted children suffer higher mortality rates than their wanted sibs. This suggests differential parental investment. But one might expect differential investment to be of limited importance in the months immediately after birth, certainly as compared to older ages. Perhaps the crucial mechanisms have to do with maternal behavior during pregnancy and post-partum, and these in turn may reflect deliberate under-investment in an unwanted pregnancy or, alternatively, the largely unavoidable choices of resource-constrained households.

Consequences for child schooling, in contrast, play out when the child is older and, presumably, reflect conscious parental decisions. The effects are apparent in within-sibset comparisons (fixed effects approach) and in estimates that derive from both within-sibset and between-sibset variation (natural experiment approach). There are direct financial costs of schooling in Bangladesh (fees, books, clothing, better nutrition), starting at the primary level and increasing sharply at the secondary level (Amin and Sedgh 1998; Arends-Kuenning and Amin 2000). There are also the opportunity costs of forgone labor, especially in the case of boys (who have more wage-earning potential). Hence parents have incentives not to school their children or to invest strategically in the schooling of a few children.

Yet as a result of the government and nongovernmental programs designed to encourage school attendance described earlier in this paper, the financial and non-financial costs of child schooling were probably less in Bangladesh as of the 1990s than in most low-income countries. These programs demonstrably reduced the direct financial costs of primary schooling for both sexes and of secondary schooling for girls (Amin and Sedgh 1998).¹⁰ Despite these policies and programs, many of the children in this rural sample apparently did not complete primary schooling, indeed many never entered school, and differentials according to child wantedness are of some magnitude (net of the sex and birth order of the child). We infer that this must reflect a parental perception of meaningful costs of schooling, monetary and/or non-monetary.

If unwanted fertility has detrimental consequences for child schooling in a society where there has been a determined effort to achieve universal primary schooling and to minimize the costs of schooling borne by households, one might guess that the consequences are larger in magnitude in

¹⁰ Amin and Sedgh (1998) also show that the time costs of schooling are relatively low in Bangladesh, because of the short school-day and school-year.

societies where universal schooling has not been pursued with such determination and households shoulder far larger child-by-child costs. At the same time, the prevalence of poverty in rural Bangladesh may result in sharper effects on infant mortality than would be observed in less resource-constrained environments. Investigating these speculations requires child cohort studies in multiple settings with relatively long-term follow-up and good measurement of fertility preferences, and for now these are rare. Hence this Bangladesh study may remain a relatively singular contribution for some years to come.

Determining whether the effects of child wantedness evident in rural Bangladesh are larger or smaller than effects in other settings is one item on the research agenda. A second important item is more comprehensive exploration of the locus of the effects. This analysis has considered consequences for the unwanted child only. Consequences for the child's siblings and parents are entirely plausible, and indeed in analysis of the sort conducted in this paper (some of which hinges on within-sibset comparison) such spillovers can disguise effects on the unwanted child. The Bangladesh data afford some opportunity to investigate cross-sib effects, and we will be turning to this next.

APPENDIX

Construction of the Child Wantedness Variables

As indicated in the text, on the basis of the prospective preferences information gathered in the KAP surveys we construct two indicators of child wantedness: “general wantedness”, with categories

- “wanted” if either another boy or girl are wanted
- “up to God” if the question about another boy and another girl both received this response
- “unwanted” if neither a boy or girl are wanted.

and “sex-specific wantedness”, with categories

- “wanted” if a child of that sex is wanted
- “up to God” if the question about another child of this sex received this response
- “unwanted” if a child of that sex was not wanted

The construction of these two variables is straightforward from information on:

1. Whether another boy is wanted, not wanted, or “up to God”.
2. Whether another girl is wanted, not wanted, or “up to God”.

The following examples illustrate the logic underlying the classification.

- (a) If a woman wanted neither a boy nor a girl, the subsequent children are “unwanted” on both general wantedness and sex-specific wantedness.
- (b) If a woman wanted two boys but no girls, her next boy is “wanted” on both counts but the following girl is “wanted” only on the general wantedness variable. If she then has another boy, he is again “wanted” on both counts because the girl did not count towards the 2 boys that she wanted.
- (c) A woman says that the number of boys is “up to God” but she does not want any girls. She then has a girl, who is classified as “unwanted” on the sex-specific variable but “up to God” on the general variable.
- (d) A woman says that she wants a boy but that the number of girls is “up to God”. She then has a girl, who is classified as “up to God” on sex-specific wantedness but “wanted” on general wantedness. Subsequent girls are “up to God” on both counts because the desired number of children is now fulfilled (although desired sex-composition is not).

In short, for assigning general wantedness this logic places the strongest burden of proof on “unwanted” – this must be the clear assignment on her preferences for both sexes – and the weakest burden of proof on “wanted” – as long as she wants a child, a child of either sex will be “wanted”.

The default is to use prospective preferences reported in the most recent survey preceding the conception of the birth. That is, in the case of births from pregnancies current (and recognized) at the time of a survey, classification is based on prospective preferences in the previous survey. If, however,

prospective preferences cannot be ascertained from the survey most immediately preceding the conception of a birth – either because women were not interviewed or because their responses cannot be categorized as “want”, “do not want”, or “up to God” – we proceed backward to an earlier survey. This continues until a usable survey response is located or all surveys have been exhausted, in which case no assignment is made.

We also impute some of the assignments on the basis of birth order. While a vast majority of the births in our data are classified according to the above logic, prospective information on fertility preferences is not available for some births. This occurs when the household enters the SRS at some point and the woman has a birth before she is interviewed in a KAP survey. This is especially likely to occur in the case of first births, because women may have moved into the SRS area at the time of marriage. We make the assumption that first births are wanted -- in Bangladesh, as in most societies, virtually all married adults want to have at least one child. Our data confirm this, as only one percent of first births (boys and girls) are unwanted according to the prospective preferences logic described above.

There is the question, however, whether these births are wanted or “up to God”. According to our tabulations, on average 11 percent of first births occur to women who, in the previous survey, replied “up to God” when asked about their desire to have a child. Hence, one can correctly classify all first births for an analysis of the effects of a child being unwanted, but the correct classification is uncertain if one wishes to go further and make the distinction between wanted and “up to God”. Given that the fundamental comparison in this study is between children that are unwanted and the rest, we argue that the dilution of the “wanted” category with the small percentage of “up to God” births does not pose a significant threat to the confidence we have in our estimates. Hence, first births for which prospective preference information is not available are assumed to be “wanted” on both wantedness variables. This results in a reclassification of 4090 births in the entire sample, an addition of 22.8%.

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Table 1. Timeline of the SRS and the KAP Surveys in Jessore and Sirajgonj district

Site/year	82/83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	00	ongoing
Jessore																			
Abhoynagar	K ₁		K ₂					K ₃			K ₄					K ₅			
Fultala	K ₁		K ₂					K ₃											
Bagherpara				K ₂				K ₃			K ₄								
Keshobpur				K ₂				K ₃			K ₄					K ₅			
Sirajgonj																			
Sirajgonj	K ₁		K ₂					K ₃			K ₄								
Gopalpur	K ₁		K ₂					K ₃											

Notes: K₁ = first KAP survey; K₂ = second KAP survey... K₅ = fifth KAP survey

Table 2. Descriptive Statistics for the Total Sample

Variable	Jessore	Sirajgonj
Number of children	13716	8204
<i>General wantedness</i>		
Wanted	0.71	0.48
Up to God	0.08	0.20
Unwanted	0.21	0.32
<i>Sex-specific wantedness</i>		
Wanted	0.57	0.38
Up to God	0.08	0.20
Unwanted	0.35	0.42
<i>Demographics</i>		
Boys	0.51	0.51
Age (mean years)	7.02	3.99
Birth order (mean)	2.65	3.89
First births	0.37	0.24
Mother's education (mean years)	2.61	1.02
Father's education (mean years)	3.36	2.10
Muslim	0.83	0.97

Table 3. Differentials in Mortality and School Attainment by Child Wantedness

Wantedness measure^a and Outcome	Wanted	Up to God	Unwanted	Number of children
<i>General wantedness</i>				
Mortality ^b				
Proportion neonatal death	0.066	0.072	0.060	21920
Proportion postneonatal death	0.042	0.061	0.049	20321
Proportion early childhood death	0.026	0.055	0.043	17673
Schooling				
Years schooling completed ^c	2.40	1.96	1.90	8626 ^d
<i>Sex-specific wantedness</i>				
Mortality ^b				
Proportion neonatal death	0.070	0.071	0.056	21920
Proportion postneonatal death	0.043	0.061	0.046	20321
Proportion early childhood death	0.027	0.056	0.037	17673
Schooling				
Years schooling completed ^c	2.43	1.98	2.02	8626 ^d

Notes:

a. See text for definitions of wantedness measures

b. Kaplan-Meier estimates

c. Standardized for differences in age distribution among wantedness categories by obtaining adjusted values after fitting a linear regression with child wantedness and age as explanatory variables

d. Sample limited to children under observation until at least age six

Table 4. Effects of Child Wantedness on Mortality

Wantedness measure^a and Outcome	Wanted	Up to God	Unwanted	Number of children	Number of sibsets
Fixed effects models ^{b,c}					
<i>General wantedness</i>					
Neonatal death	1.00	1.10	2.09***	3283	1008
Postneonatal death	1.00	1.1	2.00***	2990	826
Early childhood death	1.00	0.99	1.38	1740	486
<i>Sex-specific wantedness</i>					
Neonatal death	1.00	1.09	1.66***	3283	1008
Postneonatal death	1.00	1.27	2.20***	2990	826
Early childhood death	1.00	0.97	1.27	1740	486
Natural experiment sample ^{d,c}					
<i>Sex-specific wantedness only</i>					
Neonatal death	1.00	-	1.18	5576	-
Postneonatal death	1.00	-	1.28	5314	-
Early childhood death	1.00	-	0.92	5135	-

Notes:

a. See text for definitions of wantedness measures

b. Odds ratios from fixed effects logistic regressions applied to discrete-time data. The time unit is day for neonatal, week for postneonatal, and three months for early childhood. Duration dependence of the hazard is modeled using linear and quadratic terms

c. Other variables in regression are sex of child, year child was born, birth order, whether child was the first live birth, mother's education, father's education, whether household head was Muslim, thana, and whether the child was classified under the 'first birth' rule

d. Odds ratios from logistic regressions applied to discrete-time data

* p<.05; ** p<.01;*** p<.001

Table 5. Effects of Child Wantedness on Schooling

Wantedness measure^a and Outcome	Wanted	Up to God	Unwanted	Number of children	Number of sibsets
Fixed effects models ^{b,c}					
<i>General wantedness</i>					
Years schooling completed	1.00	0.90*	0.95	4175	1782
<i>Sex-specific wantedness</i>					
Years schooling completed	1.00	0.89*	0.93*	4175	1782
Natural experiment sample ^{d,c}					
<i>Sex-specific wantedness only</i>					
Years schooling completed	1.00	-	0.91**	2115	-

Notes:

a. See text for definitions of wantedness measures

b. Incidence rate ratios from fixed effects Poisson regression

c. Other variables in regression are sex of child, age of child, age squared, whether child was born after 1987, birth order, whether child was the first live birth, mother's education, father's education whether household head was Muslim, thana, and whether the child was classified under the 'first birth' rule

d. Incidence rate ratios from Poisson regression

* p<.05; ** p<.01;*** p<.001