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Selection bias in the link between in child wantedness and child survival: Theory and data from Matlab, Bangladesh

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Abstract

We examine the potential effects of selection bias on the association between unwanted births and child mortality from 7942 women from Matlab, Bangladesh who declared birth intentions in 1990 prior to conceiving pregnancies. We explore and test two opposing reasons for bias in the distribution of observed births: 1) Some women who report not wanting more children could face starvation or frailty and if these women are sub-fecund, the remaining unwanted births would appear more healthy; 2) Some women who report not wanting more children could have social privileges in acquiring medical services, abortion, and contraception and if these women avoid births the remaining unwanted births would appear less healthy.

We find: A)No overall effect of unwantedness on child survival in rural Bangladesh in the 1990s; B)No evidence that biological processes are spuriously making the birth cohort look more healthy; and C)Some evidence that higher schooling in women who avoid unwanted births is biasing the observed sample to make unwanted births look less healthy. Efforts to understand the effect of unwantedness in datasets that do not control for complex patterns of selective birth may be misleading and require more cautious interpretation.

I. Introduction

Many have sought evidence to assess the contribution of family planning services to child health. If there is a link, then money spent to help people avoid unintended births becomes a public health investment relevant to child survival. Demographers classify unintended births into A) mistimed e.g. “another child wanted after two or more years” and B) unwanted e.g. “I/we do not want another child”. Efforts to demonstrate health effects of unwanted births face empirical challenges that we review in this paper. We focus on the role of selection bias related to unwanted pregnancies where family planning would be used to limit rather than space births.

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The conceptual model linking declarations of wantedness to child death would have to assume that there are some parents whose pre-pregnancy declaration that another child is not wanted is signifying the parent's recognition of unavoidable threats to that child's survival, for example, extreme poverty. Hence, the population of parents saying “We don't want more children” could be enriched with a high proportion of parents who truly face profound child survival challenges that would manifest as higher rates of child death if these potential children were born. A variety of proximal mechanisms for unwanted child death could include malnutrition, violence, genetic conditions--the common distal factor being that parents are able to foresee an unborn child's survival risk and base their birth intentions on their foresight.

There are also important effects of birth selection that could skew the sample of observable unwanted births and interfere with tests for a link between child wantedness and child survival. We develop a theory of birth selection to guide attempts to demonstrate an empirical connection between statements of wantedness and child survival. We then test the hypothesis that unwantedness lowers child survival, and examine the nature of potential selection bias using data on preferences collected prior to a pregnancy in a high mortality setting in Bangladesh.

II. Background

Significant numbers of unintended pregnancies and births have been reported in virtually all developed and developing country settings for which data are available. Data from the United States indicate that roughly one-half of all pregnancies were unintended at the time of conception, with mistimed more common than unwanted pregnancies (Finer & Henshaw, 2006; Forrest, 1994; Gazmararian et al., 1995; Korenman, Kaestner, & Joyce, 2002; Marsiglio & Mott, 1988; Pulley, Klerman, Tang, & Baker, 2002). Estimates of levels of unintended pregnancies in developing countries rely heavily on Demographic and Health Survey (DHS) data (Singh, Sedgh, & Hussain, 2010).

The literature on the health and social consequences of unintended childbearing is characterized by considerable variability in terms of sample representativeness, measurement, and methodological rigor (Brown & Eisenberg, 1995; Gipson, Koenig, & Hindin, 2008), with few studies that control adequately for socioeconomic status (SES). A major challenge confronting researchers has been obtaining data on birth intentions prior to conception and then observing all of the subsequent pregnancy outcomes. Studies based on post-pregnancy declarations of birth intentions can suffer from bias if some women revise their intentions based on the birth outcome. Revision of preferences may occur prior to a conception or later (Yeatman, Sennott, & Culpepper, 2013). Data show significant revision of birth intentions that occurs after a woman learns she is pregnant (Bankole & Westoff, 1998; Casterline, El-Zanaty, & El-Zeini, 2003; Joyce, Kaestner, & Korenman, 2002; Koenig, Acharya, Singh, & Roy, 2006). If a mother gets to decide whether a child is to be declared as wanted or unwanted after she observes the child's health, then her choice about what to say may be caused by the child's actual health. (Rosenzweig & Wolpin, 1993). It is possible that an originally wanted pregnancy could be recoded as unwanted after a mother observes that her child is sickly (Rosenzweig & Wolpin, 1993). In this situation, we cannot be sure that it is really the state of pre-birth unwantedness that is associated with health risk for the child. This distinction matters because only pre-birth unwantedness can create an opportunity for family planning to potentially cause improvements in the health of a birth cohort.

Much of the existing literature on effects of wantedness could be subject to recall bias, particularly since most studies rely on cross-sectional data and post-birth expressions of wantedness. For example, an analysis of DHS data from five countries found that in three countries (Egypt, the Philippines, and Thailand), babies born to women who recalled whether a pregnancy had been wanted had higher neonatal and post-neonatal mortality (Montgomery, Lloyd, Hewett, & Heuveline, 1997).

Cross sectional studies have found significant positive associations between recalled child wantedness and women's use of prenatal care (Eggleston, Tsui, & Kotelchuck, 2001; Fawcus, Crowther, Van Baelen, & Marumahoko, 1992; Magadi, Madise, & Rodrigues, 2000; Marston & Cleland, 2003), supervised delivery (Gage, 1998; Marston & Cleland, 2003) and full child vaccination coverage (Marston & Cleland, 2003). Others have reported a significant inverse association between recalled wantedness and the risk of low birthweight (Eggleston et al., 2001) and adequate nutritional status (Montgomery et al., 1997). The effects on children of being unwanted or unintended may extend well into childhood and even adulthood, with studies reporting significant inverse associations between pregnancy intention and child development (Baydar, 1995; Kubicka et al., 1995; Matejcek, Dytrych, & Schuller, 1978). Again, these studies rely on cross-sectional data and recalled birth intentions.

Other analyses attempt to use data on siblings who are discordant on wantedness to control for family-level factors. For example, prospective maternal surveys from rural Bangladesh (1982-1998), with a sibling fixed-effects model found large and significant estimates of excess mortality during infancy among children of unwanted pregnancies (Chalasani, Casterline, & Koenig, 2007). Sibling models control for factors about a household that are the same across all siblings, but they cannot control for selection bias due to factors that are specific to only one sibling (Lordan & Frijters, 2013). Differential selection into the pool of observed births can occur with each and every pregnancy. When siblings are discordant on wantedness, they are likely to be discordant on factors controlling their chances of ever being born. These unobservable selection factors would differ between a wanted and an unwanted sibling. For example, the mother of an unwanted sibling would have been much more interested in using family planning than the mother of a wanted sibling. Her level of interest and success in obtaining contraception will drive selection bias and this cannot be statistically controlled by studying fixed effects models of discordant siblings.

III. A Theory of the Link Between Unwantedness and Child Health

We model a dichotomous variable called "*Mother says 'Don't Want'*;" ($MSDW_{ijt}$) for the i-th mother prior to her j-th pregnancy at the t-th time as

1. $MSDW_{ijt}=1$ if $Pr(\text{Say "Don't Want"}|H_{ijt}, \tilde{H}_{ijt}, X_{ijt}) = f(H_{ijt}, H_{ijt}, X_{ijt})>K$ otherwise $MSDW_{ijt}=0$

where Pr(A|B) is the conditional probability operator assumed to reflect a latent cognitive process that is distributed along a continuum. H_{ijt} is a vector of observable health determinants that are positively associated with future child survival. $\tilde{H}_{ijt}$ is a vector of health determinants unobserved by the analyst, but perceived by the woman. Presumably the woman knows and updates private information based on prior birth outcomes for herself and her family. She also knows private information about her immediate economic horizon. X_{ijt} is a vector of factors that affect fertility preferences, but which have negligible bearing on child survival. K is a constant threshold value above which the latent process registers a dichotomous value of “Don't Want”.

Partial derivatives $f'(H)<0$ and $f'(\tilde{H})<0$, imply that increases in predicted child survival lower the probability of a saying “Don't Want” and conversely that worse predictions of child health increase the probability of saying “Don't Want”. This would lead one to test the following hypothesis:

[Hypothesis 1] E(H| Say “Don't want”) < E(H| Say “Yes want”)

Where E(A|B) is the conditional expectations operator.

Hypothesis 1 implies that observed health will be lower in the group of children whose mothers said “don't want” . Tests of hypothesis 1 face selection bias because the sample of observable children includes only those who were not aborted, not miscarried, and not stillborn. Prior to conception, all women see facts about their circumstances that they use to predict the health of their unborn children and the impact of pregnancy on their lives. We assume that the expectation of child frailty has a normal distribution and we represent this distribution in Figure 1 and note that according to Hypothesis 1 the expected child health for unwanted children is shifted to the left compared to wanted children.

The selection bias occurs because the pre-conception, frailty of a potential child is not independent of its probability of being born. We assume that there are women on the left tail of the “Don't want” distribution of frailty who are more likely to be the frailest. Processes such as stress amenorrhea and pre-natal starvation leading to infecundity or infections leading to miscarriage would be more common among the frailest women. This frailty suggests we should look for higher rates of miscarriage, stillbirth and infecundity among the women who say “Don't want”. Miscarriages, stillbirths, and physiological subfecundity would remove more of these vulnerable births from the left tail of the left-shifted population of unwanted births than from the population of wanted births. This type of selection would make the unwanted births that do occur appear artificially healthier and make it artificially harder to confirm Hypothesis 1.

In contrast, on the right hand tail of the distribution in Figure 1 where women are least frail, we argue that there are a higher proportion of better off women who say “Don't want” and who have social advantages that make them more likely to successfully negotiate and to gain access to health services in the context of rural Bangladesh. They will obtain effective contraception, and access to abortions, and had they given birth, they would have made better use of services and practices that offer better health outcomes for their children. This suggests that would be higher social advantages among the women who say “Don't want” and then subsequently succeed in obtaining FP and/or abortion(DaVanzo, Rahman, Ahmed, & Razzaque, 2013). The differential uptake of FP and abortion, could lead to a spurious confirmation of hypothesis 1 by artificially eliminating the healthiest unwanted infants from the pool of observable unwanted births. The presence of selection bias on the right tail of the distribution means that it is possible for there to actually be no difference between the underlying health of unwanted and wanted potential births. Differential abortion and contraception on the basis of higher social position would, nevertheless, make the sample of unwanted observed births appear to be less healthy.

Both types of birth selection are plausible. They counteract each other. A priori, it is impossible to say which process will dominate. It is even possible for there to be socioeconomically resourceful women who are atypically frail and who consequently intensify their contraceptive efforts. This might occur in the setting of an HIV/AIDS epidemic, or if genetic screening becomes available in the private sector. In the setting of rural Bangladesh it is unlikely that "resourceful but unhealthy" cases would predominate.

Based on this model of the processes underlying child wantedness and the processes leading to births, one can see how challenging it would be to test whether being born to parents who say they don't want more children "causes" worse child health. Simple comparisons are not adequate, and simple multivariate adjustment for observable variables has been repeatedly shown to be an ineffective remedy for selection bias (Manski, 1989). To understand the direction of the bias, it would help to know more about all pregnancy outcomes including miscarriages, still births, and abortions. It would also help to know if effective contraception and abortion is more selectively practiced by women with higher education and social status or even by women who perceive that they are at high risk of a poor birth outcome.

IV. Empirical Methods

To demonstrate an approach to assessing the potential size and direction of selection bias, we analyze the connection between pre-conception statements of unwantedness and subsequent child survival. As discussed in the results section, our analysis showed no significant correlation between wantedness and child survival across a large array of model specifications. However, in the face of competing possibilities for selection bias this could still be a spurious finding. We illustrate how to illuminate the nature of selection by performing a subsequent analysis of the linkage from unwantedness to stillbirths, miscarriages, and prior child demise to see whether selection bias is removing the more frail children from the left tail of the observable sample of unwanted children. We also examine the impact of mother's schooling on access to modern family planning and abortion to see whether selection bias is removing less frail children from the right tail of the observable sample of unwanted children. These subsequent analyses help us evaluate whether the null findings from the main analysis can be attributed to selection bias or not. They illustrate one potential way to make progress in the face of a selective sample of observable unwanted births.

Data

Our longitudinal data come from Matlab, Bangladesh. In 1975, the International Centre for Diarrhoeal Research, Bangladesh (ICDDR,B) began an experiment in which 70 villages (Pop. 89,350) were designated as a treatment area and 79 villages (Pop. 85,596) were designated as a comparison area. Individuals in the treatment area received door-to-door biweekly visitation by community health workers promoting family planning and health services. The comparison area continued receiving standard government services.

The Knowledge Attitudes and Practices (KAP90) survey that supplied data on pregnancy wantedness was conducted in 1990 among 7,946 reproductive age women in both the Matlab intervention and comparison areas. Multi-stage sampling procedures were employed to yield a sampling frame of approximately 8,500 currently married women of reproductive age. This resulted in the random selection of 31 and 36 villages in the intervention and comparison areas, respectively. Ongoing surveillance information was used to enumerate all eligible women in selected villages, with every alternate woman chosen for interview. Interview completion rates were high in both areas, exceeding 90%.

The survey covered a range of topics related to contraceptive and health behavior, household socio-demographic characteristics, as well as respondents' perceived access to and quality of care. Also included were detailed questions concerning women's preferences for additional childbearing. Women in the KAP90 survey were asked whether they wanted additional sons or daughters. They were also asked if they would be happy, unhappy, or indifferent if they had an additional son, and if they would be happy, unhappy, or indifferent if they had an additional daughter.

Data on births and deaths came from the Demographic Surveillance System (DSS) which is collected annually in Matlab. Data on pregnancies came from the Record Keeping System (RKS) that is updated whenever community health workers learn of a pregnancy RKS data were merged to the DSS and to the KAP90 data.

Although a total of 7,946 women completed the survey, 885 said they were currently pregnant and their data are excluded from our analysis leaving a total analytical sample of 7,059. There were a total of 5,860 subsequent pregnancies occurring after the KAP90 survey whose resolution could be determined and coded as livebirth, miscarriage, stillbirth, or abortion. In the Bangladeshi health system a procedure called “menstrual regulation” or MR by means of vacuum aspiration is condoned up to 10 weeks since the last menstrual period provided there is no positive pregnancy test that would reclassify the procedure as an abortion (Dixon-Mueller, 1988). The record keeping system (RKS) of ICDDR,B would use a combination of woman's report of a missed period, symptoms of pregnancy, and health care provider's diagnosis of pregnancy to register a pregnancy. Once registered, the village health worker would code a pregnancy's result as “miscarriage-induced”, “miscarriage-spontaneous”, “stillbirth”, or “live”. Our analysis codes “miscarriage-induced” as abortions even though officially the Bangladeshi clinicians call these events “menstrual regulation”. Very early miscarriages and some abortions may not have been reported.

There were 5,041 live births resulting from these 5,860 pregnancies, and the child's survival status to age 3 could be determined for 4,270 (85%) of them. Missing observations of an independent variable were imputed using a regression on women's age, parents' schooling, household size and study area. Models with non-imputed data were also examined to check whether imputation was driving the results.

We created three different measures of wantedness: A)”Don't' want”; B) “Unhappy any”, and C) Don't want and unhappy any. “Don't want” was coded as a dummy variable equaling one if the child was born up to 7 years after the date of the survey and a non-pregnant mother had replied “None” to both “What is your desire for additional sons?” and “What is your desire for additional daughters?” . Women were asked, “How would you feel about an additional son/daughter?” as “Unhappy”, “Indifferent”, or “Happy”. If they responded as “Unhappy”, they were coded as “Unhappy any”. Non-numerical responses were lumped together with wanting additional pregnancies. Later the results were compared when non-numerical responses were coded as missing.

Extending the inclusion window out to 7 years has the advantage of offering a larger sample size, but the disadvantage that 7 years after their birth intentions were registered, some women may have altered this decision, weakening the accuracy of our data on wantedness. To assess the impact of trading accuracy for sample size, we conducted sensitivity analyses that tested successively smaller windows of inclusion with smaller samples from 7 through 1 year after the KAP90 interview. The vital status of each birth was ascertained every 10-12 months as part of ICDDR,B's routine demographic surveillance in the DSS of Matlab. The final vital status data on children for this study were obtained as of January 1, 2000. Data on childhood mortality are classified by the timing of the death for survival analysis and dichotomously as a variable equal to 1 for all children who died before their third birthday for logistic regression analysis.

The asset score data came from a socioeconomic census in Matlab in 1996 as did updated data on parental schooling attainment. Asset scores were used as quintiles derived from the unweighted summation of items owned out of a standard asset list that ICDDR,B administered throughout the study area in the census in 1996 (Razzaque, Streatfield, & Gwatkin, 2007).

Analysis

We plotted Kaplan-Meier curves and estimated Cox survival models for the number of years decedent children survived. We also used logistic regression of the death of a child at any age prior to three years against mothers' pre-pregnancy declaration of wantedness and the covariates shown in Table 3. Robust standard errors were estimated because some women contributed more than one birth to the sample between 1990 and 1997.

To assess for an association between saying “Don't want” more children and birth selection effects potentially leading to a healthier sample of births, we estimated logistic regression models of having a pregnancy end in miscarriage or stillbirth, as a function of women's prior statements of pregnancy wantedness and control variables. To test for an association between saying “Don't want” more children and non-contribution of more advantaged children to the sample of live-born children we estimated logistic regression models of ever having an abortion, and of ever using modern family planning methods defined as sterilization, condoms, foam, IUD or hormonal methods. We specifically examined whether higher woman's schooling is associated with higher rates of use of modern family planning and abortion for women who say they don't want to have another child.

V. Results

Table 1 compares the characteristics of non-pregnant women stratified on the basis of whether they desired additional children at the 1990 survey. The populations are very different with higher parity, higher age, and lower schooling as well as higher rates of family planning use among women who say they don't want more children. We find that 30% of women who said they wanted more children were concurrently using family planning. A separate analysis (not shown) showed that of these 958 women wanting more children and using family planning, 889 (93%) said they want their next child to be born more than 2 years from now and 69 said they want their next child 1 year from now so these women can be largely presumed to be practicing birth spacing. Women who wanted more children had lower parity than women who did not want children. Gender preference contributed to fertility intentions--a separate analysis (not shown in the table) indicated that 94% of women who had at least one daughter but no son wanted more children and symmetrically 80% of women who had at least one son but no daughter wanted more children (p=0.000).

Among the 3,859 women who said they did not want more children in 1990 there were 1,470 subsequent pregnancies and 1,121 live births prior to the end of 1997 (Table 1). The ratio of reported stillbirths per unwanted pregnancy was statistically significantly higher than that for wanted pregnancies (4% versus 3 %, p=0.017). The ratio of miscarriages per unwanted pregnancy was statistically significantly higher than that for wanted pregnancies (7% versus 5% p=0.042). These higher unadjusted rates of miscarriages and stillbirths might signify that the sample of women who do not want to get pregnant is slightly more prone to worse pregnancy outcomes than those women who do desire future children. However, multivariate models (Table 5) showed that the adjusted incidence of miscarriages and stillbirths does not significantly vary across wantedness status. Multivariate models thus appear to control for the type of selection bias that removes less healthy children from the observed sample among women who don't want future children.

The ratio of abortions to unwanted pregnancies was also significantly higher than that for wanted pregnancies (13% versus 3%, p=0.000). This result remained significant in multivariate models of the relationship between abortion and wanting no more children (Odds ratio=3.697, p=0.000, Table 5). Separate analysis (not shown in tables) revealed that average schooling was higher for the 191 women with unwanted pregnancies who aborted vs. those with unwanted pregnancies who did not abort (2.4 years versus 1.7 years, p=0.002). Separate analysis also showed that asset scores and counts of living children in the household were not statistically significantly different between women who aborted unwanted pregnancies and those who did not. The mean years of schooling of women who wanted more children was 2.04 (SD: 2.78) in the full sample and 2.00 (SD:2.76) in the sample of women who did not have abortions, showing that schooling was only slightly lower in the observable sample of births by women who wanted more children. In contrast, overall mean schooling of women who said “Don't want” more children was 1.76 years (SD: 2.67), but reduced to 1.67 years (SD:2.58) in the women who did not have abortions. This reduction in mean years of schooling is an artifact of abortion-based birth selection, making schooling levels lower in the sample of observable unwanted births compared to the full sample. This artifactual effect is more pronounced in the sample of women who don't want pregnancies, because they are more likely to have abortions. The extent to which selective access to abortion skews other unobservable aspects of socioeconomic status besides education is unknown.

Table 2 shows that in a simple comparison of means there was a negative, but not statistically significant association between having a mother declare unwantedness prior to conception and child death (p=0.13). Children who died had larger household size, and lower maternal schooling attainments. The asset quintiles had no statistically significant association with child death. Overall, the percent who died in the treatment area was statistically significantly lower than the percent who died in the comparison area (8.07% versus 10.1%, p=0.01).

Table 3 reinforces the observation that unwantedness did not have a statistically significant relationship with child survival whether we use multivariate logistic regression or Cox proportional hazard estimation and whether or not an interaction between unwantedness and mother's schooling is used. Multiple other model specifications were estimated and none of these offered a significant coefficient on unwantedness except for the unadjusted model shown in column 3 of Table 3. These alternative models included models with and without controls for each of the following variables: household size, number of surviving children, asset score and distance to primary school and models where missing variables had not been imputed.

Multiple alternative ways to code a child's wantedness were all statistically insignificant predictors of child death. There was no significant effect on survival when unwantedness was coded in its most stringent form requiring that mothers say they do not want additional sons or daughters and that they say they would be unhappy if they had additional sons or daughters. There was no effect of unwantedness in its loosest form as saying either “Don't want” or “Unhappy any”. There was no significant effect on survival when analysis excluded all women who gave non-numerical answers like “Up to God” in response to the number of additional sons or daughters desired.

The Kaplan-Meier survival curve (Figure 2) shows that any possible difference in survival between wanted and unwanted children could be occurring after 300 days of life, but these differences are not statistically significant. The first part of the survival curves overlap for children of wanted and unwanted pregnancies. The multivariate Cox proportional hazards analysis confirmed the multivariate logistic finding that wantedness had no statistically significant effect on survival.

Table 4 examines whether the results seen in Table 3 are an artifact of liberal inclusion criteria that allowed children to be coded as born from unwanted pregnancies up to 7 years after their non-pregnant mothers were interviewed about whether they wanted more children. Table 4 varies the time window from 7 years down to 1 year after the KAP90 interview and shows that none of the multivariate models of child death yielded a significant odds ratio on infant wantedness.

The first 3 columns of Table 5 examine whether women who say “Don't Want” are more likely to have non-birth pregnancy resolutions like miscarriage or stillbirth or to have a higher probability of child death. Although there are significant bivariate associations (top row); these become non-significant when controlling for confounders. All of the various models shown in Table 3 were also examined for this analysis and none of the multivariate models showed a significant effect of wantedness on subsequent miscarriages and stillbirths, or child death prior to the interview.

The final two columns of Table 5 show that women who say they don't want more children are more likely to have an induced abortion or to use a modern family planning method. The results are seen in both bivariate and multivariate models indicating that children from mothers who don't want children are more likely to be actively doing something about it and their births will be more frequently censored out of the analysis. The significant coefficient on schooling in the final two columns suggests that this particular observable social advantage is correlated with seeking abortion and using modern family planning methods.

VI. Discussion

Contrary to our expectations, we found no relationship between a woman saying she did not want any more children and the risk of death of children who were conceived and born after that intention was expressed in 1990. None of the multivariate logistic models disclosed an effect of unwantedness on survival. Multivariate Cox survival models also failed to find a robust relationship between being unwanted and the risk of child death.

Our theory of selection suggests that null findings like this could occur due to biological censoring of children with lower health endowments—making it appear that unwanted children who were liveborn were healthier than they would have been in the absence of selection. We tested for this by modeling the odds of miscarriage, stillbirth, and prior child death as a function of unwantedness, but we found no convincing evidence that higher risk pregnancies were differentially being censored out of the sample of observable unwanted children. To the contrary, we found evidence that censoring processes were actually making it easier to find spurious harmful health effects of unwantedness, because socioeconomic processes were removing healthier children from the observable pool of unwanted children through differential access to abortion and family planning among women with more schooling.

Given the unique strengths of ICDDR,B's record keeping system which coded every detectable pregnancy and its outcome, we were able to test whether biological censoring could be a reason for the null finding on the effects of wantedness. Rates of stillbirth and spontaneous abortion were higher among women who said they did not want more children in bivariate analysis, but in multivariate analysis, these results became non-significant. The lack of association between not wanting additional pregnancies and prior child death makes it unlikely that high risk women were saying they didn't want to get pregnant and then trying harder than average to achieve contraceptive success. It appears from Table 5 that simple multivariate control variables eliminated the effects of biological selection bias.

Our analysis showed that selection was occurring--more educated women were better able to realize their goal of not having unwanted children. The sample of observable unwanted births had lower maternal schooling levels than the full sample of women who said they did not want to become pregnant, and may have had lower levels of other unobservable social advantages. We did not find effects of unwantedness on survival and we found that socioeconomic selection bias was actually making it more likely to find spurious effects of wantedness on child health.

The dueling selection biases we have discussed will make it very challenging to produce an unbiased estimate of the effect of wantedness on a child's survival. Without analyzing differential rates of conception, abortion, miscarriage, and stillbirth in a population one cannot assess the relative strength of biological selection and social selection in biasing observed correlations. By examining the relationship between wantedness, social status, and non-birth pregnancy resolutions as illustrated in this paper, one can shed light on the direction of the selection bias. We hope that future investigations into the effects of unwantedness use available data on non-birth pregnancy resolutions and family planning to quantify the directions of selection bias when claiming to find effects of wantedness on health.

Our data set had several important strengths in terms of size, prospective measurement of abortion rates, family planning use and pregnancy outcomes other than live birth, long follow up and ante-natal registration of whether a woman wanted more children. Prior studies that attempted to measure effects of unwantedness on child health outcomes did not have these advantages and may have been unable to assess the relative strengths of the dueling selection biases. Findings from this study may not generalize to other settings where child survival, access to abortion, and preferences for childbearing vary from those in Bangladesh.

Given the importance of selection bias in attenuating or accentuating an observed correlation between unwantedness and child survival, what should be done? We do not believe studying siblings who are discordant on wantedness offers an adequate solution. The birth of the unwanted member of a sibling pair would still occur due to unobservable advantages and disadvantages that apply only to the unwanted child and not to the wanted sibling. Attributing the unwanted sibling's worse health only to whether they were wanted, would overstate effects if women with hidden social advantages are more able to access abortion and family planning to avoid unwanted births.

Quasi-experiments or natural experiments that randomly altered the likelihood that an unwanted child would be born, could, in theory, eliminate the selection bias. In the case of the Matlab data, the quasi-experiment would not work because the treatment area received both intensified family planning and intensified child survival interventions so one could not use the treatment as an instrument for unwantedness. Treatment area residence would have independent survival advantages for children. Furthermore we found that even in the treatment area, rates of socioeconomic selection into abortion and family planning use by families with higher levels of husband's schooling were not attenuated relative to the comparison area. In other words, the treatment did not reduce the importance of the household's socioeconomic advantage to access services. The inability to use Matlab as a quasi-experiment for wantedness is disappointing because the Matlab area is one of the world's largest populations with a history of quasi-experimentally delivered family planning services.

The prior studies that succeed best in eliminating selection bias would be the ones that followed outcomes for children of women who were denied abortions due to factors outside the women's control. Here all of the observable children of unwanted pregnancies share a homogeneous selection process, because all mothers sought abortions. The advantage of the abortion-denied studies is offset by their lack of representativeness. Women who seek and are denied abortion are a non-representative sub-sample of all women who have unwanted births (Forssman and Thuwe 1981; Kubicka, Matejcek et al. 1995; Myhrman, Olsen et al. 1995; Pop-Eleches 2006). These results can inform policies concerned with abortion availability, but can not be extrapolated to a broader population of women expressing fertility preferences.

We believe that the absence of any survival disadvantage from being born unwanted in our analysis indicates that the heightened availability of family planning and abortion for women who wanted to limit their fertility played an insignificant role in improving child survival in the Matlab area of Bangladesh in the 1990s. However it very likely did improve child survival through birth spacing (DaVanzo, Hale, Razzaque, & Rahman, 2008) and it saved mother's lives because women who prevented an unwanted pregnancy could not die from maternal causes (Diamond-Smith & Potts, 2011). The lives of families who were able to realize their intention to limit fertility would have been better in many other ways, besides child survival. Benefits to mothers and benefits to children other than survival were not explored in this paper.

Given our results, it would be short-sighted to build a case for supporting family planning solely on the aspiration that there will be better child survival from reducing the number of unwanted children. Selection bias will make it difficult to conclusively demonstrate that unwanted children have worse survival than wanted children. Because child survival rates are improving around the world due to better public health environments, the potential impact of more family planning on child survival will diminish over time. Ultimately societies will need to sustain their basic interest in helping families control reproduction even after child survival goals are achieved. Basing concern for reproductive control on regard for human dignity and the freedom to determine a life course is ultimately more sustainable.

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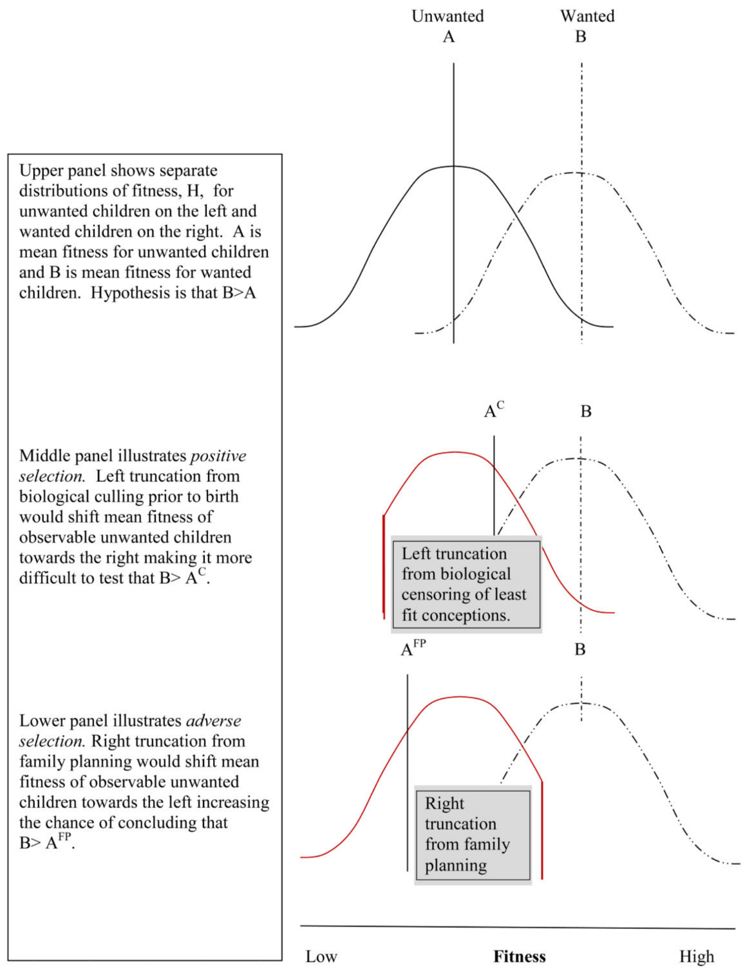

Figure 1. Competing effects of birth selection on hypothesis testing

Upper panel shows separate distributions of fitness, H, for unwanted children on the left and wanted children on the right. A is mean fitness for unwanted children and B is mean fitness for wanted children. Hypothesis is that B>A Middle panel illustrates *positive selection.* Left truncation from biological culling prior to birth would shift mean fitness of observable unwanted children towards the right making it more difficult to test that B> A^C. Lower panel illustrates *adverse selection.* Right truncation from family planning would shift mean fitness of observable unwanted children towards the left increasing the chance of concluding that B> A^{FP}.

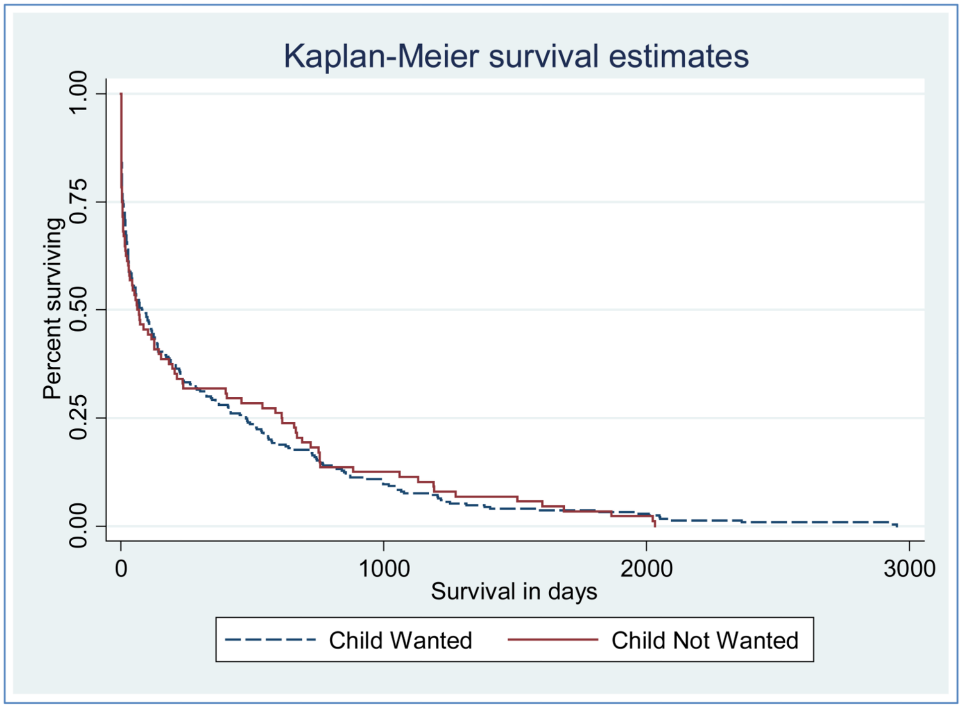

Figure 2. Unadjusted survival curves for children from unwanted and wanted pregnancies in Matlab 1990-2000. Hazard ratio is 0.997 with standard error of 0.124

Table 1

Sample description of 7059 non-pregnant women surveyed in Matlab, Bangladesh stratified by stated birth intentions in 1990

		Non-Pregnant Woman Says "Don't Want" in 1990			Non-Pregnant Woman Says "Want More" in 1990			
	Source	**Numerator/ Denominator**	**Mean or Proportion**	**SD**	**N**	**Mean or Proportion**	**SD**	
		Denominator is 3859 women			*Denominator is 3200 women*			P-value
Current Family Planning User	KAP90	2067/3859	54%		958/3200	30%		0.000
Number of living sons	KAP90	3859	2.5	(1.2)	3200	0.8	(0.9)	0.000
Number of living daughters	KAP90	3859	2.1	(1.3)	3200	0.9	(1.1)	0.000
Total living children	KAP90	3859	4.6	(1.7)	3200	1.7	(1.3)	0.000
Age of woman	KAP90	3859	36.4	(6.9)	3200	25.5	(5.3)	0.000
Years of schooling	KAP90	3859	1.5	(2.5)	3200	2.1	(2.9)	0.000
Asset Score	Census96	3538	3.1	(1.4)	2915	3.0	(1.4)	0.006
Fraction of women with susbsequent pregnancies	RKS	1027/3859	27%		2514/3200	79%		0.000
Fraction of women with subsequent live births	RKS	874/3859	23%		2433/3200	76%		0.000
Fraction of women with subsequent stillbirths	RKS	56/3859	1%		2514/3200	3%		0.000
Fraction of women with subsequent miscarriages	RKS	91/3859	2%		206/3200	6%		0.000
Fraction of women with subsequent abortions	RKS	162/3859	4%		107/3200	3%		0.024
		Denominator is 1470 pregnancies			*Denominator is 4390 pregnancies*			
Fraction of pregnancies with live birth	RKS	1121/**1470**	76%		3920/**4390**	89%		0.000
Fraction of pregnancies with still birth	RKS	58/**1470**	4%		119/**4390**	3%		0.061
Fraction of pregnancies with miscarriage	RKS	100/**1470**	7%		236/**4390**	5%		0.004
Fraction pregnancies with abortion	RKS	191/**1470**	13%		114/**4390**	3%		0.000

[1] All birth outcomes observed between 9 mos and 7 years after wantedness interview

Table 2

Bivariate tabulation of correlates of child survival. (N=4270 Children with Survival Data)

	Source	Child Survived to Age 3 Mean or N Proportion SD			Child Died Prior to Age 3 Mean or N Proportion SD			P-Value
Not pregnant and "don't want" additional son or daughter	KAP90	3917	23%		353	26%		0.128
Not pregnant and "unhappy" if additional son or daughter	KAP90	3917	22%		353	23%		0.828
Child is girl	DSS	3917	49%		353	52%		0.248
Years of schooling of mother	Census96	3710	1.95	(2.80)	331	1.43	(2.41)	0.001 ***
Years of schooling of father	Census96	3917	3.35	(4.25)	353	3.00	(3.75)	0.136
Household size	DSS	3917	8.66	(4.26)	353	10.32	(5.75)	0.000 ***
Proportion abortions/pregnancies in village over 11 years	RKS	3917	2.77%	(0.02)	353	2.95%	(0.02)	0.097
Average of FP worker responsiveness per village	KAP90	3917	3.39	(0.34)	353	3.38	(0.33)	0.473
Treatment Block A	DSS	3917	9%		353	7%		0.240
Treatment Block B	DSS	3917	19%		353	19%		0.902
Treatment Block C	DSS	3917	11%		353	8%		0.145
Treatment Block D	DSS	3917	10%		353	7%		0.104
Comparison Area North	DSS	3917	15%		353	17%		0.297
Comparison Area South	DSS	3917	36%		353	41%		0.078
Asset Score in 1996	Census96	3558	2.97	(1.38)	323	2.94	(1.40)	0.725
Poorest Asset Quintile	Census96	3558	19%		323	21%		0.352
2nd quintile	Census96	3558	22%		323	20%		0.472
3rd quintile	Census96	3558	20%		323	20%		0.961
4th quintile	Census96	3558	22%		323	22%		0.880
Richest Asset Quintile	Census96	3558	17%		323	17%		0.981

Data Sources: KAP90 is Knowledge Attitude Practice survey of 1990. DSS is demographic surveillance system. RKS is record keeping system.

Table 3

Multivariate models of odds ratios of child death

	Logistic regression of odds child died before age 3			Cox regression of hazard of child death		
Mother said don't want more children	0.823 [-1.204]	0.911 [-0.556]	1.213 [1.648]	0.896 [-0.865]	0.966 [-0.231]	0.997 [-0.0255]
Don't want x Years of schooling of mother	1.076 [1.361]			1.071 [0.969]		
Mothers age at child's birth	0.997 [-0.229]	0.997 [-0.229]		0.998 [-0.116]	1.000 [-0.00146]	
Years of schooling of mother	0.907** [-3.687]	0.922** [-2.771]		0.955 [-1.191]	0.970 [-0.826]	
Years of schooling of father	1.005 [0.316]	1.007 [0.395]		1.038 [1.706]	1.041 [1.828]	
Asset Score	1.001 [0.0295]	1.000 [-0.0118]		0.978 [-0.527]	0.979 [-0.522]	
Distance to primary school	1.000 [-0.400]	1.000 [-0.371]		1.000 [-0.0150]	1.000 [0.00654]	
Household size	0.995 [-0.445]	0.995 [-0.438]		1.001 [0.0871]	1.001 [0.0650]	
Total living children	1.103 [1.776]	1.102 [1.744]		1.006 [0.0986]	1.004 [0.0721]	
Proportion girls of living children	1.331 [1.468]	1.337 [1.483]		0.895 [-0.635]	0.912 [-0.519]	
Constant	0.0746** [-6.862]	0.0725** [-6.889]	0.0770** [-32.06]			
Observations	4,270	4,270	4,270	338	338	338
Models include dummies for six separate study areas						

Robust z-statistics in brackets adjust for heteroskedasticity of each woman.

** $p<.05$

Table 4

Sensitivity of logistic regression results on child death to seven different choices of eligibility for inclusion. Longer windows of time include larger sample sizes, but may be miscoded for women who change birth intentions. Shorter windows are less prone to miscoded birth intentions, but lower sample size

Number of years after 1990 interview a child's birthday could occur. Children born after this were not included.	**Seven**	**Six**	**Five**	**Four**	**Three**	**Two**	**One**
LOGISTIC MODEL BIVARIATE RESULTS							
Non-pregnant woman says “Want no more” children in 1990	1.213 [1.648]	1.158 [1.325]	1.133 [1.027]	1.104 [0.783]	1.253 [1.711]	1.32 [1.654]	1.191 [0.499]
Observations	4,270	4,123	3,667	3,117	2,420	1,629	772
COX PROPORTIONAL HAZARDS BIVARIATE RESULTS							
Non-pregnant woman says “Want no more” children in 1990	0.997 [-0.0255]	0.979 [-0.183]	0.902 [-0.808]	0.862 [-1.159]	0.917 [-0.631]	0.824 [-1.316]	0.768 [-1.790]
Observations	338	331	308	268	213	155	77
LOGISTIC MODEL MULTIVARIATE RESULTS							
Non-pregnant woman says “Want no more” children in 1990	0.911 [-0.556]	0.911 [-0.551]	0.865 [-0.856]	0.793 [-1.369]	0.872 [-0.715]	0.905 [-0.372]	0.821 [-0.345]
Mother's age at birth	0.997 [-0.229]	0.995 [-0.301]	1.007 [0.493]	1.015 [0.873]	1.023 [1.103]	1.017 [0.601]	0.996 [-0.111]
Years of Schooling of Mother in Census 1996	0.928*** [-2.771]	0.920*** [-2.991]	0.909*** [-2.926]	0.894*** [-3.799]	0.886*** [-3.535]	0.860*** [-3.268]	0.875** [-2.525]
Observations	4,270	4,123	3,667	3,117	2,420	1,629	772
COX PROPORTIONAL HAZARDS MULTIVARIATE RESULTS							
Non-pregnant woman says “Want no more” children in 1990	0.966 [-0.231]	0.960 [-0.270]	0.911 [-0.574]	0.846 [-1.109]	0.928 [-0.405]	0.828 [-0.832]	0.875 [-0.420]
Mother's age at birth	1.000 [-0.00146]	1.003 [0.164]	1.006 [0.364]	0.995 [-0.342]	0.991 [-0.486]	0.980 [-0.761]	0.980 [-0.585]
Years of Schooling of Mother in Census 1996	0.970 [-0.826]	0.967 [-0.873]	0.978 [-0.455]	0.941 [-1.290]	0.922 [-1.399]	0.911 [-1.382]	0.852 [-1.586]
Observations	338	331	308	268	213	155	77
Multivariate models include husband's schooling, distance to primary school, and treatment area							

Robust z-statistics in brackets adjust for heteroskedasticity of each woman.

** $p<0.05$,

$^{***}p<0.01$

Table 5

Determinants of censoring of children with either lower or higher health endowments

	Tests for Biological Censoring of Children with Lower Health Endowments			Tests for Socioeconomic Censoring of Children with Higher Health Endowment	
	Miscarry	**Stillbirth**	**Prior Siblings Died**	**Abortion**	**Uses Modern FP Method**
Bivariate results					
Non-pregnant woman says "Want no more" children in 1990	1.327 [1.751]	1.522*** [2.610]	2.070*** [8.323]	6.137*** [11.78]	1.254** [2.478]
Observations	4,901	4,901	4,901	4,901	4,901
Multivariate results					
Non-pregnant woman says "Want no more" children in 1990	1.052 [0.287]	1.147 [0.659]	0.852 [-1.486]	3.697*** [7.097]	1.478*** [3.116]
Mother's age at birth	1.036*** [2.938]	1.033** [2.359]	1.139*** [14.61]	1.087*** [7.405]	1.012 [1.325]
Years of Schooling of Mother in Census 1996	0.996 [-0.121]	0.986 [-0.294]	0.946** [-2.500]	1.090** [2.365]	1.038 [1.767]
Years of Schooling of KAP Husband	1.017 [0.658]	0.963 [-1.240]	0.966** [-2.451]	1.069*** [2.968]	0.998 [-0.127]
Asset Score in 1996	1.013 [0.255]	1.072 [1.135]	1.006 [0.193]	0.971 [-0.544]	0.980 [-0.567]
Distance to Primary School in Meters	1.000 [0.880]	0.999 [-1.529]	1.000 [0.895]	1.000 [-0.994]	1.000 [-0.418]
ICDDR, B TreatmentArea Dummy Variable 2	1.003 [0.0126]	1.396 [0.827]	1.258 [1.861]	0.809 [-0.772]	0.911 [-0.571]
ICDDR, B Treatment Area Dummy Variable 3	1.894*** [2.819]	2.111 [1.959]	0.772 [-1.647]	1.040 [0.109]	0.960 [-0.230]
ICDDR, B Treatment Area Dummy Variable 4	1.265 [0.634]	1.890 [1.537]	1.320*** [2.586]	0.700 [-0.862]	1.020 [0.103]
ICDDR,B Comparison Area Dummy Variable 5	1.446 [1.730]	3.535*** [3.598]	2.234*** [6.624]	3.295*** [4.865]	0.198*** [-7.908]
ICDDR, B Comparison Area Dummy Variable 6	1.263 [1.139]	1.568 [1.278]	1.904*** [5.737]	1.215 [0.984]	0.130*** [-11.44]
Constant	0.0119*** [-11.79]	0.00701*** [-9.211]	0.00961*** [-15.00]	0.00104*** [-13.95]	0.57 [-1.734]

	Tests for Biological Censoring of Children with Lower Health Endowments			Tests for Socioeconomic Censoring of Children with Higher Health Endowment	
	Miscarry	**Stillbirth**	**Prior Siblings Died**	**Abortion**	**Uses Modern FP Method**
Observations	4,213	4,213	4,213	4,213	4,213

Robust z-statistics in brackets adjust for heteroskedasticity of each woman.

** $p<0.05$,

*** $p<0.01$